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THE
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AND
JOURNAL OF PHYSICAL SCIENCE.

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A Journal of Practical Chemistry

IN ALL ITS APPLICATIONS TO
PHARMACY, ARTS, AND MANUFACTURES.

EDITED BY
WILLIAM CROOKES, F.R.S., &c.

VOLUME XLI.—1880.

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THE
CHEMICAL NEWS
AND
JOURNAL OF PHYSICAL SCIENCE

Edited by
Wm. Crookes, F.R.S.]

(WITH WHICH IS INCORPORATED THE "CHEMICAL GAZETTE")

Established
Thirty-Eight Years.

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CONTENTS.

ARTICLES:—	PAGE
On a New Hydride of Silicium, by M. J. Ogier	1
Answer to the Note by Dr. Phipson called "New and Old Views on the Nascent State of Bodies," by Dr. Tommasi	1
The Periodic Law of the Chemical Elements, by D. Mendeleef..	2
Experiments on the Contamination of Atmospheric Air with the Vapours arising from Arsenical Pigments, by W. Foster, M.A.	3
The Supposed Artificial Production of the Diamond.....	4
NEWCASTLE CHEMICAL SOCIETY.—Steel from Cleveland Iron—Notes on Magnesian Limestone Bricks	5
NOTICES OF BOOKS.—The Sizing of Cotton Goods—Sizing and Mildew in Cotton Goods—Annals of Chemical Medicine.....	9
CORRESPONDENCE.—Water of the Carisbrooke Castle Well.....	10
CHEMICAL NOTICES FROM FOREIGN SOURCES	10
MEETINGS FOR THE WEEK.....	10

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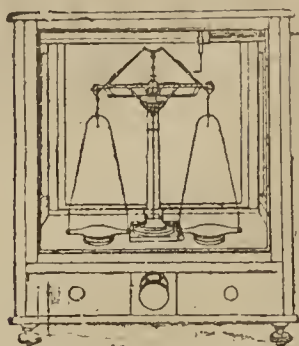
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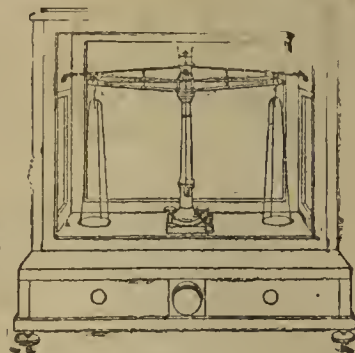
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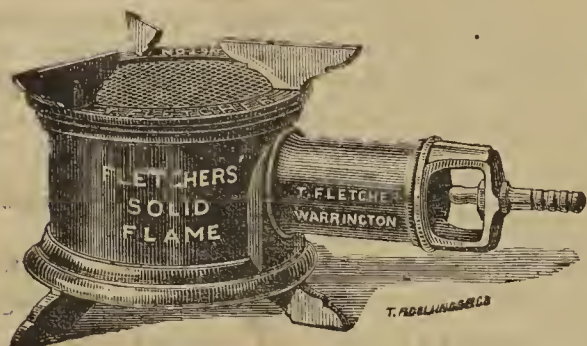
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INDEX NUMBER, CONTAINING TITLE-PAGE, INDEX, &c., TO VOLUME XL.

CONTENTS.

ARTICLES:—	PAGE
The Crystallisation of Carbon	13
On the Reduction of Auric Chloride by Hydrogen in Presence of Platinum, by Dr. T. L. Phipson, F.C.S., &c.	13
On the Noxious Action of Acid Vapours on Vegetation, by Dr. G. Lunge	14
Determination of Cobalt and Nickel, by E. Donath	15
On the Estimation of the Nitrous Compounds in the Various Stages of the Manufacture of Sulphuric Acid, by James Maclear, F.C.S., F.I.C.	16
Estimation of the Total Carbon in Iron and Steel <i>en masse</i> , by Sydney C. Jutsum	17
NOTICES OF BOOKS.	19
CORRESPONDENCE.—Oxidising Value of Chlorate and of Bichromate of Potash—The Blue Flame in Coal Fires	21
CHEMICAL NOTICES FROM FOREIGN SOURCES	22

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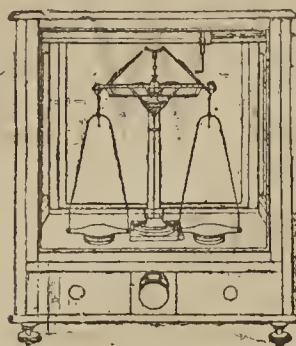
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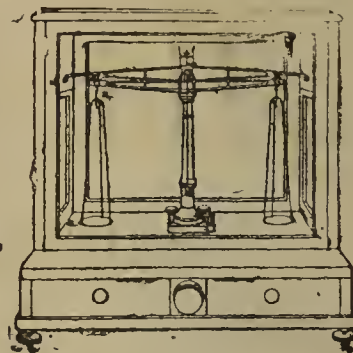


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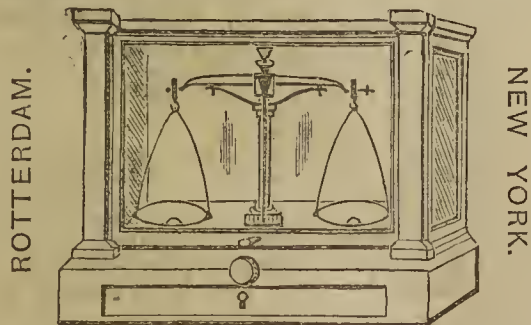
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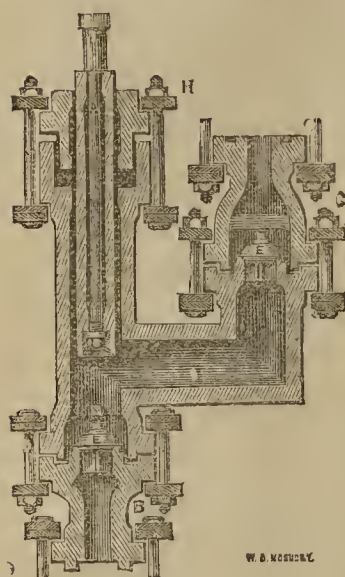
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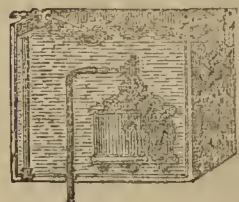
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	27
On the Separation and Estimation of Cadmium in the Presence of Zinc: with Remarks upon the Separation of Copper, Cadmium, and Zinc, by C. C. Hutchinson, Assoc. R.C.Sc.I.....	28
On the Detection of Organic Matter in Water, by F. Tiemann and C. Preusse	30
Determination of Chromium and Tungsten in Steel and in Iron Alloys, by Rudolf Schöffel	31
Volumetric Determination of Cerium, by Prof. Franz Stolba.....	31
NEWCASTLE CHEMICAL SOCIETY.....	32
CORRESPONDENCE.—Sizing of Cotton Goods—The Petroleum Act, 1879	34
CHEMICAL NOTICES FROM FOREIGN SOURCES	34

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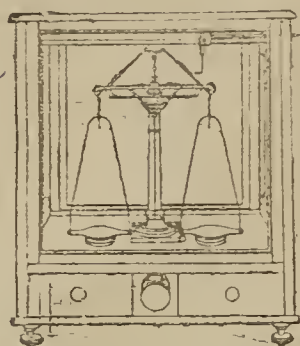
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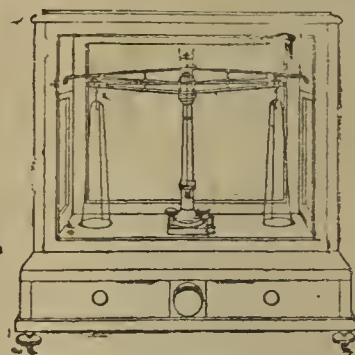


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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	39
On Chemical Repulsion, by Edmund J. Mills, D.Sc., F.R.S.....	40
On the Heating Power of Coal-gas of Different Qualities, by Dr. W. Wallace.....	41
Determination of Silver in Galena, by C. Balling	42
Detection and Determination of Nitrous Acid in Drinking-water, Acids, &c., by A. R. Leeds.....	42
Composition and Quality of the Metropolitan Water	42
On the Estimation of Nitrous Compounds in the the Various Stages of the Manufacture of Sulphuric Acid, by James Maclear, F.C.S., F.I.C.....	43
CHEMICAL SOCIETY	44
SOCIETY OF PUBLIC ANALYSTS.....	46
NOTICES OF BOOKS.—Year-book of Pharmacy.....	46
CORRESPONDENCE.—Blue Flame in Coal Fires—The Testing of Vitriol-Chamber Exits.....	47
CHEMICAL NOTICES FROM FOREIGN SOURCES	47

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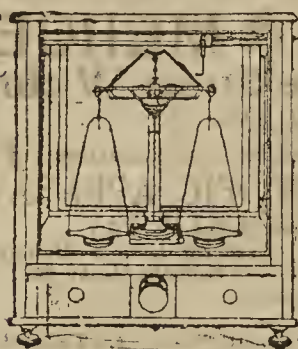
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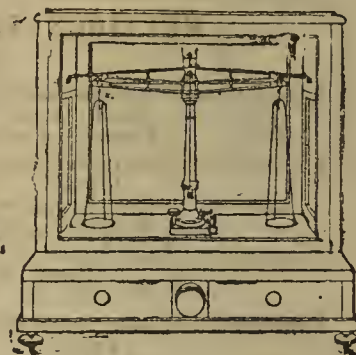


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MONDAY, 26th.—London Institution, 5.
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— Royal Geographical, 8.30.
TUESDAY, 27th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
— Civil Engineers, 8.
— Anthropological Institute, 8.
WEDNESDAY, 28th.—Society of Arts, 8. Ordinary Meeting.—"The Future of Ep ping Forest," by William Paul, F.L.S.
THURSDAY 29th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
— Royal, 8.30.
— London Institution, 7.
FRIDAY, 30th.—Royal Institution, 8. Mr. J. Marshall, "Proportions of the Human Figure," 9.
— Society of Arts, 8. Indian Section.—"Herat," by Colonel G. B. Malleson, C.S.I.
SATURDAY, 31st.—Royal Institution, 3. Prof. T. Rupert Jones, "Coal."

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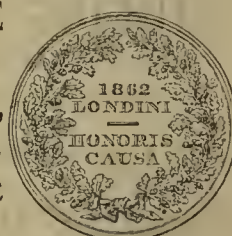
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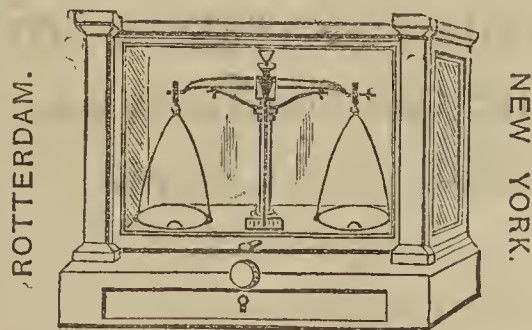
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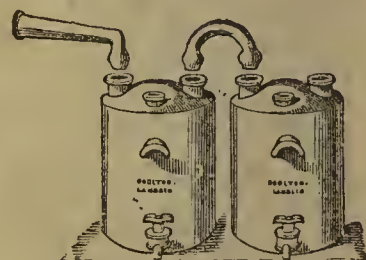
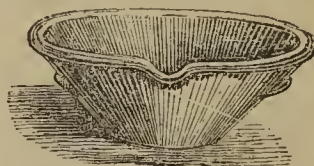
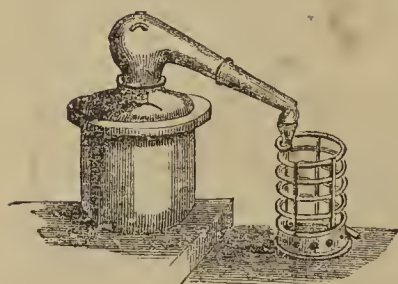
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	49
Report to the Local Government Board as to the Treatment of Sewage, by Dr. R. Angus Smith, F.R.S.....	50
On the Use of the Spectroscope in Discriminating Anthracens, by Benj. Nickels, F.C.S., F.I.C.....	52
On the Estimation of Nitrous Compounds in the Various Stages of the Manufacture of Sulphuric Acid, by James Maclear, F.C.S., F.I.C.....	52
Ancient Methods of Filtration, by H. Carrington Bolton, Ph.D...	54
Sale of Unfermented Wine.....	56
PHYSICAL SOCIETY.—Induction in Telephonic Circuits—Inter- mittent Currents and the Theory of the Induction Balance....	56
CORRESPONDENCE.—Water of the Carisbrooke Castle Well—Alco- hol Tables—"Redonda" Phosphate	57
CHEMICAL NOTICES FROM FOREIGN SOURCES	58

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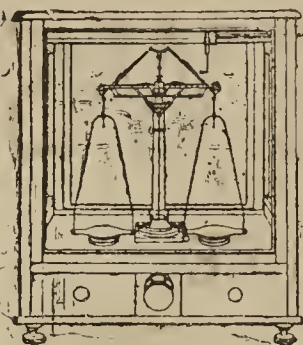
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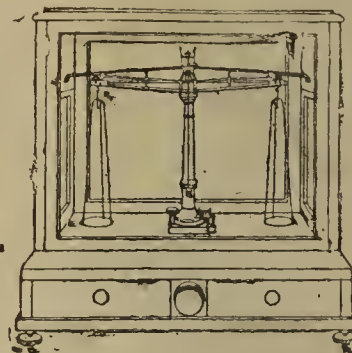
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	61
An Examination of some County Dublin Waters, by J. Fletcher, F.C.S.....	62
Notes on the Alkaloids, by T. Tattersall	63
On the Tannic Acid of Sumach Leaves, by H. Macagno.....	63
Cupric Test-pellets for Sugar.....	63
On the Deterioration of Library Bindings, by Prof. Wm. Ripley Nichols	64
Action of Bone-black on Solutions of Pure Sugar, by P. Casamajor	66
On the Estimation of Nitrous Compounds in the Various Stages of the Manufacture of Sulphuric Acid, by James Macfear, F.C.S., F.I.C.....	67
Artificial Production of Precious Stones, by A. H. Allen, F.C.S....	68
CORRESPONDENCE.—Testing of Vitriol Chamber Exits, &c.—	
Alcohol Tables	69
CHEMICAL NOTICES FROM FOREIGN SOURCES	70

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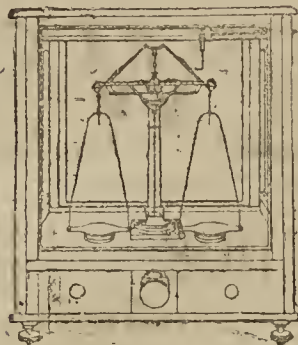
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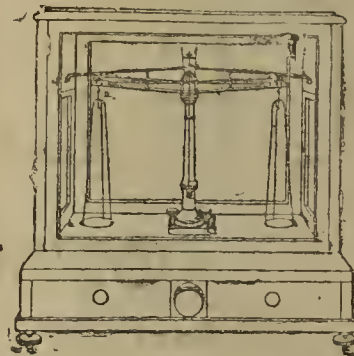


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MONDAY, 9th.—London Institution, 5.
— Medical, 8.30.
— Royal Geographical 8.30.
— Society of Arts, 8.
TUESDAY, 10th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
— Civil Engineers, 8.
— Anthropological Institute, 8.
WEDNESDAY, 11th.—Society of Arts, 8.
— Microscopical, 8.
THURSDAY, 12th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
— Royal, 8.30.
— London Institution, 7.
— Society of Arts, 8.
FRIDAY, 13th.—Royal Institution, 8. Mr. W. H. Preece, "Wheatstone's Telegraphic Achievements, 9
— Astronomical, 3.
— Quekett, 8.
SATURDAY, 14th.—Royal Institution, 3. Prof. Pauer, Sebastian Bach," (with Musical Illustrations.)
— Physical, 3. Annual General Meeting. "On a Quartz-Iceland Spar Achromatic Spectroscope," Dr. W. H. Stone.

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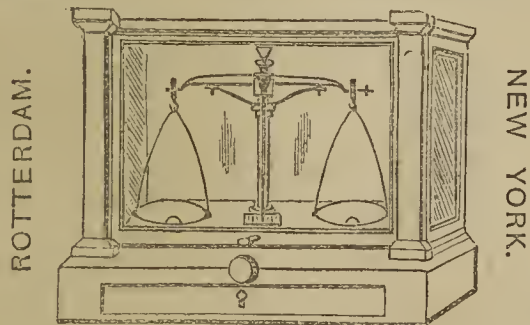
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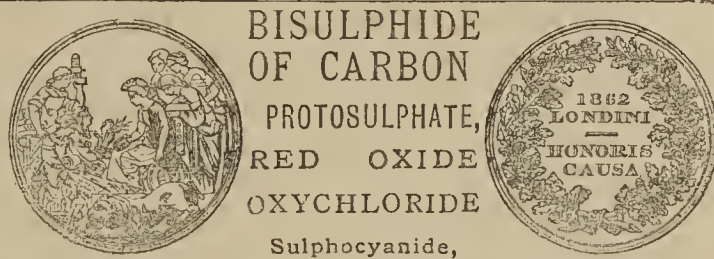
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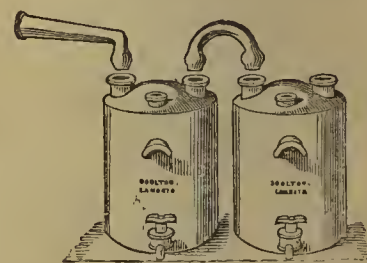
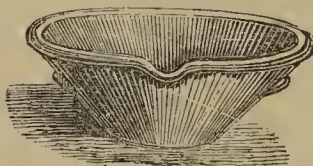
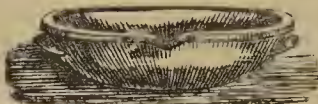
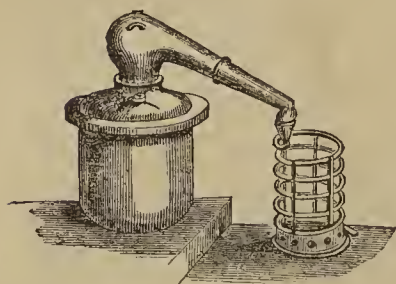
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	71
Remarks on the New Metals from Gadolinite and Samarskite, by M. M. Delafontaine	72
Products of the Slow Oxidation of Phosphorus, by R. H. Ridout..	73
The Results of Sewage Treatment	73
Researches on Cobalt and Nickel, and a Method for their Distinction in Mixed Solution, by Dr. G. Papasogii	74
On the Physical Constants of Liquid Hydrochloric Acid, by Gerrard Ansdell, F.C.S.....	75
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY	76
PHYSICAL SOCIETY	77
CORRESPONDENCE	78
CHEMICAL NOTICES FROM FOREIGN SOURCES	79
MISCELLANEOUS.—Balmain's Luminous Paint.....	81

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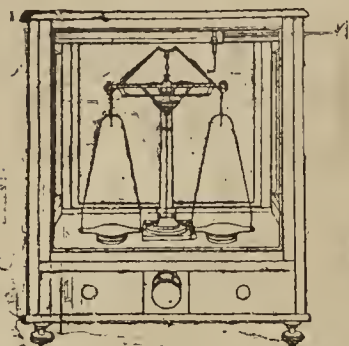
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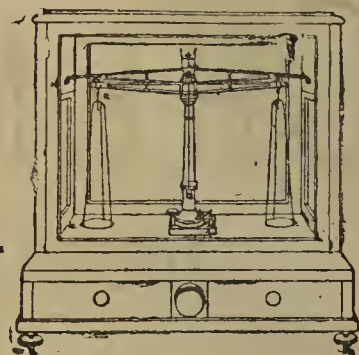


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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	83
On a New Method of Spectrum Observation, by J. N. Lockyer, F.R.S.....	84
PROCEEDINGS OF SOCIETIES:—	
SOCIETY OF ARTS—A New Metallic Compound	87
PHYSICAL SOCIETY.—On a Quartz and Iceland Spar Spectroscope Corrected for Chromatic Aberration—On an Automatic Switch for Telephone Circuits—Theory of Terrestrial Magnetism.....	91
CORRESPONDENCE.—Cupric Test-Pellets—Testing Vitriol Chamber Exits	91
CHEMICAL NOTICES FROM FOREIGN SOURCES	92
NOTES AND QUERIES.....	92

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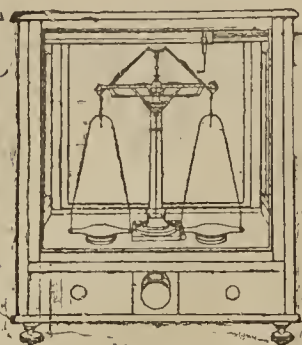
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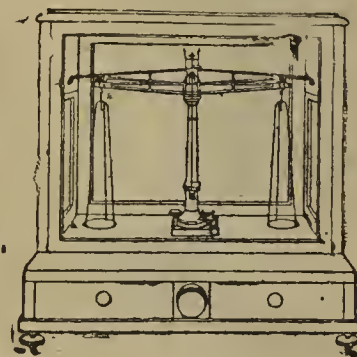


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MEETINGS FOR THE WEEK.

MONDAY, 23rd—London Institution, 5.
 — Medical, 8.30.
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 — Society of Arts, 8. "The Manufacture of India-rubber and Gutta-percha," by Thomas Blas, F.C.S.
TUESDAY, 24th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
 — Civil Engineers, 8.
 — Anthropological Institute, 8.
 — Society of Arts, 8. Mr. William Forster, "Views of Colonisation."
WEDNESDAY, 25th.—Society of Arts, 8. Mr. E. K. Muspratt, "The Noxious Gases Bill."
 — Geological, 8.
THURSDAY, 26th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
 — London Institution, 7.
 — Royal, 8.30.
FRIDAY, 27th.—Royal Institution, 8. Mr. P. J. Bramwell "Sequel to the Thunderer Gun Explosion," 9.
 — Quekett, 8.
SATURDAY, 28th.—Royal Institution, 3. Mr. Saintsbury, "Dryden and his Period."
 — Physical, 3. "On some Effects of Vibratory Motion in Fluids," Mr. H. R. Ridout. "On the Determination of Chemical Affinity in Terms of Electromotive Force," Part I., Dr. C. R. A. Wright; Part II., Dr. C. R. A. Wright and E. H. Rennie.

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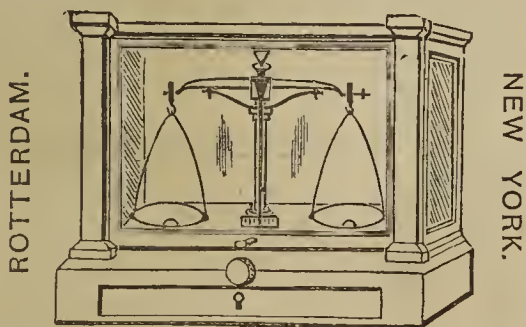
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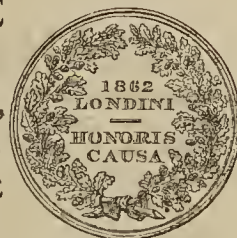


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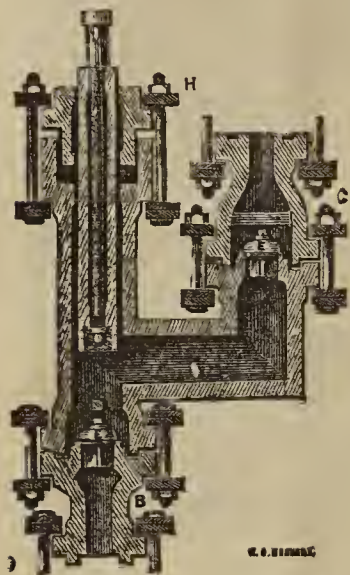
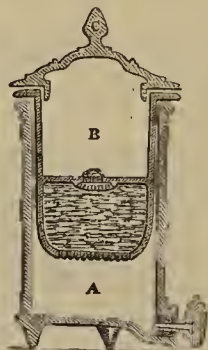
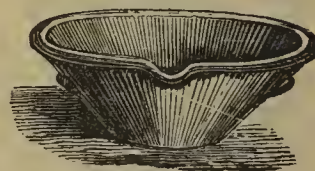
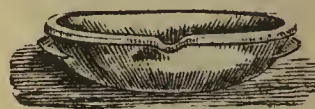
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	93
On some of the Effects Produced by an Induction-coil with a De Meritens Magneto-electric Machine, by W. Spottiswoode, P.R.S.	94
On the Use of the Spectroscope in Discriminating Anthracens, by Benj. Nickels, F.C.S., F.I.C.....	95
A Series of Analyses of Air, by H. Macagno	97
Artificial Production of the Diamond.....	97
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—Production of Ozone during the Combustion of Coal-gas—New and Improved Laboratory Appliances—Some Recent Researches on the so-called Unsaturated Compounds..	98
METEOROLOGICAL SOCIETY	99
CORRESPONDENCE.—Alcohol Tables—Chemical Society	100
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	101

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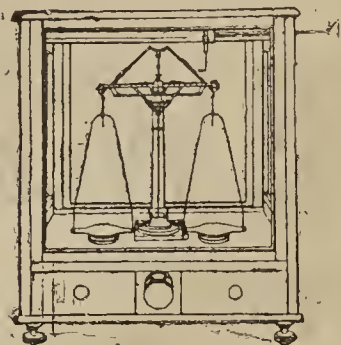
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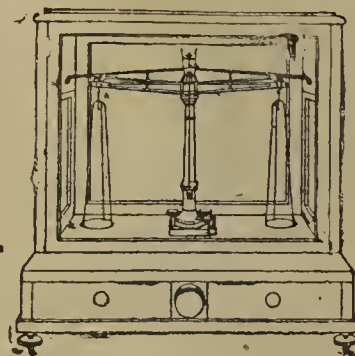


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CONTENTS.

ARTICLES:—	PAGE
On the Solubility of Solids in Gases, by J. B. Hannay, F.R.S.E., F.C.S., and James Hogarth, F.C.S.	103
On the Artificial Formation of the Diamond, by J. B. Hannay, F.R.S.E., F.C.S.	106
The Periodic Law of the Chemical Elements, by D. Mendeleef..	106
PHYSICAL SOCIETY.—Some Effects of Vibratory Motion in Fluids	
—Determination of Chemical Affinity in Terms of Electro-motive Force	108
NOTICES OF BOOKS.—The Retrospect of Medicine	109
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	109
MISCELLANEOUS.—Crystallised Carbon in Abstract—London	
Water Supply.....	111
MEETINGS FOR THE WEEK	111

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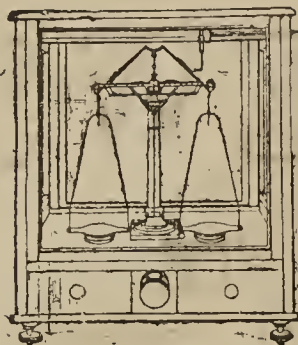
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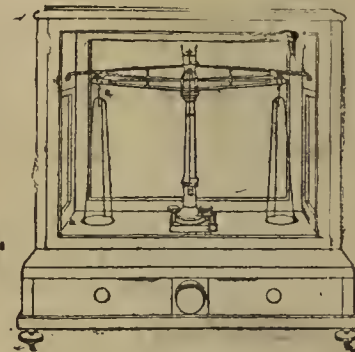


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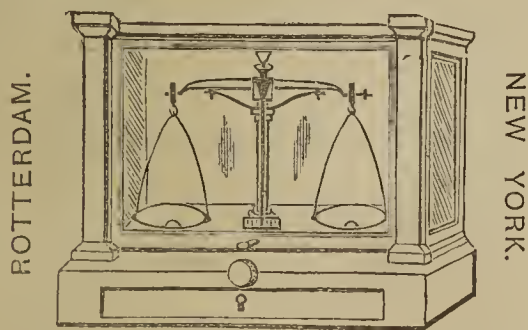
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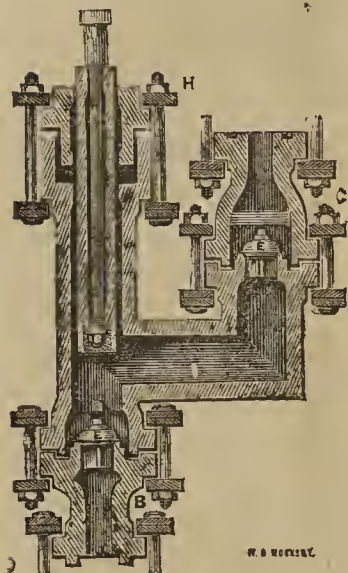
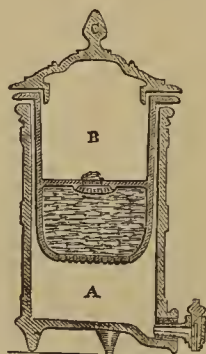
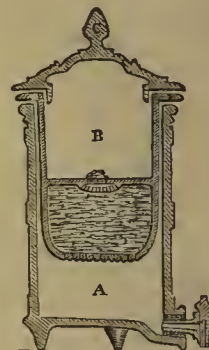
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	113
A New Method for the Alkalimetric Determination of Sulphates, and its Application to the Commercial Assay of Saltcake, by J. Grossmann, Ph.D.....	114
On the Reduction of Chloride of Gold by Hydrogen in the Presence of Platinum, by Dr. D. Tommasi.....	116
On Vesbium and Norwegium, by C. Rammelsberg.....	116
On the New Hydrogen Lines and the Supposed Dissociation of Calcium, by H. W. Vogel.....	116
On the Use of the Spectroscope in Discriminating Anthracens, by Benjamin Nickels, F.C.S., F.I.C.....	117
Some New Azo-Colours, by James H. Stebbins, jun., B.S.....	117
Compound Platinates and a New Platino-potassium Salt, by Lucius Pitkin.....	118
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—Relation between the Molecular Weights of Substances and their Sp. Gravities when in the Liquid State..	120
ROYAL INSTITUTION OF GREAT BRITAIN.....	121
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	121

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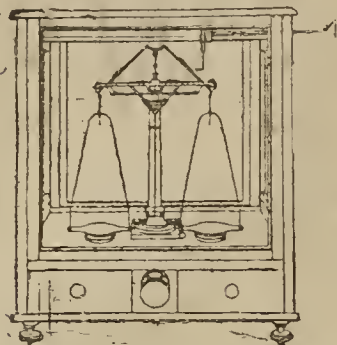
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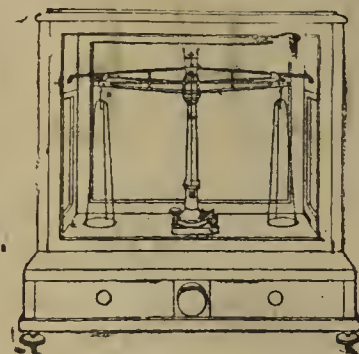
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CONTENTS.

ARTICLES:—	PAGE
The Periodic Law of the Chemical Elements, by D. Mendeleef..	125
On the Phenomenon of "Flashing" in Assays of Gold, and on the Influence of the Metals of the Platinum Group on it, by Dr. A. D. Van Riemsdijk	126
On the Composition and Analysis of Weldon Mud, by Dr. Lunge	129
PROCEEDINGS OF SOCIETIES:—	
PHYSICAL SOCIETY.....	131
BIRMINGHAM PHILOSOPHICAL SOCIETY	131
NEWCASTLE CHEMICAL SOCIETY.....	132
CORRESPONDENCE.—Action of Mercury on Solution of Sodium in Ammonia—Blowpipe Analysis—Transparency of Gold.....	133
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	121

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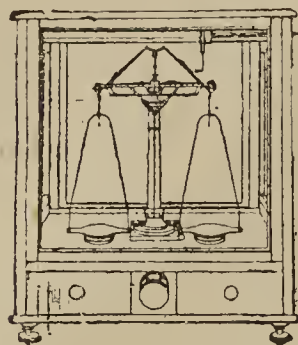
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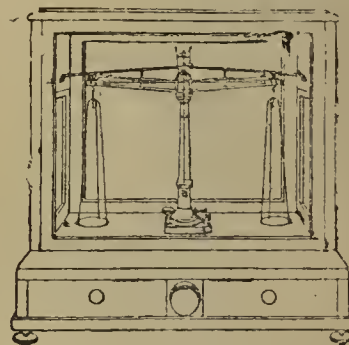


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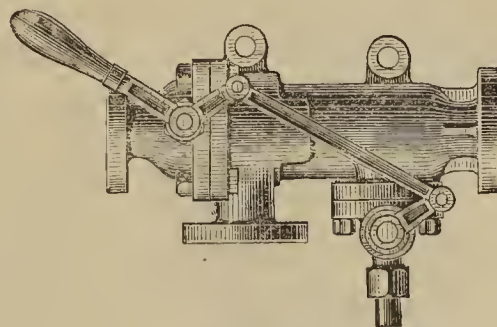
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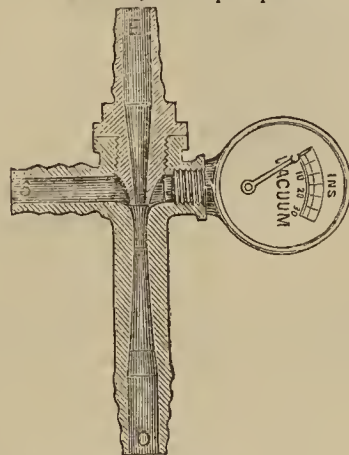
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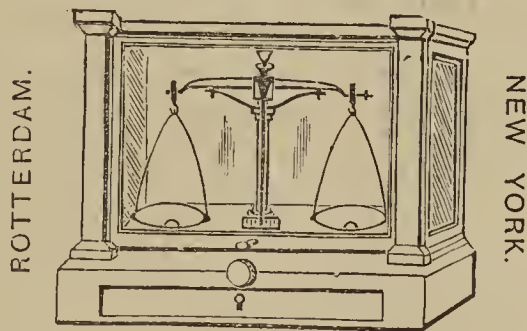
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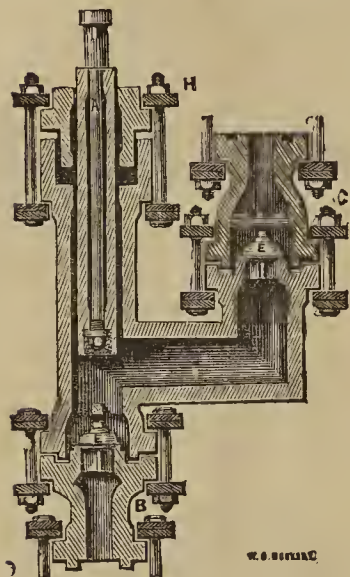
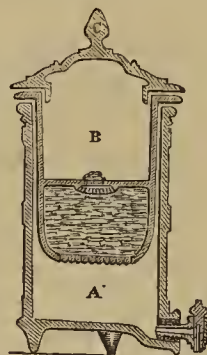
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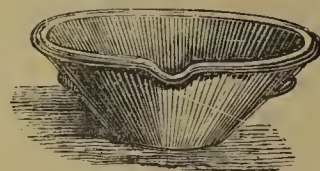
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CONTENTS.

ARTICLES:—	PAGE
A Contribution to the Knowledge of Chlorine, by V. Meyer and H. Züblin	135
Balmain's Luminous Paint, by C. W. Heaton, F.C.S.....	135
The Lines of Discovery in the History of Ozone, by Albert R. Leeds, Ph.D.....	138
On the Composition and Analysis of Weldon Mud, by Dr. Lunge	141
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—River Water.....	143
CORRESPONDENCE.—The Direct Method of Testing Vitriol Chamber Exits.....	145
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	146

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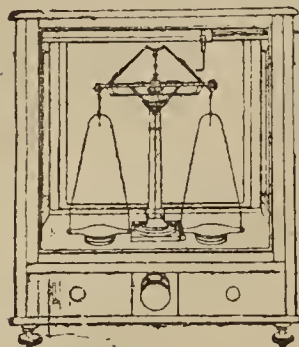
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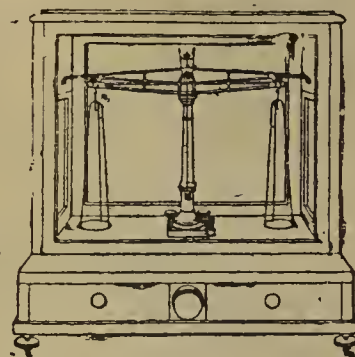


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CONTENTS.

ARTICLES:—	PAGE
On the Density of Bromine Vapour at a Yellow Heat, by V. Meyer and H. Züblin	151
On the Behaviour of Iodine at Elevated Temperatures, by V. Meyer	151
The Estimation of Carbon in Steel, by James W. Westmoreland	152
Boracic Acid as a Preservative, by H. Endemann	152
A Method for Estimating Bismuth Volumetrically, by M. Kuhara	153
A New Anæsthetic	153
A New Form of Instrument for the Determination of Specific Gravity, by F. P. Dunnington	154
Ammonia from the Nitrogen of the Atmosphere and the Hydrogen of Water	155
The Purification of Gas, by Harry Edward Jones, M. Inst. C.E.	155
INSTITUTE OF CHEMISTRY OF GREAT BRITAIN AND IRELAND....	156
CORRESPONDENCE.—"New" Methods—"New Metallic Compounds"—Blowpipe Analysis	158
CHEMICAL NOTICES FROM FOREIGN SOURCES	159

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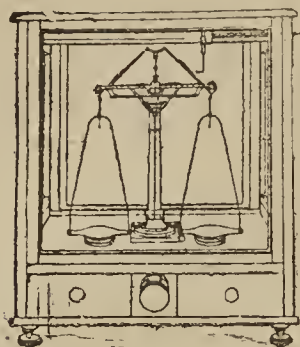
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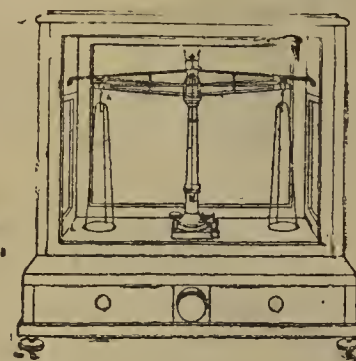
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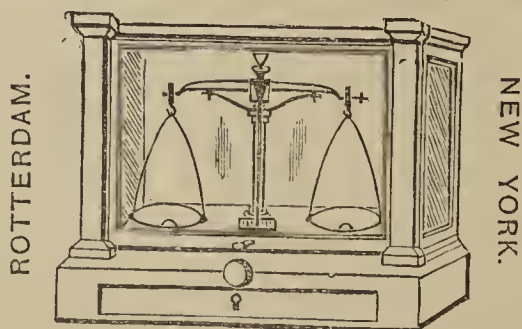
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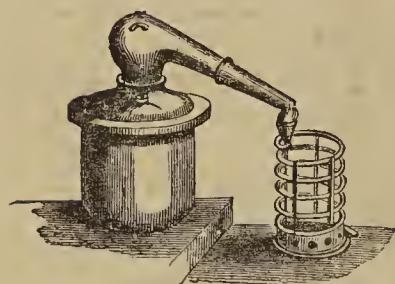
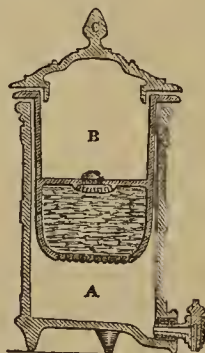
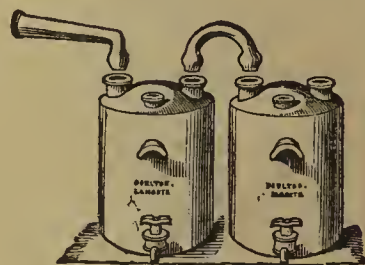
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CONTENTS.

ARTICLES:—	PAGE
Peroxide of Hydrogen and Ozone, by Albert R. Leeds, Ph.D.....	163
Notes on Solubility, by D. B. Dott	165
CHEMICAL SOCIETY.—Aniversary Meeting—Betorcinol and some of its Derivatives—Note on Chemical Equilibrium—Action of the New Diastase, Eurotin, on Starch—Products of Combustion of Coal-Gas—Polysulphides of Sodium—The Reflection from Copper, and the Colorimetric Estimation of Copper by means of the Reflection Cuprimeter—On Pyrene—Analysis of the Ash of the Wood of Two Varieties of the Eucalyptus—Action of Organo-zinc Compounds on Quinons.....	168
NOTICES OF BOOKS.—Saint Germain-en-Laye. The Sewage of Paris.....	171
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	171
NOTES AND QUERIES	174
MEETINGS FOR THE WEEK	174
TO CORRESPONDENTS.....	174

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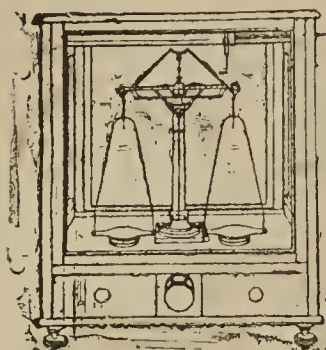
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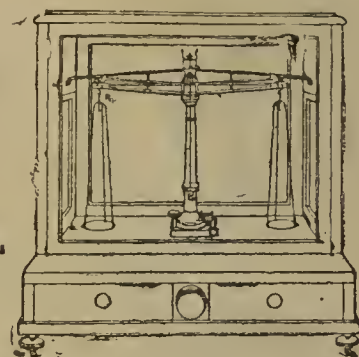


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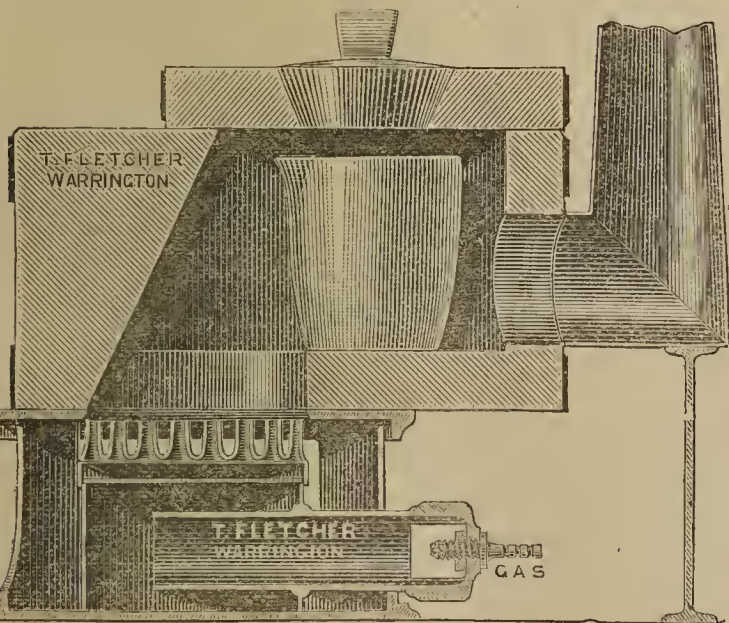
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CONTENTS.

ARTICLES:—	PAGE
Betorcinol and some of its Derivatives, by J. Stenhouse and Charles E. Groves	175
On Nascent Hydrogen, by Dr. D. Tommasi.....	176
Detection of Hydrochloric Acid by Sulphuric Acid and Acid Potassium Chromate, by H. W. Wiley.....	176
On Detection and Estimation of Arsenic, by T. D. Boeke, D.Sc..	177
Note on Steel Plates, by Sergius Kern, M.E.....	178
PROCEEDINGS OF SOCIETIES:—	
PHYSICAL SOCIETY.—Human Eye as an Automatic Photometer..	179
NEWCASTLE CHEMICAL SOCIETY.....	179
CORRESPONDENCE.—Composition of "Weldon Mud"—The direct Method of Testing Vitriol Chamber Exits—Peroxide of Hydrogen and Ozone—Edison's Electric Light—Alkalimetric Determination of Sulphates.....	181
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	183

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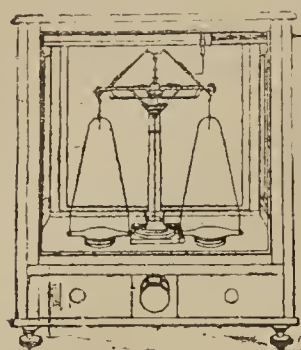
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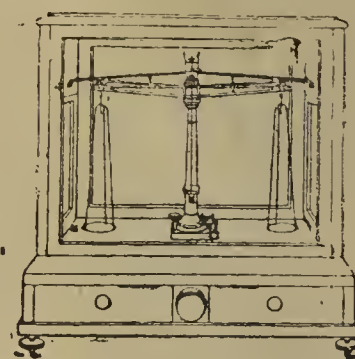


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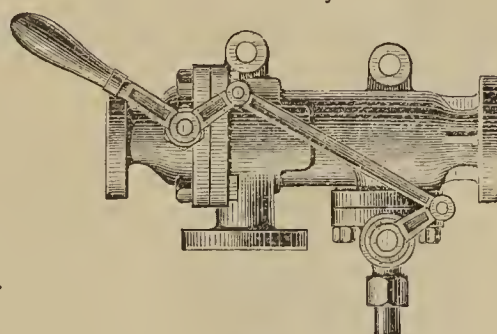
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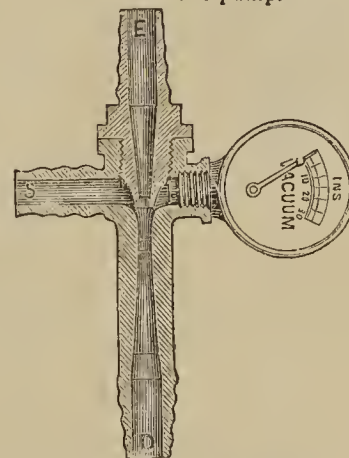
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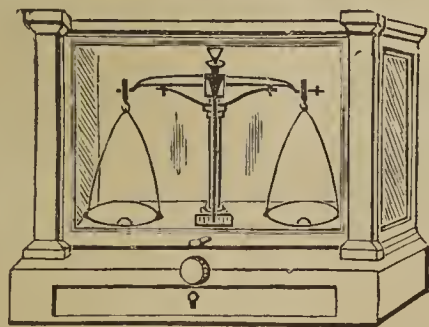


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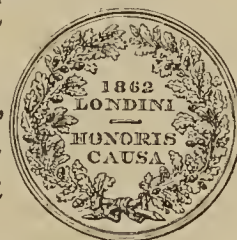
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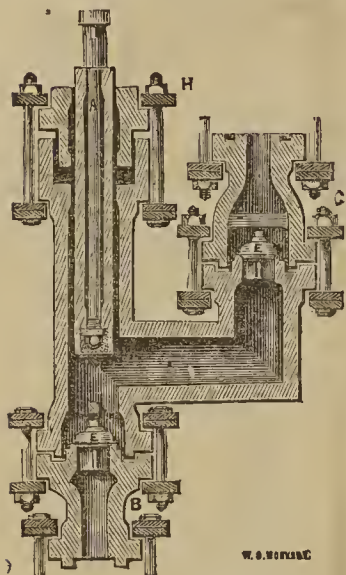
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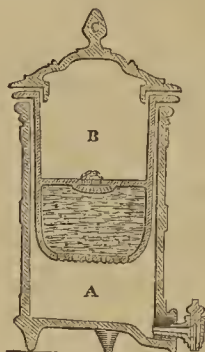
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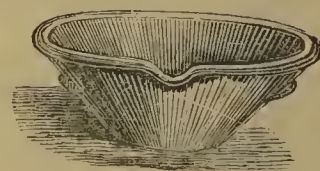
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CONTENTS.

ARTICLES:—	PAGE
A New Blowpipe Test for Phosphoric Acid, by Lieut.-Colonel W. A. Ross, late R.A.....	187
Edible Earth from Japan, by E. G. Love, Ph.D.....	187
A Direct Method of Testing Vitriol Exits for Nitrogen Compounds, by George E Davis.....	188
The Volatilising-point of Metallic Arsenic, by Ewing C. M. Conechy	189
On Portland Cement, by Mr. Watson.....	189
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—Lecture Illustration of Chemical Curves—Analysis of Organic Bodies containing Nitrogen—Volatilisation of Solids in Vacuo—Determination of Nitric Acid as Nitric Oxide by means of its Reaction with Ferrous Chloride—Six Possible Isomeric Dibrom-toluols..	191
NOTICES OF BOOKS.....	193
CORRESPONDENCE	195
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	195

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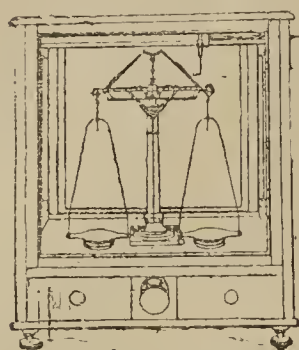
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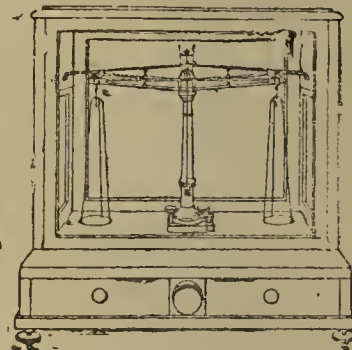


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CONTENTS.

ARTICLES:—	PAGE
Some Electrical Measurements of one of Mr. Edison's Horseshoe Lamps, by Henry Morton, Ph.D., Alfred M. Mayer, Ph.D., and B. F. Thomas, A.M.	199
On the Efficiency of Edison's Electric Light, by Prof. H. A. Rowland, and Prof. George F. Barker	200
On the Condition in which Sulphur exists in Coal, by Dr. W. Wallace, Glasgow	201
The Atomic Weight of Antimony, by Josiah P. Cooke	201
A New Method of Taking the Specific Gravity of Liquids, by H. Sommerkorn	203
On the History of Atomistic Periodicity, by Lothar Meyer.....	203
PROCEEDINGS OF SOCIETIES:—	
PHYSICAL SOCIETY	204
MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.—The Radiograph—Bog Butter from Ireland.....	204
NOTICES OF BOOKS.—Water Analysis for Sanitary Purposes—Glasgow Sewage	206
CORRESPONDENCE.—Volumetric Estimation of Manganese—Catalytic Force and the Nascent State—Ammonia from Atmospheric Nitrogen.....	207
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	208

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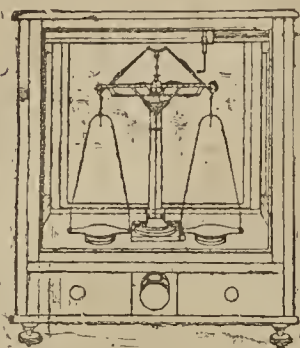
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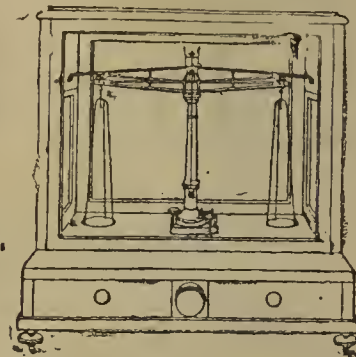
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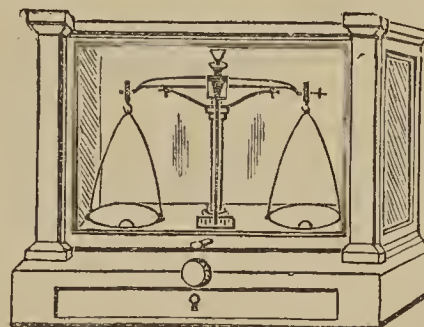


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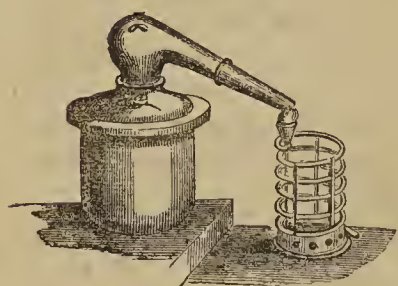
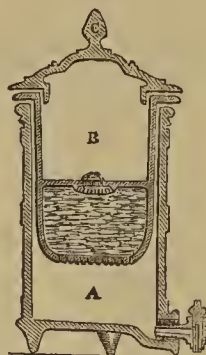
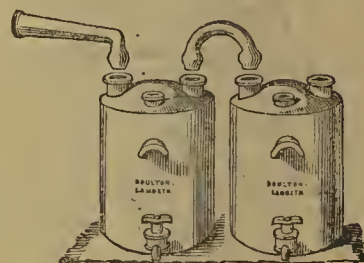
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CONTENTS.

ARTICLES:—	PAGE
Measurement of the Actinism of the Sun's Rays and of Daylight, by Dr. R. Angus Smith, F.R.S.....	211
Revision of the Atomic Weight and Valence of Aluminium, by J. W. Mallet, F.R.S.....	212
The Application of the Electric Current in Analytical Chemistry, by C. Luckow.....	213
Determination of Active Oxygen in Barium Bin oxide and in Oxygenated Water, by M. Armand Bertrand.....	215
On the Quantitative Determination of Oxygen Dissolved in Water, by J. König.....	215
On the Preservation of Solutions of Palmelline, by Dr. T. L. Phipson, F.C.S.....	216
NOTICES OF BOOKS.....	216
CORRESPONDENCE.—Ammonia from Atmospheric Nitrogen.....	218
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	218

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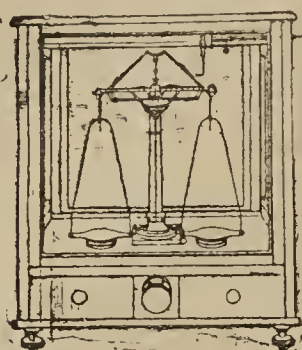
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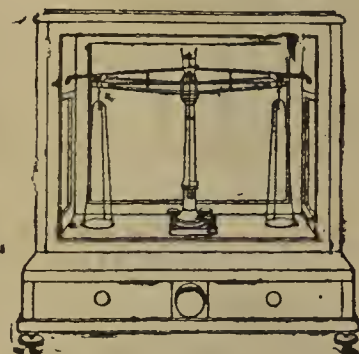


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CONTENTS.

ARTICLES:—	PAGE
On the Detection of Starch Sugar Mechanically Mixed with Refined Cane Sugar, by P. Casamajor	221
Volumetric Determination of Phenol, by P. Degener.....	222
On the Determination of Chromium, by Theodor Wilm.....	222
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—Action of Sodium on Phenyl Acetate—Action of Sodium on some Ethereal Salts of Phenyl-acetic Acid—Determination of Nitrogen in Carbon Compounds—Essential Oil of Sage—Presence of Nitrogen in Iron or Steel—Application of Pettenkofer's Process for the Determination of Carbonic Acid in the Expired Air—Improved Form of Oven for Heating Sealed Tubes and avoiding Risks of Explosions—Convenient Form of Lead-bath for Victor Meyer's Apparatus..	222
PHYSICAL SOCIETY	224
SOCIETY OF ARTS.—The Newer Artificial Colouring-matters Derived from Benzene	225
CORRESPONDENCE	228
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	230

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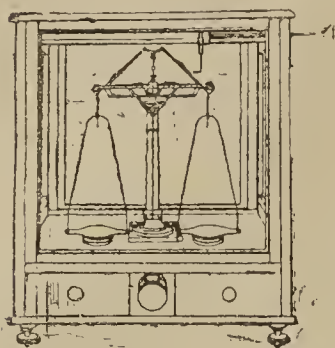
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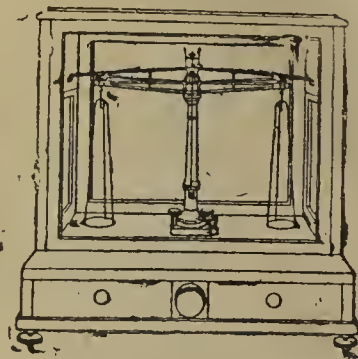


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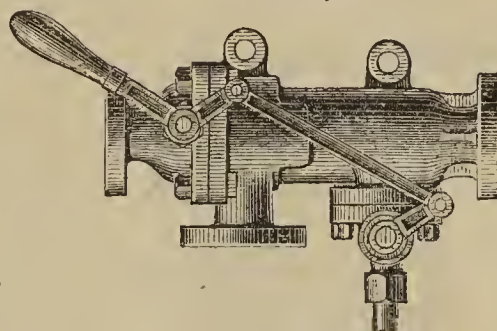
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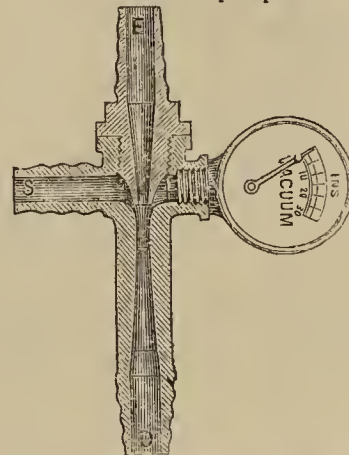
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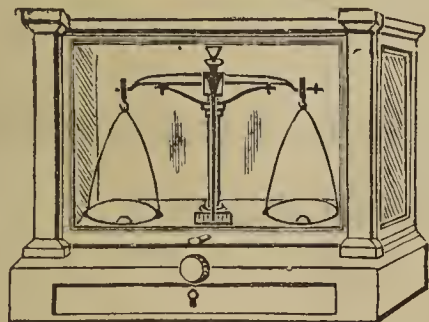


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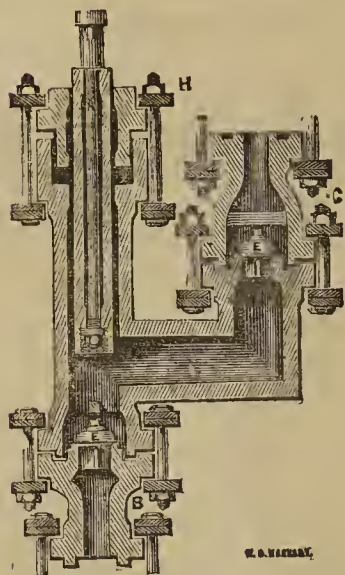
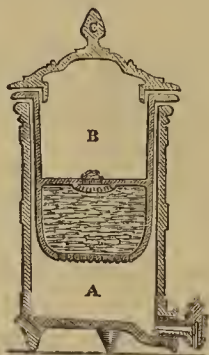
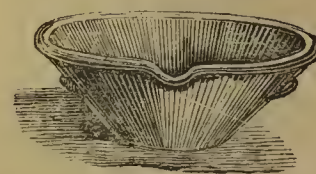
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CONTENTS.

ARTICLES:—	PAGE
On the Presence of Nitrogen in Iron and Steel, by Alfred H. Allen, F.C.S.....	231
On the Separation of Silica in the Analysis of Limestones, Iron Ores, and other Minerals, by H. Rocholl, F.I.C.....	234
The Metallurgical Chair at the Royal School of Mines.....	235
PROCEEDINGS OF SOCIETIES:—	
ROYAL INSTITUTION OF GREAT BRITAIN.....	235
PHILOSOPHICAL SOCIETY OF GLASGOW.—Formation of Sulphuretted Mineral Waters—Action of Concentrated Sulphuric Acid on Various Qualities of Lead.....	236
CORRESPONDENCE.—Dephosphorisation of Iron—Ammonia from the Nitrogen of the Atmosphere.....	237
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	230

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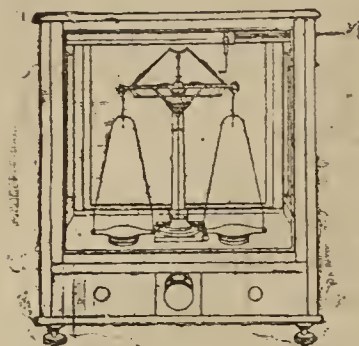
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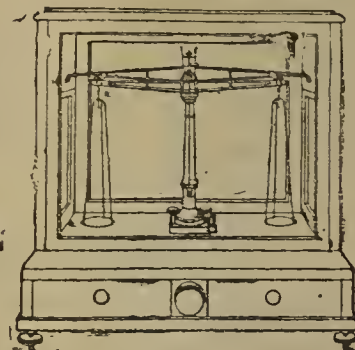


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CONTENTS.

ARTICLES:—	PAGE
Proposed Society of Industrial Chemists.....	243
Sulphuretted Hydrogen Apparatus, by A. Percy Smith, F.C.S., F.I.C.....	243
On a Black Substance Produced from Sulphur, by H. Chapman Jones.....	244
Analyses of some American Tantalates, by W. J. Comstock.....	244
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—On the Action of Air upon Peaty Water..	245
MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.—Analyses of the Ash of Wood of Two Varieties of the <i>Eucalyptus</i> Tree .	247
NOTICES OF BOOKS.—Alphabetical Manual of Blowpipe Analysis —The Chemistry of Agriculture.....	249
CORRESPONDENCE.—Sulphur in Pyrites—Blowpipe Analysis—The Estimation of Carbon in Steel.....	250
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	250
MEETINGS FOR THE WEEK	252

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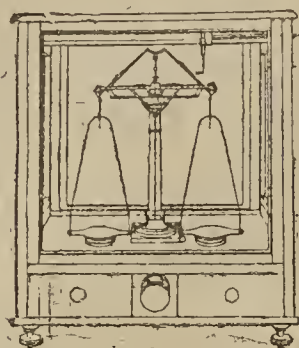
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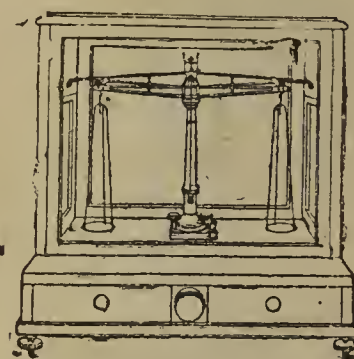
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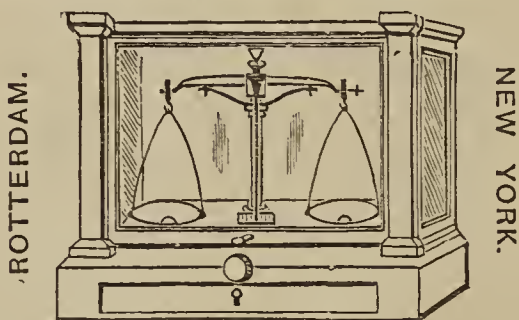
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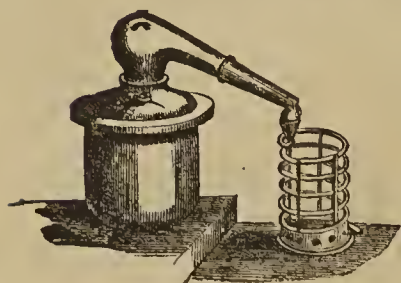
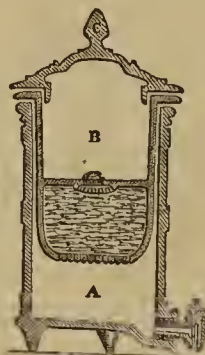
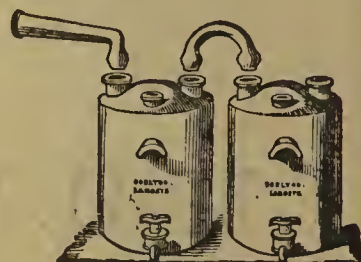
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CONTENTS.

ARTICLES:—	PAGE
On a Combination Blowpipe for Glass-working, by Charles H. Gillingham, F.C.S.....	253
Organic Matter in Water, by C. T. Kingzett, F.I.C., F.C.S.....	254
The Alkaloids.....	255
On the Recognition of Alizarin, Iso-purpurin, and Flavo-purpurin in Mixtures, and on the Quantitative Determination of Alizarin, by E. Schunck and H. Römer.....	256
A Method for the Proximate Analysis of Plants, by Henry B. Parsons	256
PROCEEDINGS OF SOCIETIES:—	
PHYSICAL SOCIETY	258
CORRESPONDENCE.—Ammonia from Atmospheric Nitrogen—Error in Vapour-density Determinations—River Water—The Proposed Society of Industrial Chemistry—Proposed Technical Society.....	259
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	261
NOTES AND QUERIES.—Extracts of Dyewoods	263
MEETINGS FOR THE WEEK	263
TO CORRESPONDENTS.....	263

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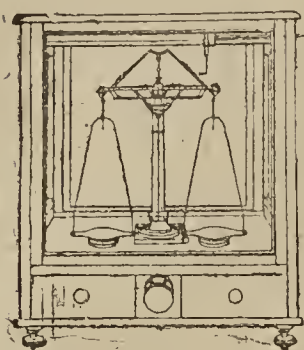
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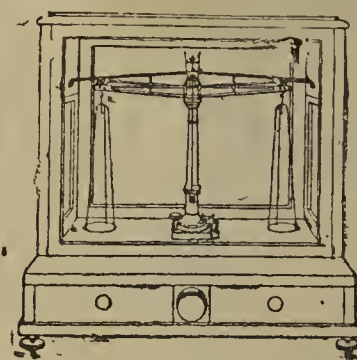


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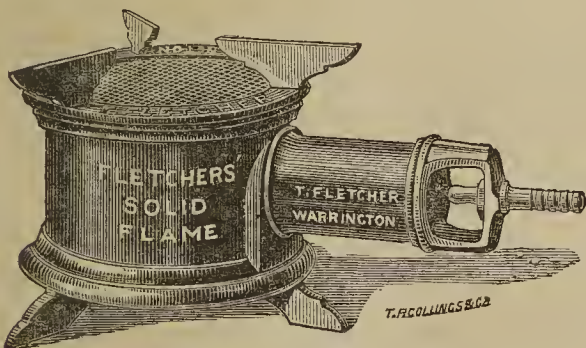
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CONTENTS.

ARTICLES:—	PAGE
Observations upon Dr. Tidy's Paper on River Water, by R. Warington	265
On the Influence of Superfusion on the Molecular Arrangement of Cupelled Gold, by A. D. van Riemsdijk	266
A Method for the Proximate Analysis of Plants, by Henry B. Parsons	267
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—On some Products of the Oxidation of Paratoluydin—Detection of Foreign Colouring-Matters in Wine—Action of Organo-zinc Compounds upon Nitriles and their Analogues—Action of Zinc-ethyl upon Benzo-nitrile—Relation between the Molecular Structure of Carbon Compounds and their Absorption-spectra—Simple Method of Determining Vapour-densities in the Barometer Vacuum	269
MINERALOGICAL SOCIETY.....	271
CORRESPONDENCE	271
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	273

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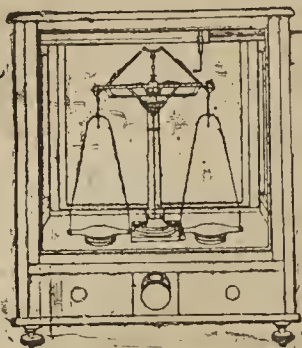
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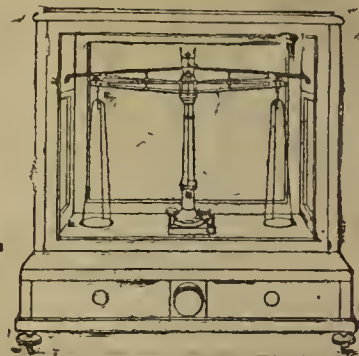


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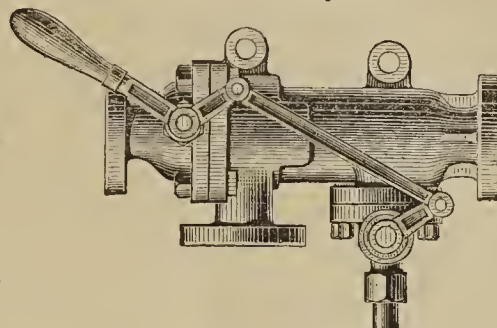
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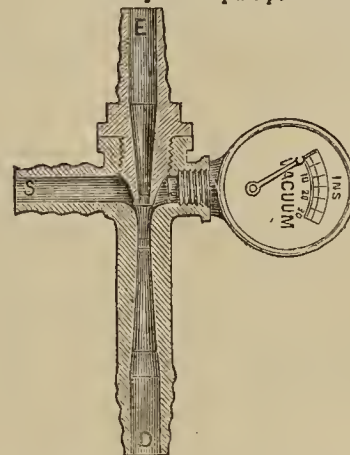
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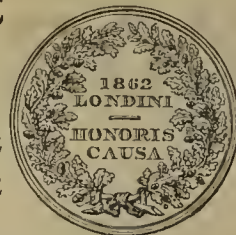
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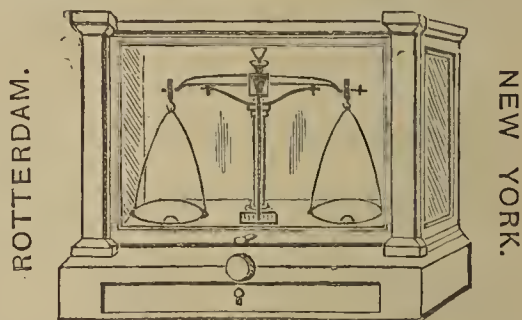
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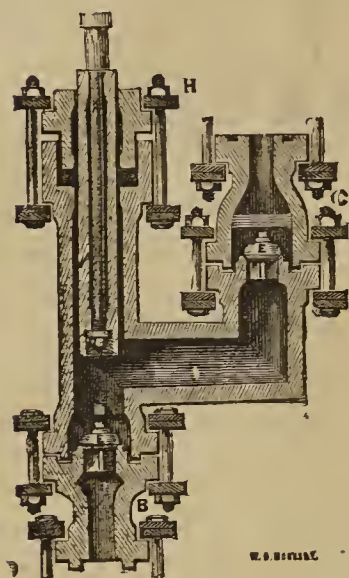
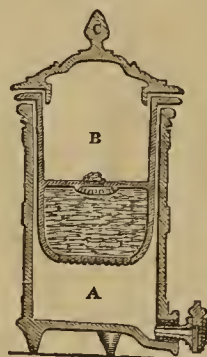
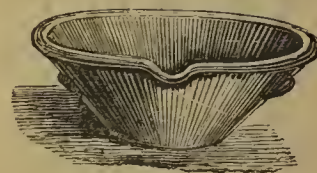
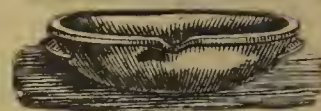
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CONTENTS.

ARTICLES:—	PAGE
On a Fourth State of Matter, by W. Crookes, F.R.S.....	275
On the Composition and Analysis of the Weldon Mud, by Dr. Jul. Post	276
Some Remarks on Siemens-Martin Steel, by Sergius Kern, M.E.	278
Analyses of some Hair-dyes, by J. F. Braga	278
Note on the Actual State of the Determination of Zinc, by W. Alexandrowicz	279
The Behaviour of Sulphuretted Hydrogen with the Salts of the Heavy Metals, by H. Delffs	279
On an Automatic Mercurial Pump, by M. G. Couttolenc.....	279
On the Electrolytic Determination of Metals, by Ludwig Schicht	280
Detection and Determination of Chlorine in Presence of Bromine and Iodine, by G. Vortmann.....	280
NOTICES OF BOOKS.—Potable Water—Properties, Preparation, and Uses of Methyl Chloride—A Course of Qualitative Chemical Analysis—Price List of Dr. Theodor Schuchardt's Chemical Works, Goerlitz—On Shoyu.....	280
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	282
NOTES AND QUERIES.—Stearate of Soda—Analysis of Cinchona Bark	284
MEETINGS FOR THE WEEK	284

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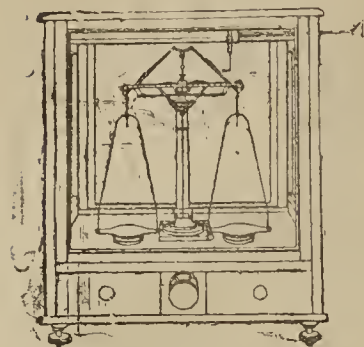
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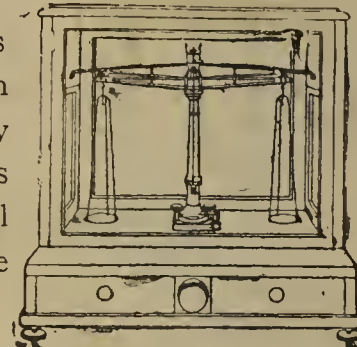
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MEETINGS FOR THE WEEK.

TUESDAY, 22nd.—Anthropological Institute, 8. Peter Bembridge, "The Aborigines of Victoria." Prof. W. H. Flower, F.R.S., "On a Collection of Crania from the Fiji Islands." Rev. Lorimer Fison, "Land Tenure in Fiji."

WEDNESDAY, 23rd.—Geological, 8.
Society of Arts (Anniversary), 4.

FRIDAY, 25th.—Quekett, 8.

SATURDAY, 26th.—Physical, 3. Dr. Balfour Stewart, "On a Modification of Bunsen's Colorimeter." F. Guthrie and C. V. Boys, "On Magneto Electric Induction." W. Huggins, "Exhibition of Star Spectra." Dr. J. H. Gladstone, "On the Refraction Equivalents of Isomeric Bodies."

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CONTENTS.

ARTICLES:—	PAGE
A New Patent Bill	285
Note on the Bearing on the Atomic Weight of Aluminium of the Fact that this Metal Occludes Hydrogen, by J. W. Mallet, F.R.S.	285
Preliminary Note on some Orcinol Derivatives, by J. Stenhouse, LL.D., F.R.S., and Charles E. Groves.....	286
The Absorption of Sugar by Bone-black, by Henry A. Mott, Jr., Ph.D., E.M.....	287
Distillation of Gas-tar.....	289
PROCEEDINGS OF SOCIETIES:—	
CHEMICAL SOCIETY.—On Pentathionic Acid—On some Orcinol Derivatives—Determination of Carbon in Soils—Note on Camphydrene—Action of Nitric Acid upon Dipara-tolyl-guani- dine—Some Higher Oxides of Manganese and their Hydrates— A New Method of Preparing Dinitro-ethylic Acid—Action of Organo-zinc Compounds upon Nitriles and their Analogues— Action of Benzoyl Chloride on Morphine—Examination of Terpenes for Cymene by means of the Ultra-violet Spectrum— Notes on the Purple of the Ancients.....	290
PHYSICAL SOCIETY	292
METEOROLOGICAL SOCIETY	292
NOTICES OF BOOKS.....	293
CORRESPONDENCE.—A New Phenol Reaction—Celluloid—Irish Bog Butter	294
CHEMICAL NOTICES FROM FOREIGN SOURCES.....	295
MISCELLANEOUS.—Tables of Specific Gravities—Society of Arts	295

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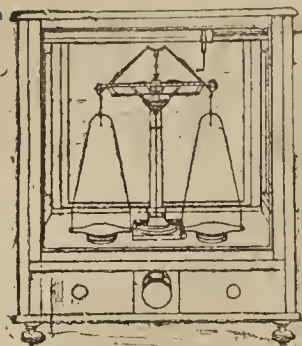
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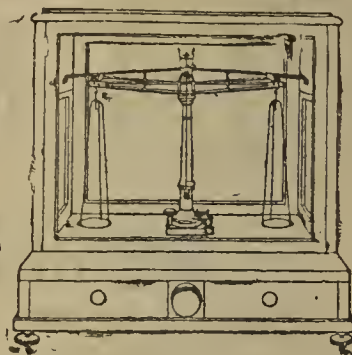


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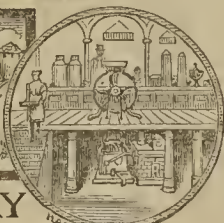
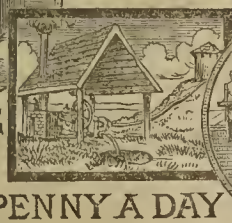
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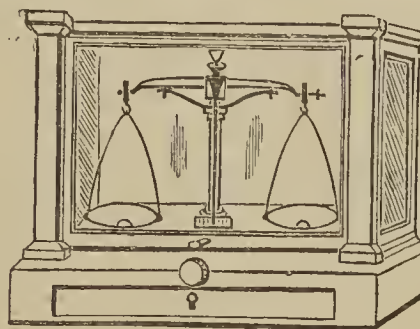
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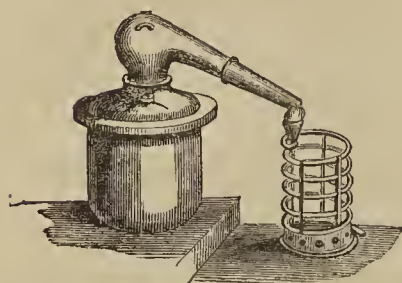
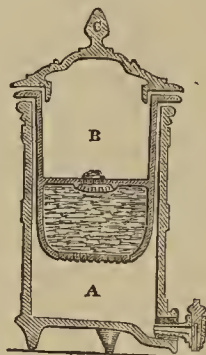
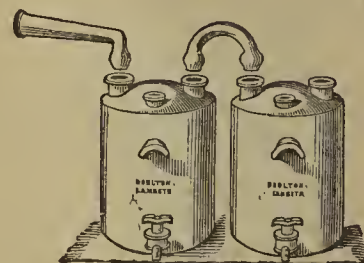
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THE CHEMICAL NEWS.

VOLUME XLI.

EDITED BY WILLIAM CROOKES, F.R.S., &c.

No. 1049.—JANUARY 2, 1880.

ON A NEW HYDRIDE OF SILICIUM.

By M. J. OGIER.

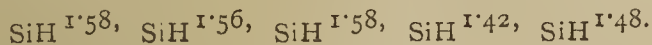
1. I HAVE submitted pure siliciuretted hydrogen to the action of the electric effluvium.* After a sufficient time this gas is entirely destroyed; a yellow coating is formed in the interior of the tubes, and the volume of the gas increases up to an apparently constant limit.

2. The residual gas does not contain a trace of silicium, but it is simply pure hydrogen.

The ratios between the volumes of the siliciuretted hydrogen used, and the hydrogen resulting from its decomposition, have been found in five different experiments to be:—

$$\frac{1}{1'21}, \quad \frac{1}{1'22}, \quad \frac{1}{1'21}, \quad \frac{1}{1'29}, \quad \frac{1}{1'26}.$$

From these results it is easy to find the composition of the solid which was left on the glass, which contains the remainder of the hydrogen, united with all the silicium. In this way we arrive at the following formulæ ($\text{Si} = 28$):—



These figures answer very nearly to the composition of Si_2H_3 . The body under examination will, therefore, be a subhydride of silicium, corresponding to the suboxide of carbon, or again to crotonylene, in the series of carburetted gases.

3. Some of the properties of this body are as follows:—When moderately heated in air it burns, throwing off small sparks. The combustion takes place spontaneously when hit with a hard body, or when rubbed with a steel blade. This subhydride is inflammable in chlorine when cold. When heated cautiously in an atmosphere of hydrogen or nitrogen, it evolves siliciuretted hydrogen, easily recognised by its spontaneous inflammation in the air; a higher temperature only causes a deposit of silicium with the evolution of hydrogen.

4. We have seen that the electric effluvium at a high tension produces effects on siliciuretted hydrogen, analogous to those which it produces on formene (the formation of acetylene, and a condensed carbide having a smell like turpentine, according to M. Berthelot). The analogy rests, if we study the fixation of nitrogen, by the effluvium

on the siliciuretted gas. In fact, a mixture of equal volumes of nitrogen and siliciuretted hydrogen gave after the action of the effluvium a residual gas, consisting of hydrogen, nitrogen, and ammonia, which latter was easily detected by means of hydrochloric acid; but the ammonia was present in a very minute quantity, in fact it was hardly 1 per cent. of the residue.

On the other hand, the solid matter had equally absorbed a small quantity of nitrogen, which was evolved as ammonia by means of melting potash. This is exactly what was observed when the mixture of formene and nitrogen was submitted to the action of the electric effluvium.

5. I have experimented to see if any analogous hydride would be produced when siliciuretted hydrogen is carefully heated. This gas is almost as stable as the corresponding carbides of hydrogen, for it will stand a temperature of nearly 400° before beginning to decompose. Keeping it at this temperature, I observed after a quarter of an hour a decomposition of more than 10 per cent.; after an hour the destruction was almost complete. Thus the siliciuretted hydrogen resolves itself, purely and simply, into silicium and hydrogen without forming any intermediate hydrides as the carbides of hydrogen do.

6. I have similarly studied the action of the effluvium on arseniuretted hydrogen; it also forms a solid hydride As_2H , corresponding to the solid phosphide of hydrogen, P_2H .*—*Comptes Rendus*, Dec. 22, 1879.

ANSWER TO THE NOTE BY DR. PHIPSON

CALLED

‘NEW AND OLD VIEWS ON THE NASCENT STATE OF BODIES.’

By Dr. TOMMASI.

In the note by Dr. Phipson there are three points on which I should like to make some observations:—

1. Dr. Phipson says:—Many chemists will doubtless have been misled by the title of Dr. Tommasi's first note, ‘On the Non-Existence of Nascent Hydrogen,’ but it appears by his second note that he admits its existence, for he says:—‘The reductive power, &c. . . . So that the author not only does *not* deny the existence of nascent hydrogen, but actually supports it by a theory of his own.’

* In the pipettes described by M. Berthelot (*Annales de Chimie et de Physique*, 5th series, vol. x., p. 76), in which the gas can be very easily introduced, taken out, and measured over mercury.

* This research was carried on in M. Berthelot's laboratory at the Collège de France.

I will remind Dr. Phipson that since I have occupied myself with the question of nascent hydrogen, I have always been convinced that hydrogen is much more active at the moment of liberation than when in the ordinary state; and the only aim of the experiments that I have done on this subject has been to find out the cause of this singular anomaly.

Dr. Phipson is therefore in error when he thinks that I contradict myself, because, after having denied the existence of nascent hydrogen (as being a state peculiar to this gas), I gave an explanation of it later on. Dr. Phipson's observation arises no doubt from a misunderstanding; therefore, I shall not press this point any further.

2. According to Dr. Phipson, my theory on nascent hydrogen is nothing more than a reproduction of the theory that he started in 1855, with this difference only, that in the place of "electricity" I have employed the word "caloric."

It is possible that Dr. Phipson is right, but it seems to me that he ought to have the kindness to prove it.

Why does he not bring forward some of the passages from his pamphlet on "Nascent Hydrogen?" Why does he not make known the experiments that he has made on this subject? Finally, why does he not give the definition of nascent hydrogen as it is, without doubt, in the pamphlet?

M. Berthelot also not long ago,* pretended that he was the first to give a rational theory on the nascent state of hydrogen, and it is in the following terms that he does so.

"All this (reductions produced by alkaline amalgams), enters into the general theory of the reactions attributed formerly to the nascent state, reactions of which I exposed the real interpretation in 1865, during my lectures at the 'College de France' (published by the *Revue des Cours Publics*, and partially reproduced in the *Annales de Chimie et de Physique*, 4th series, vol. xviii., pp. 61, 66, 15 and thereabouts Sept., 1869)." But if you open these two volumes, you will not be able to find a single sentence which makes one even suspect why nascent hydrogen is more active than ordinary hydrogen. That, however, does not stop M. Berthelot from being altogether as convinced as Dr. Phipson that he was the first to explain the nascent state of hydrogen. I will even venture to say, that to give an explanation of the action of the reduction of alkaline amalgams, M. Berthelot has been obliged to have recourse to my thermic theory, and to avail himself of my own expressions.

3. After having addressed flattering words to me, for which I thank him greatly, Dr. Phipson proceeds in this manner; "Dr. Tommasi will be the last person not to see the identity of the reduction of auric chloride by hydrogen gas in presence of platinum, for instance, with the phenomena of hydrogen in the nascent state."

By a lucky chance Dr. Phipson has not made a mistake in saying that, for it is precisely the following that one sees in my first pamphlet on nascent hydrogen.

"According to the researches of M. Becquerel it follows that whereas hydrogen does not reduce a solution of chloride of gold, it acquires this property when the solution is in contact with a platinum wire, of which one end is plunged in the liquid and the other in the gas; the gold is precipitated in a metallic state on the part of the wire which is in the liquid, and the gas is absorbed according as the deposit is affected. M. Becquerel explains these reductions by the action that the presence of platinum would give rise to. As for us, we think that the reduction of the chloride of gold by means of hydrogen and platinum is due to the heat disengaged during the condensation of the hydrogen on the platinum." (*Rendiconti del R. Istituto Lombardo*, serie ii., vol. x., fasc. xv., p. 6. 1877.)

Solubility of Benzoic and Salicylic Acids.—E. Bourgoin.—At 0° 1 part of benzoic acid requires for solution 588 parts of water and 1 part of salicylic acid, 666 parts. —*Journal de Pharmacie et de Chimie*.

* *Journal de Pharmacie et de Chimie*, Août, 1879, p. 132.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

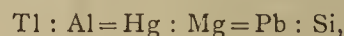
(Continued from vol. xl., page 304.)

THE remarkable researches of Roscoe, on vanadium, have shown the quantitative, and also in many cases the qualitative (such as in the oxychloride), analogy of this element with phosphorus. The position that vanadium holds in the system is expressed by the following proportions which, I think, do not need further explanation:—

- (1.) $V : P = Nb : As$, or $V : As = Nb : Sb = Ta : Bi$;
- (2.) $V : P = Ti : Si = Cr : S$;
- (3.) $V : Cr : Ti = Nb : Mo : Zr$.

It is seen from these proportions that the true atomalogues of V are, on the one hand, Ti and Cr, and on the other hand, Nb and Ta, with which it shows more analogy than with P: in the same way Cr is more nearly related to Mo and to W than it is to S. The corresponding compounds of Ti, Cr, and V resemble one another in their reactions, their properties, and even in their aspect. The yellow colour of the acid salts of chromium are very similar to many of the acid salts of vanadium: the green colour of the salts of sesquioxide of vanadium reminds us of the green colour of the salts of sesquioxide of chromium. The distribution of V in nature corresponds with that of Ti and of Cr. The analogy which exists between the oxychlorides $VOCl_3$ and $POCl_3$ has for a companion the analogy between CrO_2Cl_2 and SO_2Cl_2 . Further, this circumstance corresponds with the interesting case of isomorphism that Marignac has noticed between R_2TiF_6 , R_2NbOF_5 , and $R_2WO_2F_4$.*

We know that the most different opinions have been expressed relative to the position that thallium holds in the system until its properties were fully known. According to the periodic law this element is placed in the eleventh series, between $Au=197$ and $Hg=200$ on the one hand, and $Pb=207$ and $Bi=208$ on the other. The nature of its highest oxide, Tl_2O_3 , gives it at the same time a place in the third group, near $Al=27$ (and $In=113$, as we shall see in the following chapter). This place corresponds to all the properties of Tl, although at first sight it gives rise to doubts, the oxide of thallium, Tl_2O_3 , showing very little analogy with alumina. However, the proportion—



carries sufficient conviction that such is the natural position of thallium.

It is only the highest oxide of Hg—that is, HgO —which shows any analogy with MgO ; PbO_2 , the highest oxide of Pb, only shows any analogy with SiO_2 . In the same manner, Tl only shows any analogy with Al in its highest oxide, Tl_2O_3 . HgO and MgO are bases which give salts, such as RX_2 ; Tl_2O_3 and Al_2O_3 are less energetic bases which give neutral and basic salts such as RX_3 ; PbO_2 and SiO_2 , on the contrary, are but feebly acid oxides. Oxide of thallium is relatively to acids a more energetic base than Al_2O_3 , exactly as HgO takes the place of MgO . If Tl gave, independently of Tl_2O_3 , a highly basic suboxide, Tl_2O , of which Al is not capable, Hg would give Hg_2O independently of HgO , whilst for Mg we only know of one form of oxide. Pb, differently to Si, gives independently of PbO_2 a highly basic oxide, PbO .

The higher oxides HgO , Tl_2O_3 , PbO_2 , Bi_2O_5 are peroxides, relatively to the lower forms, Hg_2O , Tl_2O , PbO , Bi_2O_3 . If in Bi_2O_3 the basic properties are more feebly expressed than in PbO , Tl_2O , that is explained by the more strongly acid properties of Bi_2O_5 , relatively to Tl_2O_3 and to HgO . If Tl_2O in the salts corresponds in part to

* We can consider $R_4MnO_3F_9$ and $R_2OsO_4F_9$ as the continuation of this series, although it is rather doubtful. (See Marignac *Bibliothèque Universelle de Genève*, t. xxiii.; *Annales de Chimie et de Physique* [3], t. lxxix.)

X_2O , we can also find an analogy between PbO , in the salts PbX_2 , and CaO , as well as between Bi_2O_3 , in the salts BiX_3 , and the elements of the third group, which give salts such as RX_3 . The highest oxides, HgO , Tl_2O_3 , PbO_2 , and Bi_2O_5 , are coloured powders; they can be reduced to lower oxides, and when calcined they give off oxygen; mercury forms a stable perchloride, $HgCl_2$, corresponding to the maximum of oxidation; perchloride of thallium has been prepared ($TlCl_3$, or apparently Tl_2Cl_6), but it is easily decomposed by heat with the evolution of chlorine, and the lower form of combination, $TlCl$, is formed. This explains why lead and bismuth do not furnish us with compounds, as $PbCl_4$ and $BiCl_5$, but give stable bodies, $PbCl_2$ and $BiCl_3$. $BiCl_3$ is easily decomposed by water; $PbCl_2$ is only decomposed by steam when red hot; $TlCl$ is stable. Thallium is less volatile than mercury, and more so than Bi or Pb. All the properties that I have mentioned are shown by the following proportions:—

- (1.) $Tl : Hg : Pb = Al : Mg : Si$;
- (2.) $Tl : K = Pb = Ca$;
- (3.) $Tl : Hg = Pb : Tl = Bi : Pb$.

It should not be forgotten that these relations are not arbitrary; that they not only express the relation of the properties, but also that of the atomic weights. Thus the proportion—

$$Tl : Hg : Pb = Al : Mg : Si.$$

is represented by the members—

$$204 : 200 : 208 = 27 : 24 : 28.$$

If, instead of the sign ($:$) of geometric proportion, we put the sign ($-$) used in arithmetic proportion, we get in reality—

$$204 - 200 - 207 = 27 - 24 - 28.$$

However, we must remember, first, that we do not know exactly the real value of atomic weights; and, secondly, it is necessary to observe that we are unable to wait for completely exact relationships, because the true function, expressing how the properties depend on the atomic weights, is unknown to us.

Further, it is certain that the elements possess, independent of the primary properties which determine the analogy and the position of elements in the system, certain individual properties. When the above-mentioned function has to be found exactly, these properties will be explained without doubt by the irregularities in the variations of the atomic weights, irregularities which now appear to be inexplicable.

We can also foresee in the atomic law a kind of perturbation, which cannot cause us to suspect the exactness of the law. The transition of HgO to Hg_2O , and of Tl_2O_3 to Tl_2O , is in the example quoted. A disturbance of this kind for the higher oxides would keep the suboxides of unequal composition subordinate. These suboxides are, among themselves, in the same relation as Cu_2O or Ag_2O is to K_2O or to Na_2O . The suboxide of mercury is, in many respects, analogous to oxide of silver; as suboxide of thallium is analogous to oxide of potassium. However, the salts HgX and TlX concord more between themselves and with AgX than AgX and KX do. These phenomena are already very complicated, and if I mention them here it is only to call attention to the problems raised by the application of the periodic law to the system of elements. The periodic law, I think, should not be neglected when chemistry becomes more elaborated, although it has, I well know, need of development.

I will add still another remark: it is that the use of the periodic law facilitates the learning of chemical facts by beginners. I have come to this conclusion during the courses of lectures that I have given for two years, and during the preparation of my "Traité de Chimie Inorganique," now published (in Russian), which treatise is based on the periodic law.

(To be continued.)

EXPERIMENTS ON THE CONTAMINATION OF ATMOSPHERIC AIR WITH THE VAPOURS ARISING FROM ARSENICAL PIGMENTS.*

By WILLIAM FOSTER, M.A. (Cantab.), F.C.S., &c.,
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LIKE others similarly circumstanced to myself, I am frequently called upon to examine fabrics and domestic materials for arsenicum. An ordinary but marked case came under my notice about two and a half years ago, some particulars of which are given in the *Lancet* of the 11th August, 1877. The circumstances were briefly as follows:—A family had suffered from illness: the symptoms described were those which follow the administration of arsenicum. Samples of the chintz curtains and linings used in certain rooms were sent me for examination, and were found to contain large quantities of the element in the condition of an arsenate of chromium and iron. I estimated the amount of arsenicum in the chintz and the lining, and, having been furnished with the numbers representing the quantity of the two materials used in one of the bed-rooms, I calculated the total amount of arsenicum present. Considering the element as white arsenic, there was as much in this one bed-room as would furnish 26 ounces of white arsenic.

The mischief arising from the presence of these excessive quantities of arsenicum in the green colouring-matter of these chintz curtains was believed, and stated at the time, to be due to the dissemination of fine particles of the colouring-matter in the atmosphere of the apartment, and I am not in possession of any information which would lead me to alter my belief. However, the possibility of arsenicum assuming the vaporous condition in this and similar instances was forcibly brought before me at that time. Since then one occasion has presented itself where an arsenical paint was believed to be the origin of much mischief. In such cases the most feasible explanation of the effects of such paints is that which attributes the action of the poison to some volatile compound of arsenicum. This belief is somewhat widely spread, and has been strengthened by more than one speaker at meetings of this Society, when the subject of arsenical pigments has been under discussion. The arsenicum in these instances is believed to slowly take the form of gaseous arseniuretted hydrogen. During the early part of this year I therefore commenced some experiments on an arsenical pigment, and I selected Scheele's green, in consequence of its being one of the best known and most extensively used of the pigments containing arsenicum as an essential constituent.

The two questions I proposed for experimental investigation were—

1. Does Scheele's green, at any ordinary temperature, give off white arsenic when the pigment is subjected to a current of air?
2. Does Scheele's green, mixed with linseed oil and turpentine, as in ordinary paint, and spread over a large surface, give off arseniuretted hydrogen when a current of air is passed over it?

The first set of experiments were of the following character:—A bottle of about a pint capacity was fitted with an india-rubber plug having two perforations. A piece of glass tubing passed through one of the holes, and nearly reached the bottom of the bottle. A second piece of glass tubing just passed through the second hole in the plug, and at its anterior extremity was connected by means of flexible tubing with a U-shaped bulb tube containing a solution of caustic potash for the absorption of the white arsenic, should any be eliminated. The U-tube was further connected, by means of flexible tubing, with an aspirator, which was kept working regularly and continuously for several days together. The bottle containing the

* Paper read before the Medical Society, December 15th, 1879.

pigment was first filled with clean and large fragments of broken glass, and then a quantity of the dry Scheele's green was added, the whole being afterwards thoroughly shaken, so that the surfaces of the pieces of broken glass should be thoroughly dusted with the pigment. The outlet tube from the bottle containing the pigment, and in connection with the caustic potash bulb tube, was loosely but carefully plugged with cotton-wool, so that the current of air passing through the arrangement could not carry away any of the pigment mechanically. Further, the bottle containing the Scheele's green was placed in a water oven heated by a gas flame. The temperature of the oven was not by any means very uniform, but still always much higher than that of the atmosphere at the time. I have not used a pressure regulator for the gas supply; hence the temperature of the oven would be higher during the evening than any other period in the twenty-four hours. During the day the temperature of the water oven was usually 45° to 50° C.

The experiments therefore consisted in passing a continuous stream of atmospheric air over a largely-extended surface of Scheele's green, which was kept at a temperature of about 50° C., filtering the air by means of cotton-wool, and then causing it to pass through caustic potash solution. The current of air was regular and continuous in every experiment. One experiment extended over a day, two over two days, and a fourth over seven days. In each of the experiments the potash solution was treated with excess of pure sulphuric acid, and then examined for arsenicum by means of Marsh's apparatus. In each and all of the experiments I could not obtain the faintest indication of the presence of arsenicum.

The second set of experiments were made as follows:—A bottle, having the capacity of nearly 4 pints, was furnished with a well-fitting cork, having two tubes passing through it, and arranged as in the foregoing instance. In this case it was not deemed necessary to filter the air. The bottle containing the paint was in connection with two U-tubes arranged in series, and the extreme one in turn connected with an aspirator as before. One of the bulb tubes contained a solution of caustic potash, and the other a solution of nitrate of silver. A quantity of this paint was first made by mixing Scheele's green, turpentine, and linseed oil. Thin deal shavings were then smeared with the paint, and the bottle was filled and securely corked. The whole arrangement of paint bottle, bulb tubes, and aspirator, was then connected and put in working. The air leaving the bottle was first worked with the potash solution, and afterwards by the silver nitrate solution. The bottle containing the paint was heated in one experiment only. Ordinarily the temperature of the air was about 14° C. Two experiments extended over five days, two over six days, and one over nine days. In the last of these five experiments the paint bottle was placed in a water-bath, and continuously heated without noting the temperature of the bath, because the paint bottle was but partially immersed in the water of the bath. In each of the experiments the potash solution was removed from the bulb tube, made acid by means of sulphuric acid, and then tested by means of Marsh's apparatus. Not the slightest trace of arsenicum could be detected in any instance. The silver nitrate solution was also removed and diluted with distilled water, hydrochloric acid being afterwards added in slight excess. The whole of the precipitated silver chloride was then removed by filtration; the filtrate was warmed, charged with sulphuretted hydrogen, and allowed to stand for at least twelve hours. The turbidity produced was so exceedingly slight that I conclude arsenicum was entirely absent. It would have been almost impossible to deal with the minute quantity of precipitate causing the turbidity. However, as a precaution, the very faintly turbid solution was evaporated to a small bulk in a porcelain dish, and then examined by means of Marsh's apparatus. Not the slightest indication of arsenicum was obtained in any of the experiments.

From the experiments I do not get the slightest evidence capable of furnishing an affirmative answer to either of the questions I proposed at the beginning of these experiments. However, I should very much like to see the experiments on arsenical paints carried out on a much larger scale, in chambers or vessels which would admit of the whole of the apparatus being enclosed and uniformly heated.

I may mention that, in the wet paint experiments, the potash solution was always to have absorbed a considerable quantity of what was, presumably, oxidation products of the oil and turpentine. I partially examined a little in one of the experiments, and obtained an abundant reaction characteristic of acetic acid. Other substances were also present. I cannot help thinking that some of the bad effects attributed to arsenical paints are much more likely to be due to the volatile oxidation products of the linseed oil and turpentine than to the arsenicum present in the original paint, and if we knew more of their nature the whole question would be much simpler.

In conclusion, I have to thank Mr. Henry Carr, C.E., for his courtesy and kindness in furnishing me with a considerable amount of literature and information on the subject of arsenical pigments. A very large portion of this literature is descriptions of medical cases where the symptoms of arsenical poisoning have been very marked, and the writers have expressed their belief that the mischief has been caused by the inhalation of fine particles of arsenical dust. My experiments have no reference whatever to this aspect of the question, and I wish it to be distinctly understood that I most strongly deprecate the use of arsenical pigments, in consequence of the mischief which may arise from their dust, although I have not been able to show by these experiments that they give off the vapours of white arsenic or arseniuretted hydrogen.

THE SUPPOSED ARTIFICIAL PRODUCTION OF THE DIAMOND.

THE following letter from Professor N. S. Maskelyne, F.R.S., appeared in *The Times* of Wednesday, December 31st, under the above heading.

"Sir,—I should be obliged if you would accord me space in *The Times* in order that I may answer a great number of letters and applications which have pursued me during the past few days on a subject of some little public interest, that subject being the asserted formation of diamonds by a gentleman at Glasgow.

Some ten days ago I had heard nothing whatever of the claim of Mr. Maclear, of the St. Rollox Works, Glasgow, to the artificial production of the diamond. My name, however, was already in several newspapers as that of a person in whose hands the asserted diamonds had been placed for a decision as to their true nature. Ultimately a small watch-glass, with a few microscopic crystalline particles, came into my hands for this purpose from Mr. Warrington Smyth, and subsequently a supply came to me direct from Mr. Maclear. I shall proceed to state the results I have obtained from the examination of these.

Out of the first supply I selected by far the largest particle, one about 1-50th of an inch in length, and it may be that I wasted some time in experimenting on this particle, as it might not have been an authentic example of the "manufactured diamond," since it differed in some respects from the specimens I have since received direct from Mr. Maclear.

Now, firstly, the diamond excels all substances in hardness. Secondly, its crystals belong to the cubic system, and should not, therefore, present the property of doubly refracting light. Frequently, however, from the influence of strain within the crystal, caused by enclosed gas-bubbles or other causes, diamonds are not entirely without action

on a ray of polarised light sent through them. Finally, the diamond is pure carbon, and, as such, burns entirely away when heated to a sufficiently high temperature in the air, and more vividly so burns or glows away when heated in oxygen gas.

The specimens I had to experiment upon were too light to possess appreciable weight, too small even to see unless by very good eyesight or with a lens, yet were, nevertheless, sufficiently large to answer the three questions suggested by the above properties.

A few grains of the dust, for such the substance must be termed, was placed between a plate of topaz—a cleavage face, with its fine natural polish—and a polished surface of sapphire, and the two surfaces were carefully “worked” over each other, with a view to the production of lines of abrasion from the particles between them. There was no abrasion. Ultimately the particles became bruised into a powder, but without scratching even the topaz. They are not diamond.

Secondly, some particles, more crystalline in appearance than the rest, were mounted on a glass microscope slide, and examined in the microscope with polarised light. They acted each and all powerfully in the manner of a birefringent crystal. It seemed even in one or two of them that when they lay on their broadest surface (it can scarcely be called a “crystal face”), a principal section of the section of the crystal was just slightly inclined to a flattish side of it in a manner that suggested its not being a crystal of either of the orthosymmetrical systems. Be that as it may, it is not a diamond.

Finally, I took two of these microscopic particles and exposed them to the intense heat of a table blowpipe on a bit of platinum foil. They resisted this attempt to burn them. Then, for comparison, they were placed in contact with two little particles of diamond-dust exceeding them in size, and the experiment was repeated. The result was that the diamond particles glowed and disappeared, while the little particles from Glasgow were as obstinate and unacted on as before. I had previously treated the specimens I have alluded to as the first on which I experimented, by making a similar attempt in a hard-glass tube in a stream of oxygen, and the result was the same. Hence I conclude that the substance supposed to be artificially formed diamond is not diamond and is not carbon; and I feel as confident in the results thus obtained from a few infinitesimal particles that can hardly be measured, and could only be weighed by an assay balance of the most refined delicacy, as if the experiments had been performed on crystals of appreciable size.

Not content with merely proving what these crystalline particles are not, I made an experiment to determine something about what they are.

Heated on platinum foil several times with ammonium fluoride they became visibly more minute, and a slight reddish-white incrustation was seen on the foil. At the suggestion of Dr. Flight, assistant in this department, a master in the craft of the chemical analyst, these little particles were left for the night in hydrofluoric acid in a platinum capsule. This morning they have disappeared, having become dissolved in the acid, and on evaporation there is seen a slight white incrustation, on the capsule, of the residuary fluoride. I have, therefore, no hesitation in declaring Mr. Maclear’s “diamonds,” not only not to be diamonds at all, but to consist of some crystallised silicate, possibly one resembling an augite, though it would be very rash to assert anything beyond the fact that they consist of a compound of silica, possibly of more than one such compound.

The problem of the permutation of carbon, from its ordinary opaque black condition into that in which it occurs in nature as the limpid crystal of diamond, is still unsolved. That it will be solved no scientific mind can doubt, though the conditions necessary may prove to be very difficult to fulfil. It is possible that carbon, like metallic arsenic, passes directly into the condition of vapour from that of a solid, and that the condition for its sublimation in the

form of crystals, or its cooling into crystal-diamond from the liquid state, is one involving a combination of high temperature and high pressure present in the depths of the earth’s crust, but very difficult to establish in a laboratory experiment.

I am, Sir, your obedient servant,

NEVIL STORY MASKELYNE.

Mineral Department, British Museum, Dec. 30.”

PROCEEDINGS OF SOCIETIES.

NEWCASTLE CHEMICAL SOCIETY.

General Meeting, November 27, 1879.

Mr. R. C. CLAPHAM, President, in the Chair.

THE minutes of last meeting were read and confirmed.

The following nomination was made:—Mr. J. Petrie, chemical manufacturer, Seaham Harbour.

“On Steel from Cleveland Iron,” by R. C. CLAPHAM. In the paper which I read to the Society on “Steel from Cleveland Pig-iron,” at our last meeting, I alluded especially to the patent of Messrs. Thomas and Gilchrist, upon which experiments had been made during the past year in a large way at Middlesbrough, and which appeared likely to lead to a successful issue; but, in doing so, I had no intention of overlooking the important discoveries of earlier investigators in the same direction, amongst whom I must name Mr. George J. Snelus, who, I believe, was the first to produce a piece of steel free from phosphorus, made by the Bessemer process from Cleveland iron in a basic lined vessel. Mr. Snelus began his investigations as far back as 1872, and, so far as I know, no one had up to that date used a Bessemer vessel lined with lime in any form, nor had anyone removed phosphorus by any process from iron during the Bessemer process. Mr. Snelus took out a patent in the same year for the use of lime, limestone, and magnesian limestone for lining converters, and at the same time he had discovered the curious property in lime, that when heated to a very high degree it was rendered incapable for a time of taking up water and slacking, and hence he suggested the practicability of making these materials into bricks, which process has, in more recent times, been so much improved upon by other inventors. I have pleasure in adding these remarks to the paper which I read at the last meeting.

Mr. BERKLEY read the following paper:—

“Notes on Magnesian Limestone Bricks,” by P. A. BERKLEY. In bringing before the members of the Newcastle Chemical Society a few notes upon the manufacture and qualities of the magnesian limestone bricks, used by Messrs. Thomas and Gilchrist for the dephosphorising of the Cleveland pig-iron, in Bessemer steel making, I must disclaim any special knowledge or experience upon the subject, nor would I have thought of introducing the matter before this Society, except for the earnest solicitations of our President, supported by many of the members, at our last meeting. Nor am I aware that I can say much that is new. Doubtless many present have given their attention to the subject, but have gained their knowledge from those who are financially interested in the success of the discovery; therefore, a few notes by an on-looker, and the discussion that I expect will follow, may perhaps be interesting and instructive.

I was startled when I read in the papers, a short time since, that Messrs. Bolckow, Vaughan, and Co., who had discontinued bringing Spanish hæmatite ore for steel making, had again become large importers. I was afraid some hitch had occurred in the new process; but we are now informed that the firm had determined to let the Cleveland pig-iron alone, until they procured proper con-

verters, as the present ones got frequently "gobbed" with the large amount of slag that was formed.

We generally find that inventors are seldom modest enough to keep quiet until their inventions are proved a success, and while so many wonderful patents are taken out, which have to revolutionise the world, but end in nothing, the public may be excused for being a little sceptical. But I think there is no doubt that Messrs. Thomas and Gilchrist have proved that Cleveland pig-iron can be successfully used for making Bessemer steel, and that the phosphorus can be eliminated. If these gentlemen carry out their discovery to a practical and financial success, too much honour cannot be shown to them, and also to Mr. Windsor Richards, without whose aid in the experiments it is a chance that ever the scheme would have reached its present position. Of the enormous value the discovery will be to this district it is difficult to make an estimate. Mr. Richards can have little doubt of the ultimate result of the process, when, I understand, he has recommended a new plant to be procured at a cost of £10,000.

Cleveland pig-iron has never yet been used, in any considerable way, in making steel by the Bessemer process, owing to the large quantity of phosphorus it contains. Many have been the schemes proposed to get rid of this enemy.

It has generally been considered that lime would have this effect, but when the lime was added to the molten iron, it attacked the silica lining of the converter to such an extent that it was useless.

To procure a neutral lining for the converter that would resist the heat and stand the action of the lime has been the great object of experimenters. Mr. G. J. Snelus states in his paper, read before the Iron and Steel Institute this year, that "Shortly after some experiments in 1872, I found that it was possible to make bricks out of lime or limestone, provided that when the lime was used it was crushed, quickly compressed, and fired before it had time to absorb moisture, but it required intense heat to consolidate it, particularly if the lime was very pure, while a small quantity of oxide of iron or other fusible base facilitated this. If lime was used, it was necessary to be well burnt first, otherwise the material was not porous enough to allow the small quantity of carbonic acid to escape without breaking up the brick. Finding, however, that crushed limestone bent itself with considerable facility to such a purpose as lining a converter, being plastic and binding when moist, like clay, in fact, it occurred to me to use it in this form, and to fire the vessel at a very high temperature." Thus we find Mr. G. J. Snelus, in 1872, lining his steel converter with ground limestone, in fact, making a limestone brick of the shape of the inside of his converter, from which he got most successful results. He afterwards tried the same vessel lined with ganister, when the phosphorus refused to leave the metal.

Mr. Edward Riley has also manufactured lime bricks, and in order to prevent the chemical action that water had when mixed with burnt lime, he used petroleum oil, from 5 to 10 per cent. After the bricks were compressed, they were fired in the usual way, when the petroleum burnt quietly off. The following is the composition of Mr. Riley's bricks:—

Silica	8.85 per cent.
Lime	51.80 "
Magnesia	35.35 "
Alumina	2.60 "
Peroxide of iron	1.40 "
Sulphide of calcium	0.55 "
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100.55	

I produce samples of the magnesian limestone bricks manufactured by Mr. James Tait, of Raisby Hill, made at his works at Shincliffe; also a few pieces of the limestone from which the bricks are made. Mr. Tait informs me that the limestone contains about 45 per cent of carbonate of

magnesia, and 52 per cent of carbonate of lime, that it is from the Raisby Hill Quarries, and that the bricks do not contain more than 4 or 5 per cent of silica. The great aim is to get a limestone rich in magnesia. He mentioned that the Hesledon Dene stone, not far off, contained up to 70 per cent of carbonate of magnesia, but that it was very expensive to work.*

In making the bricks the limestone is ground up into powder by the same stones that are used in grinding fire clay. The crushed limestone is put into a pug mill and mixed with water; it is then moulded in the same manner as fire bricks. The bricks are then dried upon the usual flats. Nothing whatever is mixed with the ground limestone but water, which renders it quite plastic. From the flats the bricks are stacked in a covered kiln, where they are fired until they become intensely hot (to about 3000° F.)

The bricks are burnt for 108 hours. The kilns are ill adapted for the burning of these bricks, being much too high. They are capable of containing 100,000, but only some 20,000 can be safely stacked, for owing to the enormous contraction, about 40 per cent. in bulk and 45 per cent. in weight, they fall about whilst being burnt; therefore they cannot be piled high, and if they should fall upon the silica-brick lining of the kiln they cause the latter to melt, and the limestone bricks fall into powder. In these kilns the waste of heat is most excessive, therefore to have special kilns is imperative. Another serious item of cost is the enormous loss that the burners have in broken and useless bricks—about 20 per cent.

The kiln is allowed to cool down, and the bricks are taken out and dipped into tar in order to preserve them from the atmosphere.

The broken and spoilt bricks are ground up for cement, to be used in the setting of the bricks in the lining of the converters. The ground bricks are mixed with boiling tar, and this mixture used at a proper consistency whilst it is hot. The cost of the bricks is 100s. per thousand—not less. It is well known that if limestone be overburnt it is useless for building purposes, as it will not fall. The bricks by being highly burnt become most refractory; no heat seems to have any power over them. Mr. Stead, of the firm of Pattinson and Stead, of Middlesborough, states that "the greatest heat had been tried upon these bricks from an oxyhydrogen blowpipe flame, which would melt the purest silica, but it had no effect upon them; heat that would melt platinum would not affect them, and they were so dense they would not absorb water; but if they were ground very fine they would absorb water and slake."

The following analysis of the bricks now in use has been furnished me by Messrs. Pattinson and Stead, analytical chemists, of Middlesborough:—

	No. 1. Per cent.	No. 2. Per cent.
Lime	54.85	58.32
Magnesia	27.05	28.08
Peroxide of iron	3.48	3.34
Alumina	4.62	3.11
Silica	10.06	7.18
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100.06		100.03

They state that No. 1 bricks are for stock, and keep fairly well, and No. 2 are for use at once after burning. I suppose the No. 1 bricks will keep for about three months under cover.

After our last meeting, Mr. Stuart, of Hebburn, offered to test one of the bricks. About three weeks since I sent him one, and he will doubtless give you his opinion this evening of its value for chemical furnaces. He writes me yesterday that he considered the time too short (about three weeks) to fully test the refractory quality of the

* Since the above was written it has come to the writer's notice that about 45 per cent of carbonate of magnesia is the quantity.

brick. He stated to me a few days since that he had placed the brick in the hottest position he could find, and that it did not show the slightest effect from the heat, and that, after hearing the name of the maker and the price of the bricks, he would be inclined to order a quantity sufficient to line a large revolver, where there is an intensely high temperature, and where their refractory powers will be amply tested.

When at Mr. Tait's brickworks, I picked up a few pieces of limestone that were being crushed for bricks, and Mr. Kean, of Jarrow, has made the following analysis for me:—

	Per cent.
Carbonate of lime	73.366
Carbonate of magnesia	22.930
Protoxide of iron, alumina, phosphoric acid, and manganese	2.710
Insoluble residue	1.300
Moisture	0.320
	100.626
	—
Metallic iron	1.300

Also, Mr. Kean has made the following analysis of pieces broken off one of the bricks which is on the table:—

	Per cent.
Lime	73.300
Magnesia	15.880
Peroxide of iron, alumina, phosphoric acid, and manganese	9.420
Insoluble residue	0.980
	99.580
	—
Metallic iron	3.979

These analyses are conflicting with the statement made by Mr. Tait, that the limestone contained 52 per cent of carbonate of lime and 45 per cent of carbonate of magnesia. Probably the sample I took was not a fair one; but you will notice that putting the carbonates of lime and magnesia together you get 96.296 per cent of the stone, and that in both the limestone and the bricks the insoluble residue or silica is only about 1 per cent. It has been considered that the bricks could not be made with less than 10 per cent of silica.

The metallic iron, nearly 4 per cent, is rather against the refractory quality of the brick.

The bricks that line the converter do not seem to have anything to do with the removal of the phosphorus, but are simply neutral; it is the added basic material that is the agent. If the basic additions were not put into the converter, the silica of the iron would attack the lining to such an extent that one inch would be consumed in each blow.

Mr. Richards, in substance, makes the following remarks:—Into the steel converter, lined with these lime bricks, $7\frac{1}{2}$ per cent of lime is placed. Upon this is poured 6 tons of molten iron; it is then blown with the blast, at 25 lbs. per square inch pressure, for six minutes; then there is 13 per cent of oxide of iron and lime put in among the fluid iron, one-third of oxide, and two-thirds of lime—about 20 per cent of the whole charge. The blast is then put on until the carbon lines leave the spectrum and the flame drops. It is necessary to continue the blow for two or three minutes, in order to get clear of the phosphorus.

It has been proved that the phosphorus will not leave the iron, unless the slag contains under 20 per cent of silica—hence the value of lime bricks and the addition of lime that is put into the converter. There is a loss in iron of 17 per cent, and in the working of the pure hæmatite there is a loss of 15 per cent of iron.

These limestone brick linings have stood nine blows, whilst the ganister linings have stood ten or eleven blows

with hæmatite pig. There has been an idea generally prevailing that phosphorus could not be eliminated from iron at the high temperature of the converter (about 6000° F.). This has been proved to be quite erroneous by the recent experiments of Mr. Snelus and Mr. Thomas.

The PRESIDENT—We are all indebted to Mr. Berkley for his very useful and thoroughly practical paper. I shall be glad to hear the remarks which any of the members have to make on the subject.

Mr. STUART—I have tested one of the bricks in various ways. I chipped a piece off, and this piece was exposed to a damp atmosphere for three weeks, without being at all acted on; it also refused to slake when immersed in water. The rest of the brick was put into a lime-kiln—the hottest we have. At the end of three weeks a portion of it broke off, but the brick was otherwise quite unacted on. Nor was it fused even by the oxyhydrogen flame.

The PRESIDENT—You intend to try it in a revolver?

Mr. STUART—Yes, certainly.

Mr. SWAN—Are these really magnesia bricks, or was the term a printer's error?

Mr. BERKLEY—It was a mistake; it should have been "magnesian limestone bricks."

Mr. SWAN—I was very glad to hear Mr. Snelus's name was mentioned, for it would have been a pity to allow Mr. Clapham's contribution to go forth without Mr. Snelus's name appearing as the originator of the method which has been developed by Messrs. Thomas and Gilchrist. It was as far back as 1872 that Mr. Snelus observed that the phosphorus could be eliminated in the converter by the use of a lime lining.

Mr. TATTERS—At the last meeting my friend Mr. Lyte's name was mentioned. I asked him to come down tonight to give us some information on the subject, and I have now much pleasure in introducing him to the meeting.

Mr. LYTE—I much regret I had not the advantage of being present during the reading of Mr. Clapham's interesting paper, as the subject is one which has rather engaged my attention. With regard to the durability of these bricks, I would venture to suggest a possible cause of destruction which may exist, where, as seems generally to be the case, dolomite has been used in their manufacture. We are all aware that in the scale of elective affinities CaO stands superior to MgO. We have but little experience as to whether or no the order of these elective affinities is maintained at such high temperatures as those requisite for the fusion of silicates, or at the temperature of a Bessemer converter; but, granting for a moment that such may be the case, it might be possibly feasible to profit by that circumstance to attain a more efficient protection for the lining, without departure from the great principles of a basic lining and a basic slag. If, instead of using dolomite in the manufacture of these bricks, they were made from Greek-stone, which is nearly pure magnesium carbonate, and if, together with the charge, enough of the mixture of lime and "Blue Billy" were added, to insure the presence of a liberal excess of lime, the corrosion of the lining might probably be—to say the least—considerably diminished. With bricks made from dolomite, containing, as it does, 75 per cent or even more of calcium carbonate, the lime portion of the bricks would be attacked simultaneously with the rest of the lime present, and as it dissolved out the remaining magnesia would be left spongy, and liable to break down and be destroyed. I may here mention that, at works near Paris, with which I was somewhat connected, where barium hydroxide was manufactured on a large scale for sugar-refiners, under the patent of M. Tessié du Motay, one of the steps of the process was the production of barium sulphide. Now, for a long time, no fettling could be found capable of resisting the fluxing action of barium compounds at the high temperatures required. M. Tessié du Motay, with that fecundity of inventive genius for which he has always been so justly celebrated, soon,

however, overcame the difficulty. He determined to line his furnaces with a basic material, and for that purpose selected magnesia. This occurred just after the Franco-Prussian war. Greek-stone was calcined at a very high temperature, becoming thereby dense but cracked in all directions. The calcined magnesia thus produced was ground, and the powder then wetted and moulded into bricks, with just enough wet precipitated magnesium hydroxide to give it coherence, and these bricks were once more strongly burnt. A furnace lined with the very best fire-brick (or bauxite) would not have lasted for as many days as furnaces so lined lasted months, on the one necessary condition, that not one bit of ordinary brick, fire-brick, furnace slag, or any acid, silicious, or aluminous material, should be permitted to come in contact with the basic lining. I have experimented rather extensively on the manufacture of these magnesia bricks, and have found that some further solidity and durability is obtainable by making them up, not with water, but with a solution of magnesium chloride, or of some other suitable magnesium salt. Chlorine, principally as HCl, is volatilised, and a firmer and harder brick is thus obtained, some magnesia being thus finally formed in the body of each brick, and binding the material more effectually together. These facts are suggestive as to the probable advantages derivable from the employment of magnesia bricks as a lining for balling furnaces, and for some similar purposes where alkalies, metallic oxides, or basic silicates or aluminates, have to be dealt with at high temperatures. It would be impossible to attempt to manufacture bricks in this manner if dolomite were employed. This material, if calcined ever so completely, could never be pulverised and then made up with water, for the lime would under these circumstances infallibly slake and break down. Magnesia, while it stands in the order of its elective affinities inferior to lime as respects acids, stands likewise, as is well known, inferior to lime in its affinity for water, and the particular advantage it possesses, making it specially adapted for the manufacture of well-formed bricks (hard, solid, and with sharp clean edges), is that once calcined it refuses to take up water, acting merely like so much sand, uncontractile and unalterable by heat or moisture, while it can be cemented together with ease, as already stated.

The PRESIDENT—I am sure we are all very glad to hear the remarks of Mr. Lyte, who has given so much attention to the subject of magnesia bricks. I can personally testify to the purity of Greek-stone, for we used it here some years ago for the manufacture of Epsom salts, and it was almost pure carbonate of magnesia.

Mr. STUART—These bricks would be much more suitable than acid bricks for the lining of revolvers. The eye of the revolver next the heating furnace melts away very rapidly, from the action of the alkali on the silica at the high temperature of the furnace; in such a situation these basic bricks would probably last for an indefinite time. I think it will be well worth the while of alkali makers to go into the question. I intend to try the experiment of lining a revolver with these bricks, and I shall be glad to communicate the results to the Society.

Mr. PROCTOR—Can you give us an idea of the value of the pure magnesia bricks?

Mr. LYTE—I cannot at present. They cost us a good deal, for the Greek-stone then was about a thousand francs per ton. At present, however, it can be got much cheaper. I happen to be connected with the only remaining deposit—that in the Isle of Eubœa, and I have brought some over to try it. I shall be very happy to present a couple of tons of it to anyone who will try it in revolvers.

Mr. PROCTOR—I suppose there is no probability of them being made for 100s. per ton at present?

Mr. LYTE—No; I should say not, certainly.

The PRESIDENT—It is not so much the original cost of a brick as its durability which determines its cheapness.

Mr. LYTE—These bricks on the table are very dirty and deformed, and cracked in many places. Our bricks were of a pure white colour, and had an almost perfect form.

Mr. SWAN—Were they as dense and hard as these?

Mr. LYTE—About the same, I should say; and they did not exhibit traces of fusion, which these do to some extent.

Mr. STUART—If Mr. Tait could be induced to accept Mr. Lyte's offer, and make the magnesite into bricks, I should be very happy to try them. About how much precipitated magnesia was used in the moulding?

Mr. LYTE—About 5 or 6 per cent.; just enough to give them coherence. It was got by precipitating crude magnesia sulphate from Stassfurt by caustic soda.

Mr. PROCTOR—Would not some of these basic materials do for lime-light cylinders?

Mr. LYTE—It was with that object that M. du Motay first experimented with magnesia. He afterwards substituted other substances, notably zirconia; but he used magnesia a good deal, principally because it had so little tendency to fly off. He made his cylinders very much in the same way as the bricks. The hydrate was found to be essential to bind the magnesia together.

Mr. SWAN—I made an attempt to substitute magnesia for lime, but was not very successful. I had the cylinders turned out of the native magnesite, and then burnt; but they crumbled to pieces in the most beautifully symmetrical way soon after being taken out of the fire.

Mr. BERKLEY—If the magnesian limestone bricks are well burnt, they hold together very well. Their appearance has nothing to do with the result we have to get out of them. The dirt is probably due to the fact of the kilns not being suited for them, so that they have to remain very long in the kiln exposed to the dust and smoke. And their form too is not very material, for, as I have said, the waste broken bricks are made into a cement in which the bricks are bedded, and which is worked in among them until the interior of the converter is one smooth lining. With proper kilns, however, there should be no difficulty in making the bricks with a very much better appearance, and I do not see why they should not be made as cheaply as fire bricks.

Mr. LYTE—I do not allude to the colour of the bricks so much as to their shape. They seem to have become very distorted and cracked, which our bricks did not do. Our material had undergone the whole of its contraction before being moulded; this of course could not be the case with the bricks containing lime.

Mr. BEER—How have the furnaces lined with these bricks lasted?

Mr. BERKLEY—About nine blows, against ten or eleven with silica bricks and hematite pig.

Mr. STUART—I think with Mr. Berkley that Mr. Lyte exaggerates the importance of the shape of the bricks. If they will only stand the fire, there will be no difficulty in using them.

The PRESIDENT—Mr. Watson has put a piece of brick into my hands, which is much better in appearance than those on the table, and probably approaches nearer to Mr. Lyte's bricks.

Mr. WATSON—I got it at Messrs. Bolckow and Vaughan's. It has been exposed for a month to the atmosphere, and shows no change.

The PRESIDENT—The question is being rapidly raised into prominence, and will become still more important in the future, if Messrs. Thomas and Gilchrist succeed. We must receive the paper and remarks of Mr. Berkley with peculiar interest, for we know he has paid much attention to the manufacture of iron in all its stages; he has been working at it with unwearied industry ever since I first knew him, more than twenty years ago. At the same time I must express our indebtedness to Mr. Lyte for the information he has given us on pure magnesia bricks. It is quite possible that Greek-stone might be imported here, and bricks manufactured from it at a price which would make them very valuable.

"On the Noxious Action of Acid Vapours on Vegetation." By Dr. G. LUNGE.

NOTICES OF BOOKS.

The Sizing of Cotton Goods; and the Causes and Prevention of Mildew. By W. THOMSON, F.R.S.E., F.I.C., F.C.S. Second Edition. Manchester: John Heywood.

OUR readers will doubtless remember the first appearance of this work, and if so, will feel little surprise that a second edition should so speedily be called for. Sizing is undoubtedly one of the questions of the day, and all who take upon themselves to discuss it in public should at least make themselves acquainted with its true nature, with its dangers and advantages, and with the arguments used for its practice and continuance. Too many journalists, statesmen, political economists, and lawyers have omitted this preliminary step, and have consequently come to somewhat rash conclusions, which have been eagerly circulated in foreign countries, with comments highly unfavourable to British commercial morality. It is remarkable that an eminent continental journal, whilst from time to time indulging in strictures upon heavily-sized cotton goods, has till very lately defended the practice of weighting silks! It is neither our place nor our wish to defend either of these practices, but in fairness we may give an abstract of the arguments advanced by our author to show the necessity of the practice. It can scarcely be called a fraud if the merchant and the consumer buy it, as is asserted, knowingly and by preference, paying a price correspondingly less than what would be charged for "pure" cottons. Lancashire manufacturers must produce what the natives of India, China, and other countries demand, and not what is thought would be best for them, and if they object, the people of some other nation would doubtless step in. "Heavily-sized cloth is undoubtedly better adapted for many purposes for which the natives use it than a pure cotton fabric, besides being less costly." Manufacturers would, as a rule, rather make pure cloth, but the demand for that commodity is so limited that they are obliged to adapt themselves to the requirements of the market.

All the arguments are, of course, liable to be called in question. There are outsiders who would like proof that heavily-sized calicoes are preferred by any large class of consumers to a pure fabric, and that the former are really lower in price in a fair proportion to the amount of foreign matter present. It would be well if on these points some satisfactory evidence could be laid before the public.

We come to the subject of mildew. Every one has heard occasionally of bales of cotton, on arriving in India and China, being found mildewed, and sold in consequence at a loss, and the question arises how far is this evil due to heavy-sizing? Here, we think the representatives of the cotton trade are evidently in the right. A "pure" calico, where the warp is merely dressed with the small proportion of size necessary for weaving, will, if exposed to moisture, a sufficient temperature, and to the spores of certain low forms of vegetable life, become covered with minute fungoid growths. On the other hand, a heavily-sized cloth, if the sizing contains a due proportion of some antiseptic matter, such as the chloride of zinc, will remain free from mildew. Some of the substances most largely used in sizing, such as China clay, have no tendency to foster mildew. Others, such as the chloride of magnesium, have this undesirable property in virtue of the deliquescent nature, which keeps the tissues damp. The more chloride of magnesium is used, the greater is the need for chloride of zinc. Hence it is unfortunate and reprehensible that the chloride of magnesium should be sold under the name of "antiseptic." "Septic" would be much more appropriate.

Whatever conclusion is come to on the abstract morality of heavy-sizing, it will, we think, be granted that if the operation is to be performed at all it should be done on scientific principles, by which means a variety of accidents, losses, and causes of dispute between manufacturers and their customers will be prevented.

The author, in this edition, has treated of the propagation and growth of mildew to some extent, and has entered upon the difficult and delicate question of determining whether a stain upon cloth is due to fungoid growths or to other causes.

Sizing and Mildew in Cotton Goods. By G. E. DAVIS, F.G.S., F.I.C., C. DREYFUS, Ph.D., F.I.C., and P. HOLLAND, F.C.S. Manchester: Palmer and Howe. London: Simpkin, Marshall, and Co.

THIS work has reached us almost simultaneously with the kindred treatise of Mr. Thomson. On a great majority of the points concerned both authorities are substantially agreed. On the morality of sizing, on the nature and properties of the various articles used in the process, and on the mechanical appliances required there is little difference. Messrs. Davis, Dreyfus, and Holland have, however, entered more deeply than has Mr. Thomson into the nature and characteristics of microscopic fungi, to which they have devoted four chapters of their book. In this department they have secured the valuable assistance of the well-known mycologist, Mr. Worthington Smith, and of the Rev. J. E. Vize.

The appearance of the work before us may, perhaps, be traced to what is now known as the "Manchester Goods Case," in which Mr. Thomson appeared on behalf of the plaintiff and Messrs. Davis and Dreyfus for the defendants.

It appears that a certain quantity of grey shirtings were manufactured by Messrs. Langton and Riley, and sold by them to Mr. Provand for exportation to China. On arrival there the goods were found discoloured with brick-red stains, which depreciated their value. The plaintiff, therefore, brought this action against Messrs. Langton and Riley contending that these stains were mildew, produced by an improper mixture of the size, or as the lawyers preferred to say, "to the admixture of deleterious chemicals." The defendants, on the other hand, maintained that the mischief arose not from fungi, or from any fault or defect in the sizing, but from colouring matters evolved from the tar-cloths used in packing. The former view was upheld by Mr. Thomson and Mr. C. E. Groves, and the latter by Prof. Roscoe, Dr. Carnelly, Dr. Dreyfus, and Mr. Davis. Ultimately a verdict was given for the plaintiff, but as the defendants applied for a new trial, which has not yet taken place, we cannot express any opinion as to the cause of the discolouration.

The scientific evidence contains, however, some noteworthy points. Mr. Thomson gave the very important statement that he had "actually transplanted the fungus, having scraped some of the colouring matter and placed it on flour paste, when in a few days a red fungus made its appearance precisely similar to the one in the cloth, and gradually covered the whole paste from the top to the bottom." He admitted, however, that the supposed fungus had "no form from which to make a drawing." He also added that "coal-tar contains colouring matter but not tarpaulin tar." Mr. Groves also admitted that he could detect no spores under the microscope. Dr. Paul is reported to have deposed that wood-tar, which is used in the manufacture of tar-cloth, "contains little or no colouring matter."

On the other hand, Prof. Roscoe stated that the starch-granules and the warp of the cloth both remain intact; whilst if mildew had been present they would have been eaten away. Dr. Carnelly declared that no fungus-like form could be seen on microscopic examination. Mr. Davis deposed that though red mildews existed they had definite forms; that wood-tar contains certain well-known colouring matters; and that he had failed to propagate the discolouration upon damp cloth in a hot place. Lastly, Dr. Dreyfus produced printed calico dyed with wood-tar colours, and swore to having seen red stains on bleached goods before where tarpaulin only had been used.

The discrepancies and contradictions in the scientific evidence are not a pleasant subject to consider. In their

final chapter the authors, without throwing out imputations against anyone, make some very pertinent remarks on scientific witnesses and scientific evidence. They point out the utter incapacity for any ordinary jury, whether common or special, to decide such questions as the following:

Were the red stains mildew or not?

Have fungi a definite form?

Can colours be produced from wood-tar?

They submit that in this case the jury should have been composed of three merchants, three manufacturers, three botanists, and three chemists. After quoting from Dr. Angus Smith's 12th and 13th Annual Reports a most important passage on the position of scientific witnesses, they give a suggestion of their own which to us seems worthy of very serious attention. They propose that the plaintiffs' and defendants' experts might meet in committee, having first made their analyses and other investigations apart, and compare results, "each pointing out to the other any weak points in the mode of procedure or line of argument of his *confrère*."

We must not forget to mention that one of the foreign journals which have commented most severely in reference to this case upon the conduct of English cotton manufacturers, still does not scruple to insert advertisements of the chloride of magnesium as a sizing ingredient!

It is no disparagement to Mr. Thomson's treatise to say that there is decidedly room for the work of Mr. Davis and his colleagues. Both the chemist and the manufacturer, if desirous of thoroughly understanding the sizing question in all its bearings, should possess both these books.

Annals of Chemical Medicine, including the Application of Chemistry to Pathology, Physiology, Therapeutics, &c.
Vol. I. Edited by J. L. W. THUDICHUM, M.D.
London: Longmans, 1879.

THE position which Dr. Thudichum has won for himself as one of our leading physiological chemists renders him particularly fitted for conducting a periodical like the present, which purposes from time to time to lay before the medical and chemical professions accounts more or less condensed of the latest researches carried out in physiological, pathological, chemical, and pharmaceutical laboratories in Great Britain and elsewhere. In the present volume we have some twenty-three papers, most of which were read by Dr. Thudichum himself before the Pathological Institute, many of them being more or less summarised. A few of the subjects treated of will show the scope of this new candidate for public favour—the colouring matter of shells, the organic acids of the brain, a very interesting biographical sketch of R. J. Mayer, bile pigments, the urine alkaloids, antiseptic surgery, &c. Dr. Thudichum's style at times approaches the pedantic, and we feel obliged to quarrel with him for trying to render the confusion which already exists in our system of chemical nomenclature and spelling worse confounded by introducing such words as baryum, oxyde, hydrochlor (HCl), hydrothion (H₂S), molecule for molecule (which looks like a misprint), and last but not least, to fell for to precipitate. We suppose he would describe the process of forming the *arbor saturni* as *felling* a lead tree. There are two excellent name and subject indexes, and the book is well and clearly printed. Having so persistently regretted the ignorance of chemistry which is prevalent amongst our medical men, we are glad to welcome a periodical which will do much to dispel this lamentable want of knowledge.

South London School of Pharmacy.—At the Annual Dinner, on the 19th December, at the Horns Assembly Rooms, Dr. Julius Pollock presented the Medals and Certificates to the following successful Candidates:—Senior Chemistry Medal, Mr. Eaton; Certificate, Mr. Harburn. Junior Chemistry, Medal, Mr. Banks; Certificate, Mr. Littlefield. Materia Medica, Medal, Mr. Littlefield; Certificate, Mr. Slices. Botany, Medal, Mr. Parkes; Certificates, Messrs. Banks and Grover. Pharmacy, Medal, Mr. Webb; Certificates, Messrs. Fildes and Beech.

CORRESPONDENCE.

WATER OF THE CARISBROOKE CASTLE WELL.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xl., p. 310, appears a paper read before the Manchester Literary and Philosophical Society, on the "Water of the Carisbrooke Castle Well."

I published a full analysis of this water in January, 1878. (*Sanitary Record*, vol. viii., p. 18), which was afterwards reprinted in *Seaside Water*. This analysis gives the missing figures which Mr. Grimshaw asks for, namely—

		Grain per Gallon.
Nitrogen as ammonia	0.0030
" albuminoid ammonia	0.0114
" nitrates	0.7620
" nitrites	traces

My sample was of course examined while fresh, and had therefore no smell of sulphuretted hydrogen; but I found only 0.18 grain per gallon of volatile matter, as against 19.60 found by Mr. Grimshaw. Assuming the latter figure to be correct as to the sample referred to, that alone should be sufficient, not only to condemn the water, but to render the presence of sulphuretted hydrogen after the sample had been kept for five months extremely probable. Such an increase in volatile matter is, however, curious when the character of the well is considered.—I am, &c.,

G. W. WIGNER.

Laboratory, 79, Great Tower Street, E.C.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 23, December 8, 1879.

Remarks on the Saccharoses.—M. Berthelot.—The substance recently discovered by M. Peligot offers a striking resemblance to trichalose, which, however, is not quite its equal in stability. The author considers that his theory of the saccharoses distinctly foretold the synthesis of this class of compounds.

Relation between the Heat of Solution and the Heat of Dilution in Complex Solvents.—M. Berthelot.—Suppose two complex solvents at one and the same temperature, and dissolve in each of them a third body, which is in a fixed proportion with the substance already mixed with the water. The difference between the two heats of solution is equal to the difference between the two heats of dilution, observable on adding to the concentrated liquid before and after having dissolved in it the third body, the water necessary to bring it to the state of a diluted liquid: $D' - D = \Delta - \Delta'$. Thus, if we dissolve a given weight of cuprous chloride in an aqueous solution of hydrochloric acid which disengages D, and then dilute the liquid with a known weight of water which disengages Δ ; or if we dilute with the same weight of water the same acid solution, which disengages Δ' , and then dissolve in the dilute liquid the given weight of cuprous chloride, which liberates D', then the initial and final state being the same— $D + \Delta = \Delta' + D'$.

Coloured Rings produced on the Surface of Mercury.—M. Adr. Guebbard.—If the grey pellicle formed on the surface of impure mercury is carefully swept away, a surface is obtained the brightness of which is, for the moment, as great, and the mobility less, than those of

pure mercury. The moisture of the breath produces upon such a surface magnificent systems of coloured bands. More striking results are obtained on substituting for the breath the vapours of certain volatile substances, such as the common essence of petroleum. With collodion diluted with ether the author obtains pellicles which may be detached from the mercury after having regulated the colour and the thickness at pleasure.

Protochloride of Copper.—M. Berthelot.—The heat of solution of this salt in dilute hydrochloric acid varies according to the degree of dilution from -0.4 to -4.75 . The formation of anhydrous Cu_2Cl liberates $+35.6$.

Reply to the Questions on the Origin of Chlorophyll, contained in the last Notice, by M. Chevreul (*Comptes Rendus*, p. 917).—A. Trécul.—This paper is rather of a botanical than of a chemical nature.

Reply to M. Trécul and M. Chevreul on Crystalline Chlorophyll.—M. Arm. Gautier.—The author considers that the function of chlorophyll is not, as commonly supposed, to decompose carbonic acid under the influence of light. It appears to be merely a secondary agent destined to absorb and extinguish the red and yellow light-rays. After being thus modified in the leaf, the luminous *vis viva*, transformed into heat and chemical action, is utilised by the protoplasm of the chlorophyll globules to produce the reductions peculiar to the green parts of plants.

An Alcoholic Fermentation.—M. D. Cochin.—A reply to M. Berthelot's remarks in the sitting of the Academy on November 17th.

Chemiker Zeitung.
No. 49, 1879.

Determination of Potassium as Platino-potassic Chloride.—Dr. H. Precht.—The author treats, in the first place, of the preparation of platinum chloride from residues. The concentrated alcoholic solutions are reduced, at a boiling temperature, by a mixture of soda solution and glycerin. Platino-potassic chloride is also completely reduced if gradually added to the boiling mixture, and if sufficient sodium carbonate is present. The precipitate of platinum-black is washed by decantation in a porcelain capsule till all sulphuric acid and potassium are eliminated, dried, ignited, and digested at a gentle heat in hydrochloric acid, to which sufficient nitric acid is gradually added. The solution is evaporated down at a gentle heat; the platinum chloride is converted into platinum chloride by the addition of fuming hydrochloric acid and a little nitric acid; the excess of the latter is expelled by the repeated addition of water and hydrochloric acid, and evaporation. The presence of platinum chloride is detrimental for the determination of potassium, rendering the results too high, whilst nitric acid occasions an error in the contrary direction. Traces of iridium have little effect on the accuracy of the analysis. Precht gives the solubility of platino-potassic chloride in alcohol as 1 part in 42,600 absolute, 1 part in 37,300 at 96 per cent, and 1 in 26,400 at 80 per cent. Platino-sodic chloride is soluble in boiling water in almost every proportion. Platino-baric chloride has the important property of being more or less completely decomposed into platinum chloride and barium chloride. Precht conducts the determination of potassium as follows:—The sulphuric acid is removed by barium chloride in a solution containing 0.5 of hydrochloric acid to 1 of the salt. The clear liquid should contain neither barium chloride nor sulphuric acid. Traces of the latter may be removed in the measuring vessel by finely pulverised barium chloride. Small quantities of sulphuric acid are admissible if the solution with platinum chloride is not evaporated quite to dryness. In acid solutions the objection to the removal of sulphuric acid by means of barium chloride—*i.e.*, that alkalies are carried down along with the barium sulphate—has little founda-

tion. In neutral solutions so much potassium sulphate is thrown down that an error of 1 per cent may be occasioned. In evaporating down with platinum chloride care should be taken that large crystals of platino-sodic chloride are not formed, which would interfere with washing. The latter process is best performed with hot alcohol, there being no danger of the reduction of PtCl_4 . A mixture of alcohol and ether is not to be recommended, nor an addition of glycerin. For the determination of small quantities of potassium chloride along with an excess of sodium chloride, Precht evaporates 10 to 100 grms. along with a solution of platino-sodic chloride of known strength. The potassic salts are thus thrown down, the excess of the sodium compound is washed away with absolute alcohol, the platinum reduced on the filter, and weighed.—*Zeitsch. Anal. Chemie*, xviii., 509.

*Verhandlungen des Vereins zur Beforderung des
Gewerbflusses.* November, 1879.

At the meeting of the Society Dr. H. Grothe read a paper on the "International Exhibition of Arnheim." It is remarked that the glass manufacture has recently made considerable progress in Holland.

M. Bernstein read a paper on the "Methods of Testing the Inflammability of Crude Petroleum." He remarked that at present Germany is the market for such oils as cannot be legally sold in other countries. He described and criticised the various apparatus in use for the determination of the flashing-point.

Composition of the "Weldon Mud," and some Similar Compositions.—Jul. Post.—The reproduction of this interesting paper is specially prohibited.

Moniteur Scientifique, Quesneville.
December, 1879.

Sittings of the Chemical Committee of the Industrial Society of Mulhouse.—On April 9th, the Secretary read a memoir by M. G. Engel, from which it appears that the silicious tubes of infusoria have a strong analogy with cellulose as regards their behaviour with mordants and colouring matters.

On June 11th, M. Camille Koechlin complained of the contamination of cœrulein with gallein. Cœrulein shades topped with gallein gain in body but lose in brightness and after steaming, take a greivish tone.

On September 10th, M. Colin gave a preliminary account of a new soluble black colouring matter, derived from aniline, and capable of being fixed on calico by a passage through bichromate. M. Endler proposed to modify the manganese-bronze process by a subsequent treatment with the chromate of ammonia instead of caustic soda. He ascribes the "greening" of aniline-blacks not merely to the action of acid fumes, but to the decomposition of amylaceous matter present.

On October 8th, M. Prud'homme explained the action of the bisulphite of soda in cœrulein shades. It appears not to behave merely as a reducer or a solvent, but to form with the cœrulein a definite compound.

MEETINGS FOR THE WEEK.

- MONDAY, 5th.—London Institution, 5.
— Medical, 8.30
TUESDAY, 6th.—Royal Institution, 3. Juvenile Lecture "Water and Air," Prof. Tyndall.
— Zoological 8.30.
WEDNESDAY, 7th.—Geological, 8.
THURSDAY, 8th.—Royal, 8.30.
— Royal Society Club, 6.30
— Royal Institution, 3. Juvenile Lecture "Water and Air," Prof Tyndall.
— London Institution, 7.
FRIDAY, 9th.—Astronomical, 8.
— Quekett, 8.

BURNDEN CHEMICAL WORKS, AND OTHER PREMISES, AT BOLTON, LANCASHIRE.

VALUABLE LEASEHOLD LAND, BUILDINGS, WORKING PLANT, MACHINERY AND APPARATUS FOR MANUFACTURING SODA ASH, SULPHURIC ACID, MURIATIC ACID, BLEACHING LIQUOR, SULPHATE OF COPPER AND BRONZE ASH; ALSO VALUABLE DYE WORKS, AND FOURTEEN MESSUAGES OR DWELLING-HOUSES.

LOMAX, SONS, & MILLS, will **SELL** by PUBLIC ACUTION (by order of the Mortgagees), at the THATCHED HOUSE HOTEL, New Market Place, in the City of Manchester, on Tuesday, the 20th day of January, 1880, at Two for Three o'clock in the afternoon (unless previously disposed of by private contract), subject to such conditions as will be then and there produced,—

ALL THOSE VERY VALUABLE AND EXTENSIVE CHEMICAL WORKS, situate in Burnden Street, Manchester Road, Bolton, in close proximity to the Lancashire and Yorkshire Railway, known as the

BURNDEN CHEMICAL WORKS,

consisting of Ore-breaking Shed, Pyrites and Sulphur Burning Houses, Sulphate of Copper Sheds, Salt Cake Shed and Store, Bronze Ash House, Chemical Shed, Rectifying and Cooling Houses, Loading Shed, Ash Sheds, Cooper's Shop, Ash Packing Place, Joiners' and Mechanics' Shops, Engine and Boiler-houses, Wheelwright's Shop, Smithy, Nitre Store, Stabling for 17 Horses, Provender Place, Hay and Straw Stores, Private and General Offices, Laboratory, Machine-house, Chimneys, and other erections; together with the Steam-boilers, Steam-engines, Millwright's Work, Steam, Gas, and Water Piping belonging thereto; and also the whole of the Working Plant, Machinery, and Apparatus set up, affixed, and belonging to the several departments of the said works and premises, and capable of manufacturing monthly upwards of 1000 tons of Soda Ash, Sulphuric Acid, Muriatic Acid, Bleaching Liquor, Sulphate of Copper, and Bronze Ash. The Working Plant consists of Black Ash Furnace, four Cast Iron Lixivating Vats, Steam Jets, Liquor Wells, Liquor Settler, Evaporating Pan, Salting Down Pan, Soda Ash Finishing Oven, pair of Ash Crushers, and Rotating Sieve; 14 Pyrites Burners; two Nitre Ovens, Lead Concentrating Tower, three Vitriol Chambers and three Nitre Recovery Towers, Air Engine by Daglish and Co.; four Acid Eggs, four Liquor Cisterns, Boiling-down Pan, eleven Sulphur Burners, Vitriol Wash Tower, five Vitriol Chambers, and two cisterns, three Boiling-down Pans, Sixty Rectifying Furnaces, Salt-cake Pot and Roaster, eight Stone Acid Cisterns, and two Stone Condensing Towers, seven Stone Chlorine Stills, five Stone Bleaching Vessels, with Gearing and Agitators, Liquor Well, Stone Acid Cisterns, Lead and Iron Chemical Tanks, Feed Pump, Ore Grinding Mill, Riddling Machine, Copper Calcining Furnace, Wagon Hoist, Tramways and Turntable, Wash Tower, six Extracting Vats, Liquor Settler, two Precipitating Tanks, Copper Vat, precipitate Roasting Furnace, three Evaporating Pans, thirteen Lead Crystallising Pans, Liquor Well, Steam Pump, Drying and Sampling Benches, two Boiling-down Pots and Furnaces, Drying Furnace, Boiling-down Pan and Furnace, Drainer and Well, Mortar Mill, Chaff-cutting Machine, Ten-ton Wagon Weighing Machine, and Office Fixtures; together also with the **THREE PLOTS OF LAND**, forming the site thereof, and occupied therewith, and containing altogether 24,318 square yards, or thereabouts.

The Premises are Leasehold, and are held for the residue of three several terms of 990 years, 999 years, and 990 years, subject to three several ground rents amounting in the aggregate to £120.

The Buildings, which are brick-built and slated (the sheds being erected on brick pillars), are excellently adapted for the purposes of the business, a large sum of money having been expended in their construction (a considerable portion of such sum within the last few years), and are most eligibly situated, the Lancashire and Yorkshire Railway being immediately contiguous to the Works. Gas and Water Mains are laid throughout, and there is a never-failing supply of water for all purposes. Immediate possession can be had.

Also those valuable PREMISES, situate in Burnden-fold aforesaid (adjoining the last-described premises), formerly used as a DYE WORKS, with the STEAM BOILER, STEAM ENGINE, SHAFTING, GEARING, PIPING, and FIXTURES thereto belonging, which, with the ELEVEN COTTAGES adjoining, being Nos. 3, 5, and 7, Haslam-street, 11, 13, 15, and 17, and 9, 25, 27, and 29, Burnden-fold aforesaid, and the site whereof contains 1302 square yards, are held for residue of term 999 years, at yearly rent of £7 14s. 1½d., and are subject to a lease for five years, at the yearly rent of £70, and will be sold subject to such lease.

Also all those TWO MESSUAGES, or DWELLING-HOUSES, and CELLAR, with the YARD, OUTBUILDINGS, and APPURTENANCES thereto belonging, situate and being Nos. 203, 211, and 213, Manchester-road, and 6 and 8 at the back, and known as Back Burnden.

Site, 248½ square yards. Term, 999 years. Apportioned yearly rent, £2 2s. 10d.

The whole of the foregoing properties will, in the first instance, be offered for sale in One Lot, and if not so disposed of will then be offered in Two Lots, viz.:—

- Lot 1. Land, Chemical Works, Dye Works, and Dwelling-houses.
- Lot 2. The whole of the Plant and Machinery.

Full particulars of which will be given on application to Messrs. P. and J. KEVAN, Accountants, 12, Acresfield, Bolton; the AUCTIONEERS, 15, Wood-street, Bolton; Messrs. RICHARDSON and MARSHALL, Solicitors, 18, Wood-street, Bolton; or to Messrs. HOLDEN and HOLDEN, Solicitors, Bolton.

The Prestolee Alkali Works, Farnworth, near Bolton, Lancashire fitted with costly plant, machinery, and apparatus for the manufacture of soda-ash, bleaching-powder and liquor, and sulphuric acid, in complete working order. With possession.

MESSRS. FULLER, HORSEY, SONS, and CO., are instructed to **SELL** by AUCTION, at the Palatine Hotel, Manchester, on Friday, January 16th, at 3 precisely, in one lot (unless an acceptable offer be previously made by private contract), the PRESTOLEE ALKALI WORKS, a freehold property, having a superficial area of 95,741 square yards of land, subject to chief rents amounting to £407 13s. 9d. per annum, with the buildings, plant, machinery, and apparatus erected thereon, capable of manufacturing monthly a product exceeding in the aggregate 2000 tons of soda-ash, bleaching-powder and liquor (by Weldon's patent process), and sulphuric acid, also caustic soda and muriatic acid. The amount expended in the construction of these works has been very large. A valuation was made with great care in 1874 by Messrs. Holmes and Son, the well-known valuers, of Manchester; their estimate then amounted to £99,900, and since that time a sum exceeding £10,000 has been expended. The whole of the works and plant have been well kept, and are conveniently arranged for working. The Bury, Bolton, and Manchester Canal, which forms one boundary of the property, affords facility for economical water carriage, and the Lancashire and Yorkshire Railway is within a very short distance. There is a plentiful supply of water for all manufacturing purposes, free of cost, from the river Croal, and coals are raised from pits in the immediate neighbourhood. Pyrites, salt, lime, and limestone are all brought by boats direct to the wharf on the canal. The works may be economically worked with a small capital, as arrangements may be made with the vendors for a very considerable portion of the purchase-money to remain up on mortgage. Large profits have been realised in the past, and the high reputation of the Prestolee manufactures in the market will ensure to an energetic man, even at the existing low prices, an ample return for his capital invested, and possession of works not surpassed for completeness or compactness by any in the kingdom. May be viewed till the sale. Printed particulars may be had at the works; at the Palatine and Queen's Hotels, Manchester; of Messrs. Wake-man and Bleeck, Solicitors, Warminster; of Messrs. Christopher and Son, Solicitors, 28, Argyll Street, Regent Street, W.; and of Messrs. Fuller, Horsey, Sons, and Co., 11, Billiter Square, London, who are empowered to treat for the disposal by private contract, or an offer for renting would be entertained.

UNIVERSITY COLLEGE, BRISTOL.

CHEMICAL PROFESSORSHIP.

The Council invite Applications for the Chair of Chemistry. Salary, £300, with a share of the Students' Fees. Applications, with testimonials, to be sent not later than 9th February, 1880. Further information may be obtained from the Principal on application to EDWARD STOCK, M.R.C.S., Secretary.

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R. E. FARRANT and Co., Dextrine Manufacturers, Gorton Gum Works, Manchester, are prepared to undertake the manufacture of any Chemical or Foods Speciality, or to furnish assistance (if required) with ample and superior accommodation, with steam-power, to any inventor or patentee desiring to manufacture.

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CONTENTS.

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The Hon. Henry Cavendish; his Life and Character. English and American Physique. By George M. Beard, M.D.
On Water and Air. By Prof. Tyndall, D.C.L., LL.D., F.R.S.
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THE CHEMICAL NEWS.

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THE CRYSTALLISATION OF CARBON.

SINCE sending the telegram* announcing that carbon crystals, apparently diamond, could without difficulty be produced from any carbon compound, Mr. Maclear has sent me several specimens of his supposed artificial diamond. He has also called upon me with other specimens, and has explained the whole process by which he obtains such remarkable results. As, however, he has sent to the Royal Society a paper which will probably be read in the course of a week or two, I am not yet at liberty to give details of the process.

The general character of the specimens now in my possession may be described as irregularly shaped masses from 1 m.m. downwards in diameter, with rounded angles, and showing no definite crystalline appearance. They are whitish looking, translucent, and as a rule lustreless; many pieces are almost spherical and appear like fragments of corundum which have been water worn. Amongst these are perfectly clear fragments larger in size, some being 3 or 4 m.m. across, having a conchoidal fracture exactly like glass.

In a paper "On Molecular Physics in High Vacua," read before the Royal Society in March last, and now being published in the *Philosophical Transactions*, I referred to the remarkable power possessed by the molecular rays in a high vacuum of causing phosphorescence in bodies on which they fall, and I remarked that the only body which surpassed Becquerel's luminous sulphides both in brilliancy and variety of colour is the diamond. Most of these gems, whether cut or in the rough, when coming from the South African fields, phosphoresce of a brilliant light blue colour. Diamonds from Brazil shine with different colours, such as bright blue, pale blue, apricot, red, yellowish green, orange, and light green. A beautiful collection of diamond crystals, kindly lent me by Professor Maskelyne, phosphoresced with nearly all the colours of the rainbow, the different faces glowing with different shades of colour. On receiving the specimens from Mr. Maclear, I immediately submitted them to the molecular discharge. The following are the results I have at present obtained:—

In a high vacuum the specimens phosphoresce brightly of different colours—pale blue, orange, apricot, and yellowish green. The clear glassy fragments are also phosphorescent. The appearance of the phosphorescence is very similar to that shown by small rough diamonds from Brazil, called in the trade "Boart;" indeed, had I not known the history of the fragments in my tube, I should, from their appearance, have said that they were small fragments of Brazilian Boart.

The opaque rounded appearance of the fragments is unlike that of the natural diamond, but by heating a rough diamond before the blowpipe until it has partly burnt away, it assumes a very similar appearance to that of Mr. Maclear's crystals, and it is therefore not unlikely, from their mode of preparation, that these crystals have undergone partial combustion after their formation—a fact which would explain this difference in appearance. Other specimens having been placed by Mr. Maclear in competent hands, with a view of determining their hardness and chemical properties, I have refrained from making experiments in this direction.

W. C.

ON THE REDUCTION OF AURIC CHLORIDE BY HYDROGEN IN PRESENCE OF PLATINUM.

By Dr. T. L. PHIPSON, F.C.S., &c.

THIS experiment, which is so illustrative of what is termed the "nascent state" of bodies, was originally made by Becquerel; it was afterwards repeated by me and explained in my paper, "*La Force Catalytique, Etudes sur les Phenomènes de Contact (Memoire couronné par la Société Hollandaise des Sciences, Harlem, 1858)*". A platinum wire which plunges into the auric chloride and the gas occasions the precipitation of gold, whilst the hydrogen disappears. I translate the following lines from my paper of 1858:—

"In this experiment the hydrogen is burnt by the chloride of gold instead of by oxygen as in the first experiment: the positive electricity of the platinum unites with the negative electricity of the hydrogen, and the negative fluid of the platinum unites with the positive electricity of the salt [this was the language used in those days; but whatever the language, the galvanometer proved the electric action and direction of the current]; hence the hydrogen reduces the latter; there is formed hydrochloric acid and metallic gold. The same thing would occur by operating *without the platinum* [that is, without the electric action], but *only at a high temperature*, as we have already seen for hydrogen and oxygen alone, for heat develops polarity as does the presence of a third body."

Now, in the whole of this paper, of thirty-four pages in quarto, the term "polarity" has been shown to be equivalent to the term "nascent state." It is, therefore, evident that I was occupied with this question nine years before M. Berthelot, and nineteen years before M. Tommasi. The latter, in a paper published in a recent number of the *CHEMICAL NEWS*, referring to this same experiment, and quoting his paper of 1877, says:—"As for us we think that the reduction of the chloride of gold by means of hydrogen and platinum is due to the heat disengaged during the condensation of the hydrogen on the platinum."

It will be seen from the above quotation from my paper that this attempted explanation by Professor Tommasi is unacceptable. The hydrogen is not absorbed by the platinum wire, but by the solution of gold; it does not combine with the platinum but with the chlorine; it is a purely electric action going on in the cold, an electric action which an interposed galvanometer renders manifest, as shown in several other experiments given in the same paper.

But let me be allowed to quote one other passage from my paper of 1858:—"A porous vase is placed in a glass vessel. In the porous vase is placed hydrochloric acid, in the glass nitric acid; then, into each vessel a strip of gold. As long as the two strips of gold remain separate there is no action, but as soon as they are united the metal is attacked, and that portion which is in the hydrochloric acid is dissolved; the nitric acid is at the same time deoxidised by the *nascent hydrogen* transported through the sides of the porous vase, and an electric current flows through the strips of gold."

I trust these quotations will suffice to show that I had explained the phenomenon of the "nascent state" of bodies as early as 1858, and when any chemist shall have proved by the *thermometer* that there is a caloric action, as clearly as I have satisfied myself of an electric action by means of the *galvanometer*, I will admit his theory. I do not deny (as I have stated in a previous note) that the caloric and electric actions may be mutually convertible, but I maintain that in whatever circumstances the phenomenon of nascent hydrogen occurs, there we can detect electric action by means of the galvanometer; whereas the caloric theory is based upon purely speculative assumptions. Thus Prof. Tommasi, in endeavouring to explain the experiment which forms the subject of

* *CHEMICAL NEWS*, vol. xl., p. 306 (Dec. 26, 1879.)

this note, imagines the atoms of hydrogen playing against the platinum wire like the balls against the wall of a racket court.

In my work above quoted there are four conclusions; I may perhaps be allowed to reproduce here a literal translation of the first of them:—"1st. That the allotropic states of bodies analogous to ozone are due to a phenomenon of polarity acting in special circumstances, and having for effect to render the body on which we experiment infinitely more electro positive or electro negative than it was before; and that the state which we call in chemistry 'nascent state of bodies' is nothing more than that allotropic state."

I will only remind my readers that these words were published in 1858, whilst those of M. Berthelot are of 1863, and that Prof. Tommasi (in 1877 and subsequently), instead of assuring himself by direct experiments of the electric conditions of the substances on which he operates, contents himself with *supposing* a disengagement of caloric. If his suppositions happened by chance to be true, it is evident that caloric and electricity are one and the same thing.

ON THE NOXIOUS ACTION OF ACID VAPOURS ON VEGETATION.*

By Dr. G. LUNGE.

UNDER the above title, Mr. Hasenclever, the managing director of the Rhenania Chemical Works, near Aachen, has published a paper, the contents of which are not merely of great interest, but of real importance, for the British alkali-making districts. Since, hitherto, only a very short notice of this paper has been published by the CHEMICAL NEWS, it will hardly require an apology if I place its essential points before the Society in a more ample form.

Hasenclever commences by admitting at once the noxious action of acid gases upon vegetation, exercised wherever large quantities of such gases are given out. This takes place especially in calcining sulphurous ores and matts; large quantities are also given out by some chemical works. Complaints have also been made in some cases against ultramarine, glass, brick-works, and generally against those manufacturing establishments in which a large quantity of coals is consumed. The noxious action of acid gases on vegetation is further proved by the investigations of Stöckhardt, Freytag, and Schroeder (mentioned in my "Treatise on the Manufacture of Sulphuric Acid and Alkali," vol. i., p. 110-112 of the English edition). Hasenclever himself gives excellent coloured drawings, showing the outward changes produced by acid vapours on the leaves of the carrot, Scotch fir, oak, and rose. He adds pictures of tussilago leaves damaged by flue dust, and of beech leaves damaged by frost, both of which exhibit an appearance altogether similar to that of leaves damaged by acid vapours. Since, moreover, autumnal decay, fungi, insect stings, and other influences may bring about quite similar appearances, great caution is needed before fixing upon any one of these causes as the real one; and instances are given of considerable mistakes made in this way.

When the functions of the leaves are disturbed by these spots, whatever be their cause, the trees suffer in their growth; and, if this goes on for several years, they die altogether. The decrease of growth can be observed, without cutting down the trees, by taking a sample of it by means of Pressler's auger, which shows the single annual rings.

In many places, where the action of noxious gases is quite out of the question, plantations of trees exhibit the appearance of suffering. This may be produced by the tops of some of the trees projecting too far, which interferes with the growth of their neighbours; by leaving

single trees standing after cutting down those around them; and, more especially, by the drought consequent upon the regulation of water courses, the lowering of the level of lakes, and the ordinary effects of mining; and also, in towns, by the hardening of the ground round the roots produced by the street traffic. Wherever the wood is impoverished, by taking from it the decayed leaves and branches without imparting to it some kind of manure, a deterioration of the trees must necessarily follow.

Undoubtedly there are some cases where none of the causes just mentioned is present, and where acid vapours must be blamed for the destruction of vegetation. It is certain that this takes place more easily in damp than in dry weather. But the proof that such destruction is due to acid vapours, frequently sought to be obtained by a comparative estimation of chlorides and sulphates in damaged and sound plants, is anything but easy and certain. It has not been established yet that an increased percentage of acid in the leaves is regularly accompanied by a decrease of growth; neither is it known what is the maximum percentage of chlorine and sulphuric acid that leaves may absorb without being corroded. König found in the fir woods, near the Lethmathe Zinc Works, relatively considerable quantities of sulphuric acid in apparently healthy trees, whilst Schroeder found only half as much sulphuric acid in similar trees, on the Harz, which had been destroyed by noxious vapours.

No doubt, whenever much sulphur dioxide is given off, whether by the calcining of ores or in some other way, there is a good deal of sulphuric acid found in the plants. Similarly high chlorides are found near chemical works which decompose common salt, and near potteries which glaze their ware by means of salt; but both sulphuric acid and chlorine are also found in ordinary coal smoke. It is perfectly well known that formerly, when the atmosphere must have been purer from the absence of a highly-developed industry, the use of coal was disliked, and even here and there prohibited, on account of its "pestilential" smoke. Mohr asserts that the obelisk of Luxor, erected in 1836 on the Place de la Concorde at Paris, has suffered more in thirty-six years there than during as many centuries in Egypt. Rhenish and Westphalian coal contains on the average $1\frac{1}{2}$ per cent of sulphur (usually in the shape of pyrites), which must pass into the air as sulphur dioxide. The injurious effect of this is frequently felt either where coal is burnt in large quantities, or where the smoke, as in narrow valleys, is not specially diluted by air. This is proved by special instances from Stöckhardt and others. The statements of Dr. Angus Smith are also quoted, according to whom London air contains, in a million of cubic metres, 1670 grams sulphuric acid, and Manchester air 2518 grams.

Chlorine also regularly occurs in coals, and is not found in their ashes, but expelled along with the volatile constituents. The chlorine of gas water must be reduced to this origin. There is also chlorine in blast-furnace gases.

Soot seems to be quite harmless by itself, as proved by Stöckhardt's experiments; but becomes very injurious to plants when containing metallic sulphates, as is often the case in industrial districts.

Hasenclever now turns to the question, how the injurious effect of acid vapour upon vegetation, whether arising from the direct evolution of such vapour or from coal smoke, may be best prevented? Tall chimneys are quite inefficient in the case of large quantities of vapours; the dilution by air, even when sufficient in fair weather, fails to act in damp weather. Only in isolated establishments, and with moderate quantities of vapours, does dilution by tall chimneys prove satisfactory. Washing out the acids from coal smoke has not been found practicable. The legal enactments for the complete combustion of smoke enforced in some English towns have done some good, but have only diminished the *black* smoke. In Germany, at watering places, and other towns or villages which depend upon the concourse of visitors, the establishment of factories is not permitted, or is at least

* A Paper read before the Newcastle-upon-Tyne Chemical Society.

restricted. Dr. Angus Smith frequently points out that too great an accumulation of industrial establishments in any one locality should be avoided. At Hanover (and some other large German towns) the existing factories are not interfered with, but no new factories are licensed within the urban district.

All this refers to coal smoke. The sulphur dioxide escaping from glass works in enormous quantities is nowhere condensed; it is too much diluted for the condensation to be effectual. In vitriol works the escape of sulphurous and sulphuric acid has been very much lessened by the introduction of Gay-Lussac towers. But considerable difficulties still exist in the case of copper, zinc, and lead smelting works. In Germany many of these now calcine the ores in Gerstenhoefer's or in Hasenclever and Helbig's kilns, and thus utilise a portion of the sulphur as vitriol. But even then the condensation of the calcining gases is not perfect, and further progress in this direction is very desirable. Still there is a considerable difference between the desolate aspect presented by the neighbourhood of those smelting works which do not condense at all, and the field and garden culture observable close to the other kind of works, although even these interfere with fir woods, fruit-bearing trees, and some other kinds of trees.

We now come to the escape of hydrochloric acid from factories decomposing common salt by sulphuric acid. Everybody knows the former state of matters in this respect as laid bare by the Belgian Commission of 1855, and the reports of the British Alkali Inspectors since 1863. The large escapes formerly complained of can be directly traced to the fact that at that period far more hydrochloric acid was produced as an involuntary by-product than could be utilised or sold. In Germany at least this is no more so; the demand for hydrochloric acid on the part of colour, copper, glue, dye, and sugar works has increased to such an extent that some factories produce this acid as their principal article, making soda-ash only as a by-product. This is best proved by the fact that no special legislation on the condensation of hydrochloric acid was thought necessary in Prussia, although the question was mooted in its proper place, because it was considered that the self-interest of the manufacturers was quite sufficient to ensure efficient condensation. The German alkali works usually possess even a larger condensing space than that laid down as sufficient by Dr. A. Smith, and no damage worth speaking of is done to vegetation in their neighbourhood where they are isolated; but where they are situated among many other industrial establishments they are sometimes singled out, and held responsible far beyond their due, for the damage unavoidable in such circumstances.

This is illustrated by a private communication to Mr. Hasenclever from Dr. Fletcher referring to the acids escaping in the neighbourhood of St. Helen's. Dr. Fletcher calculates the weekly escape from—

Fire gases at	800 tons SO ₂
Copper works at	380 " "
Glass works at	180 " "
Alkali works at	25 " HCl

Hasenclever himself gives a table referring to the twenty-eight industrial establishments situated at Stolberg, near Aachen, on an area of 1600 acres, whose 220 chimneys emit in twenty-four hours 34,500 kilogs. of SO₂ from coals, and 50,858 kilogs. of SO₂ from zinc works, glass works, &c.; but only 480 kilogs. SO₂ and 750 kilogs. of HCl = 4 per cent of the total acids from the Rhenania Alkali Works.

Hasenclever concludes by admitting that acid vapours ought to be condensed as fully as possible; also that everybody ought to be made to pay fully for any damage he may have caused; but he claims that manufacturers ought not to be harassed *beyond* their due, and he complains that public opinion is not always just in this respect. Damaged plants and trees occur in many places

remote from chemical works and other industrial establishments, as proved by several photo-lithographic illustrations appended to the original, and their occurrence near such establishments should consequently not be taken as caused by acid vapours without real proof of the case.

The PRESIDENT—I should like to mention one circumstance before we conclude. The Committee of this Society have deliberated for some time on the possibility of amalgamating the two Societies which exist in this district. I have myself seen for several years the importance of uniting these two forces, and there seems at present a reasonable prospect of its being fulfilled. With this object, the Committees of the two Societies are going to meet to-morrow night at the house of Mr. Scholefield, who has kindly invited us; and possibly at our next meeting we may have something to communicate to the Society on this subject.

Votes of thanks were then given to the gentlemen who had furnished the material for the evening's discussion, and the proceedings terminated.

DETERMINATION OF COBALT AND NICKEL.

By E. DONATH.

E. FLEISCHER describes, in the *Journal für Praktische Chemie*, 1870, vol. ii., p. 48, a very useful method for the determination of cobalt and nickel together, which consists in halving the solution, and in each half converting both metals into sesquioxides, Co₂O₃ and Ni₂O₃, by boiling with hypochlorite of soda or soda-lye and bromine, and determining volumetrically the quantity of nickel in the precipitate of the one half. The precipitate of the other half is boiled previously with ammonia, which reduces Ni₂O₃ to NiO and chiefly dissolves it, whilst the oxide of cobalt remains unchanged.

This reduction by means of liquid ammonia is not sufficiently rapid and easy, as may be ascertained by direct experiment, and in the application of the above method it remains doubtful if all the nickel sesquioxide has been reduced.

The author has, therefore, adapted a modification of Fleischer's process which depends upon the following facts: If a cobalt solution is mixed with excess of potassa or soda-lye, and brought to a boil, the cobalt hydroxide is as completely converted into sesquioxide as by treatment with bromine; nickel oxide is not effected by this process. Hence in the one half of the solution of the mixed metals both are converted into sesquioxides by boiling with alkali and bromine, whilst in the other half merely the cobalt is converted into sesquioxide by a similar treatment with alkali and iodine.

The precipitate, freshly taken from the filter, is boiled with hydrochloric acid in a suitable distillatory apparatus, and the chlorine evolved according to the equation—



is collected in solution of potassium iodine, and the iodine separated is determined volumetrically by a decinormal solution of sodium dithionite.

As 1 atom of iodine corresponds to an atom of cobalt or nickel, and the atomic weights of the two latter are equal, =59, the difference of the required number c.c. of the dithionite solution in both determinations, multiplied by 0.0059, represents the weight of the nickel; the number in the second case, where only the cobalt is converted into sesquioxide, if multiplied by 0.0059, gives the weight of the cobalt. In this modification of Fleischer's process the cobalt admits of direct volumetric determination in a single operation. In the determination of both metals, the treatment with boiling ammonia is dispensed with

and certainty is reached that the nickel is present in the precipitate merely as NiO.

In converting the cobalt into sesquioxide by treatment with iodine in an alkaline solution, the iodine is added at common temperatures as long as the colour of the precipitate grows darker, and the liquid is then raised to a boil.

If the nickel predominates in quantity, the solutions of both metals are used in a dilute state, otherwise the conversion of the cobalt into sesquioxide may be delayed by a coating of the oxide of nickel.—*Berichte der Deutschen Chem. Gesellschaft*, xii., p. 1868.

ON THE ESTIMATION OF THE NITROUS COMPOUNDS IN THE VARIOUS STAGES OF THE MANUFACTURE OF SULPHURIC ACID.*

By JAMES MACTEAR, F.C.S., F.I.C.

1. A CONVENIENT, speedy, and accurate method of estimating the nitrous compounds in the various stages in the manufacture of sulphuric acid has long been a desideratum amongst manufacturers, the loss of nitrous compounds being a serious element in the cost of production, and as the whole of the nitrous compounds evolved from the nitrate of soda employed in the process of manufacture are, with the exception of the amount which is contained in the sulphuric acid as run off for sale or use, lost in the gases passing away from the chambers, it becomes a most necessary thing to ascertain the amount thus escaping.

2. The testings may be resolved into:—

- a. The estimation of the nitrous compounds escaping from the leaden chambers before entering the Gay-Lussac or absorbing column.
- b. The estimation of the nitrous compounds escaping from the Gay-Lussac column to the air by the chimney.
- c. The estimation of the nitrous compounds arrested or absorbed by the strong sulphuric acid in the Gay-Lussac column.
- d. The estimation of the amount of nitrous compounds in the acid from the denitrating column, or from the chambers when run off for use.

3. In *a* and *b* the gases being aspirated by the apparatus described in my paper on "Controlling the Escapes of Sulphur Gases in the Manufacture of Sulphuric Acid," read before this Society on 22nd March, 1877, are absorbed in caustic soda solution, and the nitrous compounds are then estimated; for this purpose I employ two methods.

4. First, by a method which I have introduced, and which I call the method of "total acids," which, while without any pretension to extreme accuracy, yet does good service, and gives remarkably consistent results. It is conducted thus:—The absorbing arrangement which I use consists of a convenient wooden stand, supporting a series of four tubes each, of about one inch internal diameter, and about fifteen inches long; these are all connected with each other by means of india-rubber corks, glass tubes, and rubber joints. The inlet tubes, which reach close to the bottom of the tubes, are made thus—the end of the tube is fused until the opening becomes very small; the tube is then, while still hot, pressed vertically on an iron plate so as to flatten and spread it out like the head of a nail; the small hole thus left only allows of small bubbles of gas passing, which are broken up by the broad flat end of the tube, and expose a large surface for absorption.

The tubes are filled with the absorbing liquid in the following way:—

In the 1st tube is put	50 c.c. normal soda	+	50 c.c. water
" 2nd "	25 "	+	75 "
" 3rd "	25 "	+	75 "
" 4th "	00 "	+	100 "

The last tube being tinted with litmus, so that should any sulphurous acid or nitrous acid pass the three previous tubes the colour of the litmus will be destroyed, and thus it acts as an indicator of the complete absorption of the gases in the sodic solution.

The aspiration completed, the tubes are disconnected and the contents washed into a beaker and titrated with normal HCl; it is then made up to the bulk of a litre and 250 c.c. taken, and the total sulphuric acid determined by precipitation with baric chloride in the usual way.

Let us assume a case where the gases aspirated amount to 12 cubic feet,* the absorbing tubes containing 100 c.c. normal Na₂O.

After absorption the solution required 80 c.c. normal HCl to neutralise remaining soda, then as 1 c.c. Na₂O is equal to 0.617 grain SO₃, we have:—

$100 - 80 = 20 \div 12 = 1.666 \times 0.617 = 1.0279$ grains; total acids per cube foot stated as SO₃.

The neutralised solution made up to a litre and 250 c.c. gave 0.300 gramme BaSO₄ $\times 4 \div 12 = 0.10$ gramme per cube foot = 0.529 grain per cube foot of SO₃.

Thus we have:—

1. Total acids per cube foot	=	1.0279 grains SO ₃
2. SO ₃ existing as S acids	=	0.5297 "

Excess of 1 = N compounds as SO₃ $\frac{1.0279 - 0.5297}{1} = 0.4982$ "

Equal to NaNO₂ grains per cube foot 1.0586

It can be easily seen that this method is very simple and easily applied, and as the estimation of the sulphur compounds should always form part of the work of the laboratory of every sulphuric acid manufacturer the additional labour of obtaining the nitrous compounds escaping by this method is extremely small.

The only difficulty which presents itself is in the titration of the solution, which contains invariably a not inconsiderable quantity of CO₂, which, in the presence of nitrous compounds, renders the end of the reaction rather difficult to determine.

This method I have used daily for nearly a year, and the results are most satisfactory. Compare the following tests, being for one week, stated as grains NaNO₃ per cube foot.

Total acid method.	Estimated as NH ₃
1.56	1.40
2.68	2.65
2.41	2.38
1.66	1.53
0.88	0.96
1.52	1.42
1.88	2.08
0.55	0.46
Average 1.64	1.61

5. The method of estimation of the nitrogen as NH₃ is the method I prefer and regularly employ (using the total acid method in addition as a check and a useful comparative result).

This method of estimation has been the subject of much discussion, and seems to have been praised and condemned alternately by those who have written on the subject.

* A Paper read before the Newcastle-upon-Tyne Chemical Society.

* No correction is made for bulk of gases absorbed, only unabsorbed gases being measured.

Döbereiner (*Z. pr. Chim.*, 15, 318) was, I think, the first to point out that when zinc and iron were immersed together in a solution of potash nothing but hydrogen was evolved, but if nitre be present ammonia is evolved.

The use of this reaction was first proposed by Vernon Harcourt,* as a means of estimating nitrogen compounds.

Of late there have been a number of researches published on the subject, amongst others one by J. W. Johnson,† who has investigated the methods proposed by Bunsen and Thorpe, and who condemns that of the latter as having yielded very bad results, the deficiency being in one case as much as 45 per cent.

Thorpe himself,‡ on the contrary, obtained the following results with pure potassic nitrate alone :—

	Grm.
KNO ₃ used 0.0474 grm., found (1)	0.0476
(2)	0.0473
(3)	0.0487
(4)	0.0469

Average 0.0475

Equal to 100.21 per cent.

Which are satisfactory enough. He has likewise investigated the effect of various salts on the decomposition. The results will be found fully detailed in his paper.

Dr. Lunge, in a paper communicated to this Society last session, condemns, in no measured terms, this method of estimating the nitrogen acids. In his introductory remarks he says—"It thus became the first portion of my task to thoroughly examine at least those methods of estimating the acids of nitrogen which have been proposed or used for testing 'nitrous vitriol.'"

He then, after describing the estimation of N₂O₃ by the iron process of Pelouze, with various of its modifications, proceeds to treat of the method of estimation of nitrogen acids by reduction "in an alkaline solution by means of iron or zinc;" and goes on to say: "The total nitrogen is thereby supposed to be converted into ammonia." I trust to be able to show that this supposition is well founded.

He further says: "I had, however, to use caustic soda in the place of caustic potash, as I could not procure the latter free from nitre, and it is just possible that the apparently good results obtained by this process may, in some cases, be due to the employment of impure potash. I for my own part, working with rigid accuracy, and, if anything, overdoing all the precautions prescribed for this process (for instance, allowing the mixture to stand some time and distilling very slowly, say three to four hours), never obtained any satisfactory results. The ammonia produced fell short in six experiments; from 16 to 26 per cent of that calculated from the pure potassic nitrate, &c., employed. Nor could I better the case by some experiments tried with the original plan of Schultze's, and by a modification proposed by Hager. A number of chemists have come to the same conclusion, viz.: that the process of estimating nitrates by reduction in an alkaline solution cannot be depended upon; and although a number of other chemists certainly have obtained accurate results by it this may be due partly to the fact that in some special circumstances the process does work well, whilst it does not in other cases, and partly to a compensation of the loss of ammonia by the nitre consumed in the potash employed, or by carrying over of fixed alkali. A process which gives such uncertain results in spite of scrupulously carrying out the prescriptions given for it, ought not to be resorted to so long as any other process of undoubted accuracy is available."

I regret to differ from Dr. Lunge when he says that in pursuance of the determination by thorough examination and rigid accuracy he has proved the method to be one

that can not be depended upon, giving results from 16 to 26 per cent under the true amount, and I base my opinion upon:—

1st. The good results obtained by so many competent chemists, most recently, perhaps, by J. M. Eder,* whose exhaustive research on the various methods of determining nitric acid is worth careful study.

2nd. The results of my own experience of the method of using zinc and iron with caustic soda. This method has been employed almost daily in the laboratory of the St. Rollox Works for many months, some hundreds of tests having been made by it, and the results have proved perfectly consistent and satisfactory.

3rd. A special set of experiments which I have had made, on reading Dr. Lunge's paper, partly with a view to find out the sources of error likely to occur, and to determine the accuracy of the process generally.

a. It was found that there was a danger of all the nitrogen compounds not being decomposed and driven off as ammonia in any given time, and the simple precaution against this source of error, of changing the receiver, and continuing the distillation for a short time further, is quite enough to prevent this error passing unobserved.

b. It was found that the decomposition took place most completely at the higher temperatures, and that it was necessary to evaporate until the contents of the flask were in a pasty condition, in order to get off the final traces of the NH₃.

c. The danger of fixed alkali or zincate of soda passing over even the wash flask was found to be a source of probable occasional error, and it was guarded against by neutralising the excess acid with standard soda, then adding a known excess and boiling until all the NH₃ had been expelled, when the solution is titrated with standard acid.

(To be continued.)

ESTIMATION OF THE TOTAL CARBON IN IRON AND STEEL EN MASSE.

By SYDNEY C. JUTSUM.

THE author having had some experience in these analyses of their inherent difficulties and the best means of overcoming them, takes the present opportunity of laying them before the readers of the CHEMICAL NEWS, trusting they will prove of service to those who perform iron and steel analyses.

There are three principal methods employed for this purpose. 1st. Regnault's, or the direct combustion of the iron. 2nd. Berzelius's, or dissolving by means of CuCl₂ and subsequent oxidation of the separated C. 3rd. Weyl's, or the electrolytic solution of the metal and subsequent oxidation as in No. 2. (*Pogg. Annal.*, 114, 507.)

1st. *Solution of the Metal.*—One is met at the outset by the following difficulties. Method No. 1 requires "the iron to be in the finest powder, and passed through a plate sieve with very small holes." ("Fresenius Quant. Anal.") Now there are many samples of metal which it is impossible even to file, and can only be cleaned by the grindstone; and the softer specimens are very liable to become mixed with foreign bodies when being pulverised and sieved.

No. 2 method effects solution by means of the following mixture:—6 ozs. CuSO₄, 4 ozs. NaCl, 2 fl. ozs. HCl, and 20 ozs. water. In Fresenius's laboratory I saw NH₄Cl employed instead of NaCl. Both at King's College and Fresenius's laboratories I have seen 4 to 6 days required to dissolve 10 grms. of soft iron turnings. With the tool steel which I used for analysis this method was also inapplicable, because the metal was too hard even to file, and such a compact block of metal would have required

* *Chemical Society's Journal*, xv., p. 381.

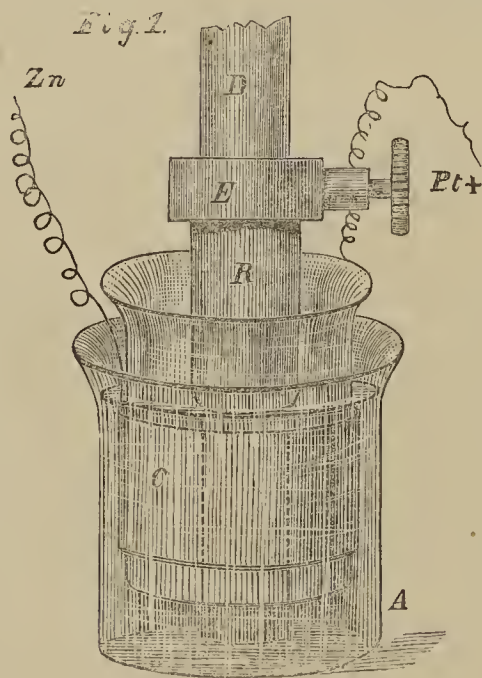
† *Silliman's Journal*, April, 1877.

‡ *Chemical Society's Journal*, 1873, 541—549.

* *Zeitschr. Anal. Chem.*, xvi., 267, 314.

weeks—perhaps months—for solution. I therefore pointed out the two objections to these two methods, viz.:—1st, the impossibility of reducing the metal to “turnings” or chips, much less fine powder in many cases, and the danger to its purity even when attained; and, 2nd, the length of time required for solution. But I never received any satisfactory reply to these two main objections. In addition to the above objections, I may add the employment of free acid as in the above solution is dangerous, as it causes a loss of C in the form of gaseous hydrocarbons, more especially if the mixture is heated. (*Hahn Annal. d. Chemie. u. Pharm.*, 129, 76.) Without free acid the time is longer. Again, the precipitation of the metallic Cu is a drawback, causing waste of H_2SO_4 and CrO_3 in the subsequent part of the analysis (should Ullgren's method be employed to oxidise the C), or else an extra loss of time to effect its solution.

With the 3rd, or electrolytic method, none of these difficulties arise. 1st. There is no need to pulverise the metal; and, 2nd, the time is about one hour to effect solution of 10 to 15 grms. metal. I used to start it to work the last thing before leaving off work, which requires about one hour; it proceeds during the night by itself, and on arriving next morning one finds 10 to 15 grms. of the hardest tool steel dissolved and the C separated out in large flakes, and in an excellent condition for filtration. There are, however, a few simple precautions to be observed; this done the method works rapidly and accurately. A single Grove or Bunsen cell is employed, and a simple decomposing cell, which I found to answer very well, as represented in Fig. 1. A is a beaker of about



5 ozs. capacity containing HCl, in which is immersed a glass cylinder, B (a beaker without a bottom), and between them is a Pt foil connected with the negative electrode. The best strength for the HCl is sp. gr. 1.050, or about 1 pt. HCl conc. and 3 water. Fresenius merely says “dilute” acid; but I found sp. gr. 1.100 or 1 pt. HCl conc. to 1 water to be too strong, causing Cl to be evolved at the surface of the steel, and thus running a risk of loss of C. D is the steel bar, and E a common binding screw. The object of this cell is to see if any bubbles of Cl or gaseous hydrocarbons are given off at the surface of the steel, which would prove fatal to the success of the analysis, and which in an ordinary decomposing cell is not at all easy to ascertain on account of the violent evolution of H from the negative electrode. The steel bar is best preserved from rusting by a strip of rubber sheeting tied round it, leaving 1 inch exposed for solution, R, Fig. 1.

Next comes the binding screw, E, for the purpose of connecting it with the battery, and the rest of the bar may be protected by a thermometer-case from the acid fumes. By these means I was enabled to preserve its brightness for two weeks. The bar I employed was 11 inches long by $\frac{1}{4}$ inch square. It was first ground bright, weighed, and immersed in the cell to a depth of about 1 inch. After solution it was again weighed; the loss represented the steel dissolved. With regard to the strength of the current, two-quart Daniell cells are too weak to overcome the resistance due to the liquid and counter current, and two Groves or Bunsen's are too strong, causing the evolution of gas at the surface of the steel. It is important to regulate these various factors, as otherwise the solution proceeds too slowly, and on arriving next morning the steel is not dissolved; or it proceeds too rapidly, and then ensues a loss of C. As before stated, one hour's work the last thing before leaving is all the time required to effect solution by this method when properly conducted, and the process proceeds and finishes by itself during the night.

2nd. *Filtration of the C.*—Of course filtering paper is inadmissible. It is recommended in the text-books to use an asbestos filter tube, but it is difficult to make them work satisfactorily. The C is often in very fine powder, especially when the method No. 2 is employed, and the filter is very liable to allow the C to run through or to choke. I tried pounded glass, glass wool, but with much the same effect, and have seen in the laboratories the results obtained by others. I have seen hours taken to filter and wash the C even when using a water pump. I have therefore invented a simple apparatus, which, while

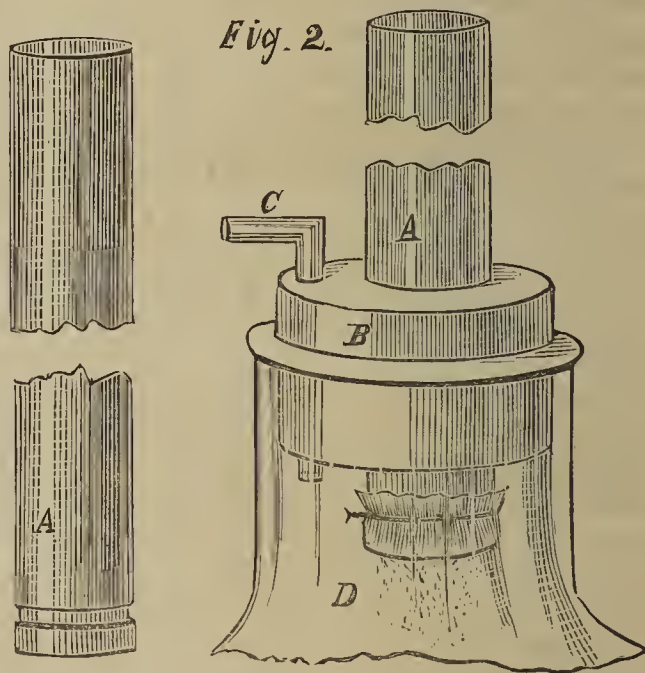


Fig. 2 represents the apparatus. A is a glass tube, about 12 inches long, 1 inch wide, and $\frac{1}{4}$ inch thick in the walls. Half an inch from one end is filed a groove, E. The tube is inserted in a doubly-perforated stopper, B; the other perforation contains a tube connected with an exhausting pump, C. The stopper is inserted in a thick glass bottle, D. Over the end, E, of A is first placed a disc of filtering paper (the purpose of which will be explained further on) next a piece of muslin, and lastly a piece of wire gauze, the whole being securely wired on to the tube, the groove, E, preventing it slipping off. Into the tube, A, purified silver sand is next placed to a height of 1 inch, and upon that glass wool to the same height. On pouring the contents

of the decomposing cell into the tube, A, and exhausting, the entire contents were completely filtered and washed in ten minutes. It is important to wash first with water, then with caustic soda, and lastly with water, in order to ensure the complete removal of HCl, which if allowed to remain would give rise to Cl in the Ullgren's apparatus and increase the weight of the potash bulbs. The stopper, B, is then removed, the cap is then taken off the tube, and the contents are pushed into the flask of the Ullgren's apparatus by first inserting an extra plug of glass wool and pushing it down into the tube: the entire contents are thus completely removed. On examining the little disc of filtering paper no C is ever seen on it except a minute brown stain. This filter tube when carefully managed never allows the smallest particle of C to run through and never filters slowly, the liquid running out so rapidly as to even cause some little trouble to keep the tube full.

3rd. *Oxidation of the C.*—This can be easily done either in a combustion furnace or in the well-known Ullgren's apparatus with H_2SO_4 and CrO_3 . I met with an unexpected difficulty, owing to the KHO containing potassic nitrite, which by absorbing the O increased the weight of the potash bulbs too much. Mr. G. S. Johnson was the first to point out this reason to me, as he had previously found the same impurity in caustic potash, and his results I confirmed by a long series of experiments, which, however, are not considered interesting enough for re-publication. Another source of error is the difficulty of obtaining a constant figure on removing, weighing, and re-attaching the potash bulbs to the apparatus. The difference, however, is small, and the general opinion seems to be that the CO_2 is absorbed into the interstices of the pumice, and is with difficulty dislodged.

In conclusion, I have to thank Prof. Bloxam for his great kindness in correcting this MS. and for superintending these analyses, and to Prof. W. N. Hartley for kindly giving me the benefit of his experience in iron and steel analyses.

NOTICES OF BOOKS.

Electro-plating. By J. W. URQUHART, C.E. Pp. vi. and 216. London: Crosby Lockwood and Co. 1880.

HAD the chemistry of this small manual been sound, we could have commended it without reservation. The author is obviously more thoroughly familiar with the metallurgical workshop than with the chemical laboratory. He speaks (p. 40) of caustic lime as being "apt to absorb moisture, and swell," as if this were an occasional and rather undesirable quality of the compound in question. The impurities of cyanide of potassium are vaguely termed "salt of potash" (*loc. cit.*). Hydrocyanic acid is spoken of (p. 43) as a solution of "a gas called prussic acid." "Fumes of cyanogen" are said (p. 83) to escape from a mixture of cyanide of potassium and sulphate of copper. The descriptions of metallic mercury (p. 44) and of the tests for its quality waste ten lines of letterpress: the essential facts might have been stated in the same space, and in simple, perfectly intelligible, and useful form. We are introduced (p. 44) to the remarkable bit of commercial intelligence that nitric acid may be bought "in Worcester quarts at 25 per cent less." The directions as to the purchase of chemicals and apparatus seem inadequate, as in the following case (p. 46):—"A galvanometer is very useful; get one with two circuits." But when we turn from the introductory and explanatory portions of Mr. Urquhart's volume to the chapters on the actual operations of electro-metallurgy, we find instructions complete enough and clear enough for all ordinary work. But even this part of the volume might have been made

more useful had it been more precise in details and more methodical in arrangement.

The author would have acted more judiciously had he refrained from attempting to include "all the necessary information concerning materials, the preparation of them, and their prices in commerce," in his small practical handbook on electro-plating and electrotyping.

Questions on Chemistry: a Series of Problems and Exercises. By F. JONES, F.R.S.E., F.C.S. London: Macmillan. 1879.

THE author or collector of these 1237 problems and exercises prefaces his little book with no justificatory remarks. He has, no doubt, felt the want of a large and varied series of intelligent questions for his classes, and has not been satisfied with the problems to be met with in the usual chemical manuals. Certainly, from whatever source or sources they may have been derived, these questions on chemistry cannot fail to be of considerable service to teachers. They are numerous; they are judicious; they are interesting, and they are often in themselves instructive. A characteristic specimen is No. 190, p. 30:—"The Koh-i-noor diamond weighs 106 carat (1 carat = 3.17 grains). What volume of oxygen at N.T.P. would be necessary to burn it, and what volume of carbon dioxide would result?" A large proportion of the questions are so turned as to discourage cram: with the same intention it would be well to introduce others beginning with "Describe the compound represented by the formula so-and-so;" "Complete the following equations," and so forth.

First Book of Qualitative Chemistry. By A. B. PRESCOTT. Pp. 160. New York: D. van Nostrand. London: Trübner. 1879.

As we cannot produce a sufficient number and variety of small chemical text-books in this country the Americans are sharing in the manufacture. We confess that we long for some one chemist, with wide and intimate experience of teaching, and profound knowledge of the science, to take in hand the task of gathering every meritorious feature of these innumerable manuals of qualitative analysis into one focus. Of course a large number of these books possess no distinguishing characteristics worth preserving, but are mere compilations, chiefly for cram purposes, written by teachers with scant knowledge and limited experience. But Professor Prescott's "First Book" belongs to a quite different class, and yet it is difficult to understand what special necessity could have determined the author to add another manual to the score of useful works previously in existence. True, we have here an elementary book introductory to larger treatises; and a clearly-written book, giving hints, notes, suggestions, explanations, and illustrations, both varied and apposite. True, we have here presented in an instructive form a more definite and detailed study of bases and acids than is usually comprised in ordinary courses of experimental chemistry. But, with all its merits, and they are many, this book exhibits more ingenuity in its course of analysis and tabular arrangements than accuracy in its details. We constantly come upon erroneous formulæ. White-lead, for instance, is not $\text{Pb}_3\text{H}_2\text{O}_2(\text{CO}_2)_2$, as stated on page 21; gypsum is not $\text{CaSO}_4\text{H}_2\text{O}$ (p. 42); cassiterite is not stannous oxide (p. 96). But if such blemishes were removed we should possess in this elementary manual a very concise, compact, and methodical arrangement of a course of qualitative analysis. The best features of the work are these two—extreme lucidity in the descriptions given of the courses of analytical operations, and extreme thoroughness in the explanations of the chemical changes mentioned or employed. The Americanisms in expression, grammar, and spelling, are peculiarities which may easily be condoned in view of the excellent qualities which we have named. In fact, the only substantial

questions relate to the utility of this volume. Was it needed at all? Does it supply a real want? Have we not already at least half-a-dozen manuals of qualitative analysis covering the same ground, and answering the same purpose as Prof. Prescott's "First Book"? However, the circumstances and aims of different teachers, and of different students, vary so much that we feel that it would not be wise to give perfectly definite answers to the questions which we have just propounded. For a time, at least, room may be found, and a sphere of usefulness, for the book before us.

Annals of Chemical Medicine, including the Application of Chemistry to Physiology, Pathology, Therapeutics, Pharmacy, Toxicology, and Hygiene. Vol. I. Edited by J. L. W. THUDICHUM, M.D. London: Longmans, Green, and Co.

WE have here the first volume of a new periodical devoted in the main to those subjects where chemistry and biology come in contact, and form, like most border-regions, a fruitful field for investigation. In his preface the author refers to the manner in which his researches, scattered through various journals and *Transactions*, have been at once ignored and pirated. To this subject he returns in a chapter on biliary pigments, in which Profs. Maly and Hoppe-Seyler are somewhat sharply criticised.

In another portion of the work he discusses the colouring matter in the shells of birds' eggs as "an illustration of chemical diagnosis at a glance, and of the value of abstract chemical researches." He criticises the views of Wicke, Sorby, and Liebermann, maintains that the green pigment of egg-shells is similar to but not identical with biliverdin, and that the colouring matters are not painted on the outside of the shell, but imbedded in its substance. Diagrams are given of the spectra of cruentin, a derivative of hemato-crystallin and of hematin, from which latter substance it is obtained by treatment with sulphuric acid.

A long but interesting chapter is devoted to an account of the life and philosophy of Robert Julius Mayer, the joint discoverer with Joule of the mechanical equivalent of heat, and hence substantially of the doctrine of the conservation of energy. It almost takes away one's breath to learn that the subject of this memoir was sentenced to "rustication" (i.e., received the *consilium abeundi*) by the University of Tübingen on the charge of "having been at a ball of the casino in a frock coat instead of the obligatory dress coat!" This is a very curious specimen of "*akademische Freiheit*." Save at the conclusion of his career, Mayer was so little appreciated that he might almost claim a place among the martyrs of science. We must admit that his great excitability, which in the latter part of his life was manifested in occasional attacks of actual insanity, and his general peculiarity of character and of style contributed greatly to prevent, or at least retard, the recognition of his views among the scientific world. But making all due allowances for such hindrances, we cannot help perceiving that it is still possible, even in Germany, for a discovery of the first order to be kept in the background by the machinations of jealous cliques whose spirit is very much on a par with that of trades-unionism. Among his opponents Dr. Seyffer might almost be characterised as a modern edition of Caccini or Riolan. But justice, tardy, indeed, but still complete, has been rendered to Mayer, and those who in our days are inclined to enter seriously upon the study of physics are now told that they cannot do better than begin with his works.

Dr. Thudichum devotes a chapter to combatting the existence of protagon as a chemical individual. He remarks that "the uniform chemical composition of the brain greatly favours the obtaining from it by the aid of processes nearly akin to trimming, of extracts of uniform composition; this uniformity can be greatly aided by limitations of the quantities of materials operated upon,

and of the quantities and strengths of the solvents, and by careful observation of these limitations, preparations are obtained which present a delusive appearance of definiteness."

The chapters on infection and infectious matter, and on the conflicting views of contagionists and non-contagionists concerning the origin of diphtheria, are an excellent summary of modern theories on these subjects. Altogether this volume is replete with valuable matter in one of the most difficult and neglected fields of chemistry. The author's occasional bitterness we can well understand when we find that some of the faculty propound inaccurate methods with the singular excuse that they are "good enough for clinical purposes." When shall we all learn that scientific work, unless accurately done, had much better not be done at all?

Photographic Printers' Assistant. By WILLIAM HEIGHMAN, author of "Practical Portrait Photography," &c. London: Richardson and Best. 1879.

MR. HEIGHMAN is well known as one of our most practical writers on photography, and he has done well to bring together in one little book a mass of valuable information with regard to the various printing processes. It is now some years since the art of photography underwent a process of self-division, and the worker with the camera rarely touches the printing frame, and *vice versa*. The processes described include paper printing in all its varieties and printing on porcelain. The sensitising and turning rooms, and their various appliances, also receive minute attention. The chapters on the treatment of disordered baths are also excellent. We are glad to see that Mr. Heighman does not confuse his pupil with too many formulæ. He might, however, with advantage have given more directions for the treatment of photographic waste.

Studies on Fermentation, the Diseases of Beer, their Causes, and the Means of Preventing Them. By L. PASTEUR, F.R.S., Member of the Academy of Sciences. A translation, made with the author's sanction, of "*Etudes sur la Bière*." By FRANK FAULKNER and D. CONSTABLE ROBB. London: Macmillan and Co.

IN examining works rendered from some foreign language we generally find ourselves face to face with two persons only, the author and the translator, and can easily apportion their shares of responsibility. Here the task is more difficult. There has been, in the first place, a literal version of Pasteur's work, executed by Mr. F. U. Waite. Then comes in Mr. D. C. Robb, who, "taking Mr. Waite's version as a basis, has so elaborated, annotated, and recast it," that the editor, Mr. Faulkner, feels bound to say that much of the value of the work, as it now appears, is due to this process of revision. This is somewhat perplexing. "Annotation" is at once intelligible, but "elaboration and recasting" seem to us to imply either additions to or modifications of the original subject matter. Not having the opportunity of comparing this edition with the French original, we are therefore at a loss to know whether it is M. Pasteur or Mr. Robb who speaks.

The change of title we hold to be an improvement. Under the name "Studies on Beer" it would have been read by brewers alone, and by that small and perfect distinct class of chemists who have made the theory of brewing their speciality. For us it would have had little attraction, since, though we by no means endorse the views advocated by Dr. Richardson and other eminent medical practitioners, that fermented beverages are altogether an evil, yet we cannot see, without regret, a highly nitrogenous product like grain used for the manufacture of a non-nitrogenous compound such as alcohol.

But M. Pasteur's work is not a mere trade manual. It enters into the question of so-called spontaneous generation (abiogenesis), it expounds the new, or physiological,

theory of fermentation, and throws valuable side-lights upon the origin and propagation of a formidable class of diseases—the zymotic, or pestilential. The theories of M. Trécul, M. Fremy, M. Béchamp, of Liebig, Oscar Brefeld, and Moritz Traube are also reviewed and combatted at some length.

M. Pasteur's own view, in brief, is that every kind of fermentation is due to the action of some organism pre-existent, and introduced into the fermentible liquid from without; such liquids, *e.g.*, grape-juice if protected with sufficient care, will not undergo fermentation—that each kind of fermentation, such as the alcoholic, the acetic, the lactic, &c., is due to a specific ferment; that each such ferment is a true and distinct vegetable species, reproducing its own kind, and that the various diseases, so-called, of wine, and especially of beer, are due to the presence of ferment germs or organisms other than those which effect the splitting up of sugar into alcohol. These propositions he has supported with such a bulk of experimental evidence that so far as we can see at present they must be accepted as in accordance with facts.

The practical conclusions drawn are that it is essential for every brewer, and indeed for every manufacturer of fermented liquids, to learn to distinguish between the ferments which serve his purpose and those which fight against him and destroy his products, and that he should be acquainted with the circumstances which favour the development of the one or the other class. This knowledge will enable him to understand and to prevent much loss which is now quietly accepted as accidental, mysterious, and therefore not to be remedied. Such knowledge the work before us supplies, or at least shows how it may be acquired.

The author shows, however, that the fermentative power is not peculiar to cells of a special nature, but depends on external circumstances and on the nutritive condition of the organism; in short, fermentation is the consequence of a peculiar vital process of nutrition which occurs under certain conditions. M. Pasteur, indeed, considers that the fermentative character may belong to every animal or vegetable cell on the condition that the acts of assimilation and excretion must be capable of taking place in such cell for a brief period without the necessity for recourse to supplies of atmospheric oxygen. In other words, an anærobian cell is a ferment cell. This generalised view of fermentation is supported by actual experiments on grapes, plums, &c., which, on immersion in carbonic acid gas, lost sugar and generated alcohol, though no ordinary yeast cells were present. These facts have been observed not only by M. Pasteur and M. Dumas, but by MM. Lechartier and Bellamy whilst engaged in researches on the ripening of fruits. Fruits, then, when surrounded with air live in their ærobian state, and there is no fermentation, but if immersed in carbonic acid they assume the anærobian state, and at once produce alcohol. These experimental results, it may here be remarked, are not favourable to the views of those who assert that alcohol is an unnatural product. If a heap of fruits be thrown together its production is an exceedingly probable reaction, and that in the entire absence of any human purpose or contrivance.

It is sometimes insinuated that M. Pasteur's conclusions are inimical to the theory of Organic Evolution. That they do not harmonise with abiogenesis we admit, but what can be said to the following passage:—"What was the nature of their original germs? (*i.e.*, of 'high' and 'low' ferments). These are questions which we are unable to answer, but we are very much inclined to think that we have here another example of the modifications which plants as well as races of animals undergo."

Turning to a very different subject we think that most Englishmen will totally dissent from the opinion repeatedly expressed in this work that malt liquors prepared by top fermentation are necessarily and universally inferior in flavour to those which, like the Bavarian beers, are made by "bottom" and "low" fermentation. That very de-

plorable beers have been made on the Continent by "top" fermentation is no argument against English and Scotch ales. We might as well condemn the choicest wines of France, Greece, or Hungary because the juice of the grape at Gruenberg yields a beverage which we can only pronounce excruciating.

But though this work will not—and in our opinion should not—induce our English brewers to revolutionise their whole system, it will doubtless suggest to them improvements of unquestionable value, and be the means of preventing great and frequent losses. It must therefore be pronounced a valuable contribution to our scientific and technical literature.

The Chemists' and Druggists' Diary for 1880. 44a, Cannon-street, E.C. 1880.

Letts and Co.'s Diaries for 1880. Letts and Co. 1880.

THE "Chemists' and Druggists' Diary," in addition to the usual trade notes and memoranda, contains a novel feature in the form of a well-written article, extending over twelve closely-printed quarto pages, on the art of dispensing, the work of several different authors, each of them specially conversant with some particular branch of the subject. Contributions have also been levied on the well-known works of Procter, Parrish, and Lescher, the *Pharmaceutical Journal*, and the *Chemist and Druggist*. We consequently have, so to speak, an eclectic essay on this important subject. The article seems very ably put together, and its general style presupposes a high standard of scientific education on the part of the student. The other articles are on the "Price of Medicines," by the late Daniel Hanbury, "Copyright in Designs," "Saturation Tables," Abbreviations, and French and German words used in prescriptions, and a Posological Table. By the way, in glancing over the table of Abbreviations used in Prescriptions we find that "A" has eight different meanings, "M" eleven, "P" twelve, and "S" no less than fifteen. Surely the sooner such abbreviations are entirely done away with the less chance will there be of physicians' prescriptions becoming possible death warrants.

We have received our usual yearly batch of Letts's useful Diaries for 1880. Having reached their thirtieth year there seems to be little more to say of them than that they still keep up their old high standard of excellence and as ingenious machines for diminishing loss of time and temper, some of them at least ought to be in the hands of every man of business, professional or otherwise. The octavo edition, No. 8, containing one page to each day, seems to be one of the most convenient for laboratory use.

CORRESPONDENCE.

OXIDISING VALUE OF CHLORATE AND OF BICHROMATE OF POTASH.

To the Editor of the Chemical News.

SIR,—With reference to the present high price of bichromate of potash, I think that many of the consumers of this article may have overlooked the fact that as an oxidising agent chlorate potash is now relatively much cheaper than bichrome. Chlorate of potash is now worth only about 8d. per pound; bichromate of potash has advanced to 7d. per pound. The relative oxidising power is, however, enormously in favour of chlorate. In round figures, one pound of chlorate will do the work of two and a half pounds of bichrome; at the above mentioned prices chlorate of potash as an oxidising agent is less than half the cost of bichrome.

Two years ago these two articles were relatively very nearly at the same price, viz., chlorate at about 9d. per pound and bichromate of potash about 4½d. per pound. For many oxidising purposes chlorate could actually be substituted with advantage for bichrome; after oxidation the residue is simply chloride of potassium, a soluble colourless salt. Bichrome leaves the insoluble dark coloured sesquioxide of chromium, which in some operations is removed with difficulty. It may also interest some of your manufacturing readers to note that, even as compared with the cheapest commercial oxidising agent—bleaching-powder—chlorate of potash is much lower than it has ever been before, and though not actually so cheap yet the relative cost of the two articles as oxidising agents now approaches each other pretty closely.—I am, &c.,

W. J. MENZIES, Managing Director.

Greenbank Alkali Works, St. Helens, Lancashire.

THE BLUE FLAME IN COAL FIRES.

To the Editor of the Chemical News.

SIR,—A year or two ago some correspondence occurred in *Nature* as to the cause of the blue flame observable in coal fires, which some of the editor's correspondents attributed to chloride of copper, others to sulphur, &c.

May I be permitted to add a suggestion to the above mentioned ingenious hypotheses, that the blue flame may be nothing but *Bunsenised* coal gas.

The flames, which are yellow and luminous when coals are first placed on a fire, become gradually thin, non-luminous, and bluish, when (as is here supposed) the bituminous matter having been consumed, the sides and bottom of the coal, or rather coke, have become porous, permitting the in-draught of air to mix with the burning gases at top, which are thus Bunsenised.—I am, &c.,

W. A. Ross.

London, W., January 3rd, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 24, December 15, 1879.

Researches on the so-called Hydride of Copper. M. Berthelot.—The author concludes that the amorphous substance precipitated in the reaction of hypophosphorous acid and sulphate of copper is not a true hydride, but a sort of phosphated hydroxide of copper, perhaps a mixture of several compounds.

Observations on the Cold supported by certain Microscopic Organisms.—M. Pasteur.—The bacteria of carbuncle and the organism producing the so-called cholera of poultry can bear temperatures of -40° without losing either virulence or their power of multiplication.

Measurement of the Intensity of the Rays of Absorption and of the Dark Rays of the Solar Spectrum.—M. Gouy.—Not susceptible of useful abstraction.

Chemical Composition of the Bones in the Arthropathy of Ataxic Cases.—P. Regnard.—In these bones the total mineral matter was found to be only 24.20 per cent, whilst the phosphates of lime and magnesia were reduced to 11.6. Of the organic matter 37.70 per cent or nearly one-half was fat.

No. 25, December 22, 1879.

Reply to M. Berthelot's Researches on so-called Copper Hydride.—A. Wurtz.—The author seeks to maintain the definite character of the compound Cu_2H_2 .

Reply to the Remarks of M. H. Sainte-Claire Deville on the Temperature of the Decomposition of Vapours.—A. Wurtz.—The author maintains that the vapour of chloral hydrate is not a compound, but a mixture, because it diffuses itself like a mixture of watery vapour and of anhydrous chloral; because, on ebullition, hydrated chloral is resolved into watery vapour and anhydrous chloral, which may be separated by means of chloroform, or by fractionated distillation; because the vapour of chloral hydrate behaves with a hydrated or anhydrous salt, like a mixture containing watery vapour; and, lastly, because the two vapours may meet without giving rise to the slightest disengagement of heat.

A New Silicium Hydride.—J. Ogier.—(See p. 1).

Comparative Researches on Pytalín and Diastase.—Th. Defresne.—The action of the saliva is arrested by pure gastric juice; but, on the other hand, if we operate with mixed gastric juice, containing merely organic acids, the saccharification of starch proceeds as well as in the mouth. Pytalín and diastase are, therefore, not physiologically identical, since the saccharifying action of the latter is irrecoverably destroyed by pure gastric juice or by hydrochloric solutions, and even after contact with mixed gastric juice, though it still dissolves starch, it no longer saccharifies.

A New Capillary Electrometer.—E. Debrun.—The instrument is a modification of the electrometer of Lipmann.

Determination of the Elements of a Vibratory Movement: Measurement of the Periods.—E. Mercadier.—A mathematical paper, not susceptible of useful abstraction.

Researches on Nitrification.—MM. Schloësing and Muntz.—Nitrification is effected within well-marked limits of temperature. Below 5° it is exceedingly feeble, if not entirely annulled, becomes appreciable at 12° , reaches its maximum at 37° , and then declines. At 45° the formation is less than at 15° , and above 55° there is no longer a trace. Nitrification increases with the proportion of the moisture, so long as the free circulation of air among the particles of the soil or other medium is not arrested. A slightly alkaline state of the medium is also necessary, due generally in nature to the presence of the bicarbonate of lime. Alkaline carbonates have the same effect if very dilute, but if they exceed 2 or 3 parts in the thousand they become unfavourable and even arrest nitrification. The presence of small quantities of neutral salts of the alkalies or alkaline earths appears to have no influence. Organic substances seem to act by affording a carbonaceous pabulum for the nitric ferment. The formation of nitrites is frequent in a liquid medium, especially if the thickness of the stratum exceeds 2 m.m. In soils nitrites are rarely produced.

Di-oxy-ethyl-methylen and the Preparation of Methylen Chloride.—W. H. Greene.—Di-oxy-ethyl-methylenic ether possesses a strong and pleasant odour; its sp. gr. is 0.851, and it boils at 89° . It is slightly soluble in water, from which it may be separated by means of calcic chloride. It dissolves in all proportions in alcohol and ether.

Palmellin and Characin, extracted from Fresh-water Algæ.—T. L. Phipson.—The yellow colouring-matter extracted from *Palmella cruenta* proves to be xanthophyll. Chlorophyll is also present as well as palmellin and characin. The latter substance is a kind of camphor, white, unctuous, volatile, inflammable, non-saponifiable, soluble in alcohol and ether, but almost insoluble in water.

Justus Liebig's Annalen der Chemie,
Band 199, Heft 2 and 3.

Contributions to the History of Starch and of its Transformations.—H. T. Brown and J. Heron.—After

a survey of the results obtained by former experimentalists, the authors describe the methods they have used for the purification of starch, for the preparation of starch-paste, and the preparation and analysis of the extract of malt, and the analysis of the transformation products of starch. They then examine the behaviour of starch with the extract of malt, after a preliminary notice of the changes undergone by a cold infusion of the latter, its fermentation in the cold, and its behaviour when heated. They find that the invertive action of the malt-ferment is most powerful about 55°, and ceases to act almost entirely at 66°. They then turn to the nature and properties of starch and starch-cellulose, the transformation of the insoluble cellulose, and the separation of a second body. They treat of the different glutinosity of starch-paste as occasioned by very slight differences in the manufacture of the starch. They describe the sp. gr. of starch in the form of paste, the solubility of granulose, the optical activity of starch-paste, its behaviour with potassa; the properties of maltose, its reaction with dilute sulphuric acid and with extract of malt; the behaviour of the latter with starch, distinguishing the various kinds of grain; the action of extract of malt upon bruised starch, upon starch-paste in the cold, describing the reaction as rapid and energetic; the behaviour of malt-extract upon starch-paste in heat at a number of specified temperatures. They then treat of the detection of soluble starch or achroodextrin in presence of erythrodextrin; the influence of neutralisation upon the activity of extract of malt, the molecular transformations of starch. They examine if dextrose is a product of the action of malt-extract upon starch, and reply in the negative, the product formed being maltose. Finally they discuss the nature of diastase.

Certain Ultramarine Compounds.—K. Heumann.—The author concludes that in a successful operation in a sealed tube, the reaction of silver nitrate upon blue sodium ultramarine is a very complete substitution of the sodium by an equivalent weight of silver, the subsidiary reactions being so trifling that they may be overlooked. The yellow silver ultramarine is beyond doubt a silver compound, corresponding to the blue sodium ultramarine. The author considers that a quantitative study of the various reactions of silver ultramarine will be of great value for throwing a light on the nature of the ultramarine compounds.

Hydrazin Compounds.—E. Fischer.—In this, his second memoir on the subject, the author describes the hydrazin compounds of the fatty series.

Ethyl Derivatives of Phenyl-hydrazin.—E. Fischer and W. Ehrhardt.—A description of diethyl-diphenyl-tetrazon, of azo-phenyl-ethyl, of hydrazo-phenyl-ethyl, and of the oxidation of phenyl-hydrazin by mercuric oxide.

Composition and Optical Behaviour of Quinamin.—O. Hesse.—The author's formula is $C_{12}H_{24}N_2O_2$.

Californian Orchella Weed.—O. Hesse.—The weed in question contains merely erythrin, along with very small quantities of roccellic acid as chromogen.

Amido-methylen-pyrocatechins.—O. Hesse.—The author obtains the hydrochlorate of monamido-methylen-pyrocatechin by the action of tin-filings and hydrochloric acid upon nitro-piperonylic acid.

On Quinin.—Z. H. Skraup.—The author considers that a methoxyl group is present in quinin, which occasions the production of formic acid. There are also grounds for assuming the existence in quinin of a hydroxyl group.

On Homo-cinchonidin.—Z. H. Skraup.—Homo-cinchonidin is struck out of the list of chemical individuals.

Annales de la Société des Sciences Industrielles de Lyon.
1879.

This volume contains nothing relating to the industrial applications of chemistry.

Revue Universelle des Mines, de la Metallurgie, &c.,
Tome 6, No. 1, July and August, 1879.

Determination of Carbonic Anhydride.—A. Classen and V. Francken.—The process cannot be intelligibly described without the accompanying plate.

Journal für Praktische Chemie.
No. 19 and 20.

Decomposition of Albumen in the Sprouts of Pumpkins.—E. Schulze and J. Barbieri.—Ferments probably take no part in the splitting up of albuminoid substances which takes place in protoplasm. Nägeli doubts if the organism ever forms ferments to act within the plasma.

Behaviour of Arseniferous Bismuth with Nitric Acid, and Preparation of Basic Bismuth Nitrate free from Arsenic.—R. Schneider.—If nitric acid acts upon arseniferous bismuth at a low temperature, the arsenic is oxidised to arsenious acid. But as bismuth arsenite is readily soluble in nitric acid, the liquid remains clear. If the temperature is allowed to rise, arsenic acid is generated, with the production of bismuth arseniate, very sparingly soluble in nitric acid.

Vitality of Schizomycetes in the Absence of Oxygen.—J. W. Gunning.—A reply to Nencki's defence of Pasteur's theory of anærobiosis.

Chemical Composition of the Bacteria of Putrefaction.—M. Nencki and F. Schaffer.—As in yeast, so in putrefactive bacteria, the layers which form cellular membrane do not consist entirely of a body resembling cellulose. An albuminoid matter is a constant element of their membranes.

Empirical Formula of Scatol.—M. Nencki.—The author gives this formula as $C_9H_9N.C_6H_2(NO_2)_3OH$.

On Urea-palladious Chloride.—E. Drechsel.—This compound is formed by adding to a solution of urea an aqueous or hydrochloric solution of palladious chloride. Its composition is $PdCl_2 + 2CN_2H_4O$.

The Source of Hippuric Acid in the Urine of the Herbivora.—O. Loew.—The author considers that in the grasses, in dandelion, &c., there exists an insoluble body which readily passes into quinic acid by hydration. The conversion of this acid into hippuric acid within the organism has been already observed.

Annales de Chimie et de Physique,
November, 1879.

The Instruments of Lavoisier.—M. Truchot.—A lengthy paper with many illustrations.

Action of Metallic Nitrates upon Monohydrated Nitric Acid.—M. A. Ditte.—The metallic nitrates may be divided into three distinct classes; those which dissolve largely in the acid and combine with it to form definite acid salts; those which, after having been deprived of their crystalline water, readily dissolve in the fuming acid, their solubility increasing with the temperature; and a third and more numerous class, which at all temperatures are insoluble, or nearly so, in the acid.

Formation-heat of Cyanogen.—M. Berthelot.—Already noticed.

On the Existence of Copper in Rocks, Seas, &c.—M. L. Dieulefait.—Already noticed.

Direct Combination of Cyanogen with Hydrogen and the Metals.—M. Berthelot.—Already noticed.

Various Thermo-chemical Data.—M. Berthelot.—Already noticed.

Action of Organic Solvents upon Sulphur and Metallic Sulphides.—M. Berthelot.—The author has dissolved powdered sulphur in pure ether and in "petro-

leum ethers," and has obtained similar results with natural sulphides, such as pyrrhotin, and with artificial ferrous sulphide.

On the Slow Changes of Wine During its Preservation.—M. Berthelot.

Galvanic Oxidation of Gold.—M. Berthelot.—Already noticed.

Decomposition of Hydroselenic Acid by Mercury.—M. Berthelot.—Already noticed.

Vibratory Forms of the Bubbles of Glyceric Liquid.—M. C. Decharme.—Already noticed.

Journal de Pharmacie et de Chimie.
December, 1879.

Mode of Action of Substances Employed in the Antiseptic Dressing of Wounds.—MM. Gosselin and Bergeron.—The authors ascribe the beneficial action of these dressings not merely to the destruction of ferments, but to the coagulation of the albumen.

Earthy Tritungstates.—M. Jules Lefort.—The author describes the tritungstates of barium, strontium, calcium, magnesium, aluminium, and also certain metallic tritungstates; those of manganese, iron, chrome, nickel, cobalt, and cadmium.

The Alkaloids of the Cinchonas.—M. Jungfleisch.—An account of anhydrous quinine and of several of its hydrates.

Poisonous Properties of Phenol.—Dr. Binnendijk.—This acid acts in a direct manner upon the cerebro-spinal nervous system, and especially upon the respiratory centre, and is not without influence upon the blood. It is partly transformed in the organism, yielding hydroquinon, pyrocatechin, &c. A portion of the phenol seems to be oxidised.

Moniteur Scientifique, Quesneville.
December, 1879.

New Violet Colouring Matter.—M. Prud'homme.—The compound in question, known in commerce as "soluble violet," or "anthracen violet," is closely analogous to gallein, but is distinguished by its non-transformation into cœrulein, its resistance to boiling soap-lye upon tissues mordanted with iron and alumina, and its greater permanence when exposed to light. It contains no nitrogen, and is probably a substitution-derivative of gallein.

Account of Foreign Chemical Researches.—M. E. Noëlting.—Abstracts from the *Berichte der Deutschen Chem. Gesell.*

The Alkaloids of the Aconites.—Dr. Alder Wright and A. P. Luff.—From the *Journal of the Chemical Society.*

Experimental Possibility of the Dissociation of the Non-Metallic Elements.—M. Raoul Pictet.—After a number of important preliminary considerations, the reader is referred to the author's original memoir published in the *Archives des Sciences Physiques et Naturelles de Genève*, October 15th, 1879.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.

No. 71, November, 1879.

Report by M. de Luynes on a Pallet for the Assay of Oils by MM. Dufour and Ronaix.—The authors use the well-known reagents, such as sulphuric or nitric acids of suitable strengths, or a mixture of these two acids, or

metallic nitrates, or nitrous acid along with the nitric acid. The sole novelty in the procedure is the method of applying these tests. They have constructed a rectangular pallet, white porcelain, in which are reserved cavities 5 millimetres in depth and 4 centimetres in diameter. The flat parts of the pallet are black, so that the double range of cavities appear in white on a black ground, which makes the observation of the colours more easy. To assay oils, into each cavity are poured twenty drops of the oil, which fills them all to the same level. Then upon the oil and in the centre of each cavity are poured 8 drops of the acid liquid. In the course of eight or ten minutes, and at the temperature of 15° to 25°, the peculiar colouration of each oil shows itself.

Memoir on the Destruction of Burrs in Wool.—M. René Leblanc.—The author describes all the known methods for this purpose, even giving a list of patents taken. He prefers dry hydrochloric acid gas, aluminium chloride, and acid phosphate of lime, and safer in their action than free liquid acids, however diluted. He then proposes a method of his own: the wool is to be thoroughly dried before being freed from the grease, to be passed in that state under heavy rollers which crush the burrs, and then treated in the ordinary manner.

Die Chemische Industrie.
No. 11, November, 1879.

This issue is chiefly taken up with an account of a congress on the German patent laws.

Chemiker Zeitung.
No. 51.

On Vanillin.—MM. Haarmann and Reimer.—An ethereal extract of vanillin, whether obtained from vanilla pods or the oxidation product of coniferin, if shaken up with a solution of sodium bisulphite, yields up its vanillin to the latter, exactly as in the case with the other aldehyds. The compound of vanillin with the bisulphite is then destroyed by the addition of sulphuric acid, and the pure vanillin is withdrawn from the liquid by agitation with ether. In this manner the actual vanillin, either in the natural pods or in artificial samples, may be accurately determined. The authors maintain that their artificial vanillin is chemically and physically identical with the natural product.

On Crystalline Calcium Fluoride.—Prof. Franz Stolba.—The author prepares the crude salt by heating in a flask on the water-bath an intimate mixture of three parts elutriated fluor-spar and one part of finely ground infusorial earth, covered with a mixture of seven parts crude hydrochloric acid and three parts water, and allows them to re-act for an hour. Calcium chloride is produced along with a solution of calcium silico-fluoride, from which the crystals are deposited on evaporation.

Volumetric Determination of Sulphuric Acid in Sulphates.—Dr. H. Precht.—The process of Wildenstein consisted in precipitating neutral solutions of sulphates with barium chloride, and detecting the end of the reaction by means of potassium chromate, whose yellow colour disappears owing to the formation of barium chromate as soon as all the sulphuric acid has been precipitated. Precht modifies this process by using an excess of barium chloride, precipitating with excess of potassium chromate, and determining the excess of the latter by means of a solution of a ferrous salt. To the liquid precipitated with normal barium chloride (104 grms. anhydrous salt in 1 litre) are added 10 c.c. potassium chromate of such a strength that 2 volumes correspond to 1 volume of the barium chloride; soda lye is added till the red colour turns to a yellow, the solution is cooled, and a little more potassium chromate is added, the half-litre flask is filled, and 1 part of the filtrate is titrated with ferrous sulphate

in presence of free sulphurous acid. The final reaction—the change of the colour—is distinctly perceptible, and is observed by placing a drop of the liquid upon a porcelain plate and touching it with a drop of solution of potassic ferricyanide till the blue colouration appears.—*Zeitschrift Anal. Chemie*, xviii., 521.

Modification of the Determination of Nitrogen by Dumas.—B. Reinitzer.—Requires the accompanying engraving. (See *Bericht. Oesterr. Gesell. Foerder Chem. Industrie*, i., 36.

The Electrolytic Determination of Cobalt, Nickel, and Copper, and its Advantages.—W. Ohl.—The author points out the sources of error and the tediousness unavoidable in the quantitative determination of these metals. He describes the small dynamo-electric Gramme machine which he employs, in default of which a Meidinger-Pinkus battery with superimposed flasks (each holding 5 kilos. sulphate of copper) may serve. In the determination of copper the needle of an interposed sinus-compass should diverge from 35 to 40, for samples of nickel 45 to 50, and for rich cobalt solutions 60 and upwards.—*Zeitschrift Anal. Chemie*, xviii., 521.

Platinum Attacked by Melting Alkaline Carbonates.—Dr. L. de Koninck.—A platinum crucible during the fusion of 6 grms. sodium potassium carbonate over a Bunsen burner and then over a blast-lamp lost 1 m.g., and on the addition of 1 grm. artificial manganese peroxide there was a further loss of 1.7 m.g. 23 grms. sodium potassium carbonate melted over a blast for 15 minutes withdrew 3.8 m.g. from the crucible. The fused mass was grey, yielding a yellowish solution, and contained exactly as much platinum as the crucible had lost.—*Zeitschrift Anal. Chemie*, xviii., 569.

Examination of Mineral Lubricating Oils.—Dr. Oscar Brenken.—The author determines:—1, the specific gravity; 2, the temperature at which inflammable and continuously combustible gases are evolved. For this purpose he heats the oil on the sand-bath in a crucible 6.4 c.m. wide and 4.7 in depth, filled to 1.2 c.m. from its edge, and after the experimental temperature has been attained he removes it from the sand-bath and passes a small gas flame over it, as in Hannemann's petroleum-test, trying it first from 5 to 5 degrees and afterwards from 2 to 2. Oils which foam strongly when heated are unfit for many purposes. He observes, further:—3, the point of congelation; 4, the undissolved constituents on dissolving 10 c.c. in an equal vol. of ether, filtering and weighing the washed residue; 5, the reaction with soda-lye of sp. gr. 1.40; 10 c.c. of the oil are well shaken up with 5 c.c. of the soda-lye and heated in the boiling water-bath. After repeated shaking the lye must remain clear, and its volume must not be altered. The test-tube used in this experiment must be absolutely free from grease; 6, the reaction with nitric acid of sp. gr. 1.45; on agitating equal volumes no rise of temperature, or but a very slight one, should be perceptible. This test shows the absence of tar-oils; 7, reaction with sulphuric acid, sp. gr. 1.53; equal volumes are shaken up and heated in the water-bath, when the acid should take merely a pale-yellow colour. A brown or black colouration shows imperfect refining, or, along with No. 6, the presence of tarry oils; 8, behaviour on shaking with water; the water must remain clear, free from a whitish turbidity, and should not have an acid reaction.—*Zeitschrift Anal. Chemie*, xviii., 546.

Presence of Alcohols in Unfermented Fruit-juices.—Dr. Gutzeit.—The author extracted the unripe fruits of *Heracleum giganteum* with ether free from alcohol. In the watery stratum of the extract free ethylic and methylic alcohol were detected. In the ethereal stratum was found butyric-ethyl-ester along with solid hydrocarbons of the formula $C_n H_{2n}$.—*Jena Zeitschrift Naturw.*, xiii., 1.

Les Mondes, Revue Hebdomadaire des Sciences.

No. 13, November 27, 1879.

Properties of the Gum of the Euphorbiaceæ.—Certain Euphorbiaceous plants growing in Natal yield a gummy matter, which if dissolved in alcohol and applied to metallic objects preserves them from the corrosive action of the sea or of brackish waters. It also secures articles of wood, &c., from the ravages of the white ants.

Superficial Viscosity of Liquids.—J. Plateau.—Continued from *Les Mondes*, Nov. 13 and 20.

No. 14, December 4, 1879.

This number contains no chemical matter which has not already appeared elsewhere.

No. 15, December 11, 1879.

Electro-magnetic Rotation of the Plane of Polarisation in Gases.—A. Kundt and W. C. Röntgen.—Not susceptible of useful abridgment.

No. 16, December 18, 1879.

The Active Matter of Malt, or Maltin and Diastase.—M. Dubrunfaut.—Diastase is in reality merely an altered product, which accidentally retains about 1 per cent of maltin. The latter substance is the true active principle of malt.

Acceleration of Tanning by Means of Phosphoric Acid.—E. Ador.—Phosphoric acid, by hindering the tanning of the albuminoid substances, enables the tannin to traverse the skins more rapidly.

No. 17, December 25, 1879.

The "Egasse" Disinfectant.—This disinfectant is said to fix instantly all the gaseous or liquid bodies which in a mass of putrescent organic matter are the sources of all infections. In addition, it can be used as a remedy for the stings of insects, for the destruction of the phylloxera and other vermin, for the preservation of timber, &c. As a disinfectant its power is asserted to be 16 times greater than that of phenol, 14 times greater than that of cresylic acid, &c. No light whatever is thrown upon its nature and composition. It must be noted that in the account of this disinfectant, carbonic acid is classed among the "deleterious miasms" escaping from cess-pools.

Reimann's Färber Zeitung,

No. 46, 1879.

This issue contains nothing of general interest.

MEETINGS FOR THE WEEK.

- MONDAY, 12th.—London Institution, 5.
— Royal Geographical, 8.30.
— Medical, 8.30.
TUESDAY, 13th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
— Civil Engineers, 8.
— Photographic, 8.
— Anthropological Institute, 8.
WEDNESDAY, 14th.—Society of Arts, 8. T. Bolas, F.C.S., "Modern Autographic Printing Processes."
— Microscopical, 8.
— Society of Public Analysts, 8. "On the Analysis of two Samples of Butter Several Hundred Years Old," by G. W. Wigner, F.C.S., and Prof. Church. "On the Composition of Unfermented Wines of Commerce," by J. Carter Bell, F.C.S. "On the Estimation of Fat in Milk," by W. M. Hamlet, F.C.S.
THURSDAY, 15th.—Royal, 8.30.
— Chemical, 8.
— Royal Institution, 3. H. H. Statham, "Modern Architecture since the Renaissance."
— Royal Society Club, 6.30.
FRIDAY, 16th.—Royal Institution, 8. Prof. Dewar, "Investigations at High Temperatures," 9.
SATURDAY, 17th.—Royal Institution, 3. Prof. T. Rupert Jones, "Coal."

BURNDEN CHEMICAL WORKS, AND OTHER PREMISES, AT BOLTON, LANCASHIRE.

VALUABLE LEASEHOLD LAND, BUILDINGS, WORKING PLANT, MACHINERY AND APPARATUS FOR MANUFACTURING SODA ASH, SULPHURIC ACID, MURIATIC ACID, BLEACHING LIQUOR, SULPHATE OF COPPER AND BRONZE ASH; Also VALUABLE DYE WORKS, AND FOURTEEN MESSUAGES OR DWELLING-HOUSES.

LOMAX, SONS, & MILLS, will SELL by PUBLIC ACUTION (by order of the Mortgagees), at the THATCHED HOUSE HOTEL, New Market Place, in the City of Manchester, on Tuesday, the 20th day of January, 1880, at Two for Three o'clock in the afternoon (unless previously disposed of by private contract), subject to such conditions as will be then and there produced,—

ALL THOSE VERY VALUABLE AND EXTENSIVE CHEMICAL WORKS, situate in Burnden Street, Manchester Road, Bolton, in close proximity to the Lancashire and Yorkshire Railway, known as the

BURNDEN CHEMICAL WORKS,

consisting of Ore-breaking Shed, Pyrites and Sulphur Burning Houses, Sulphate of Copper Sheds, Salt Cake Shed and Store, Bronze Ash House, Chemical Shed, Rectifying and Cooling Houses, Loading Shed, Ash Sheds, Cooper's Shop, Ash Packing Place, Joiners' and Mechanics' Shops, Engine and Boiler-houses, Wheelwright's Shop, Smithy, Nitre Store, Stabling for 17 Horses, Provender Place, Hay and Straw Stores, Private and General Offices, Laboratory, Machine-house, Chimneys, and other erections; together with the Steam-boilers, Steam-engines, Millwright's Work, Steam, Gas, and Water Piping belonging thereto; and also the whole of the Working Plant, Machinery, and Apparatus set up, affixed, and belonging to the several departments of the said works and premises, and capable of manufacturing monthly upwards of 1000 tons of Soda Ash, Sulphuric Acid, Muriatic Acid, Bleaching Liquor, Sulphate of Copper, and Bronze Ash. The Working Plant consists of Black Ash Furnace, four Cast Iron Lixivating Vats, Steam Jets, Liquor Wells, Liquor Settler, Evaporating Pan, Salting Down Pan, Soda Ash Finishing Oven, pair of Ash Crushers, and Rotating Sieve; 14 Pyrites Burners; two Nitre Ovens, Lead Concentrating Tower, three Vitriol Chambers and three Nitre Recovery Towers, Air Engine by Daglish and Co.; four Acid Eggs, four Liquor Cisterns, Boiling-down Pan, eleven Sulphur Burners, Vitriol Wash Tower, five Vitriol Chambers, and two cisterns, three Boiling-down Pans, Sixty Rectifying Furnaces, Salt-cake Pot and Roaster, eight Stone Acid Cisterns, and two Stone Condensing Towers, seven Stone Chlorine Stills, five Stone Bleaching Vessels, with Gearing and Agitators, Liquor Well, Stone Acid Cisterns, Lead and Iron Chemical Tanks, Feed Pump, Ore Grinding Mill, Riddling Machine, Copper Calcining Furnace, Wagon Hoist, Tramways and Turntable, Wash Tower, six Extracting Vats, Liquor Settler, two Precipitating Tanks, Copper Vat, Precipitate Roasting Furnace, three Evaporating Pans, thirteen Lead Crystallising Pans, Liquor Well, Steam Pump, Drying and Sampling Benches, two Boiling-down Pots and Furnaces, Drying Furnace, Boiling-down Pan and Furnace, Drainer and Well, Mortar Mill, Chaff-cutting Machine, Ten-ton Wagon Weighing Machine, and Office Fixtures; together also with the THREE PLOTS OF LAND, forming the site thereof, and occupied therewith, and containing altogether 24,318 square yards, or thereabouts.

The Premises are Leasehold, and are held for the residue of three several terms of 990 years, 999 years, and 990 years, subject to three several ground rents amounting in the aggregate to £120.

The Buildings, which are brick-built and slated (the sheds being erected on brick pillars), are excellently adapted for the purposes of the business, a large sum of money having been expended in their construction (a considerable portion of such sum within the last few years), and are most eligibly situated, the Lancashire and Yorkshire Railway being immediately contiguous to the Works. Gas and Water Mains are laid throughout, and there is a never-failing supply of water for all purposes. Immediate possession can be had.

Also those valuable PREMISES, situate in Burnden-fold aforesaid (adjoining the last-described premises), formerly used as a DYE WORKS, with the STEAM BOILER, STEAM ENGINE, SHAFTING, GEARING, PIPING, and FIXTURES thereto belonging, which, with the ELEVEN COTTAGES adjoining, being Nos. 3, 5, and 7, Haslam-street, 11, 13, 15, and 17, and 9, 25, 27, and 29, Burnden-fold aforesaid, and the site whereof contain 1302 square yards, are held for residue of term 999 years, at yearly rent of £7 14s. 1½d., and are subject to a lease for five years, at the yearly rent of £70, and will be sold subject to such lease.

Also all those TWO MESSUAGES, or DWELLING-HOUSES, and CELLAR, with the YARD, OUTBUILDINGS, and APPURTENANCES thereto belonging, situate and being Nos. 209, 211, and 213, Manchester-road, and 6 and 8 at the back, and known as Back Burnden.

Site, 248½ square yards. Term, 999 years. Apportioned yearly rent, £2 2s. 10d.

The whole of the foregoing properties will, in the first instance, be offered for sale in One Lot, and if not so disposed of will then be offered in Two Lots, viz.:—

LOT 1. Land, Chemical Works, Dye Works, and Dwelling-houses.
LOT 2. The whole of the Plant and Machinery.

The Conditions of Sale will be open for inspection at the Offices of Messrs. HOLDEN and HOLDEN, Solicitors, Bolton, seven days before the day of sale.

Full particulars of which will be given on application to Messrs. P. and J. KEVAN, Accountants, 12, Acresfield, Bolton; the AUCTIONEERS, 15, Wood-street, Bolton; Messrs. RICHARDSON and MARSHALL, Solicitors, 18, Wood-street, Bolton; or to

Messrs. HOLDEN and HOLDEN,
Solicitors,
Bolton.

The Prestolee Alkali Works, Farnworth, near Bolton, Lancashire fitted with costly plant, machinery, and apparatus for the manufacture of soda-ash, bleaching-powder and liquor, and sulphuric acid, in complete working order. With possession.

MESSRS. FULLER, HORSEY, SONS, and

CO., are instructed to SELL by AUCTION, at the Palatine Hotel, Manchester, on Friday, January 16th, at 3 precisely, in one lot (unless an acceptable offer be previously made by private contract), the PRESTOLEE ALKALI WORKS, a freehold property, having a superficial area of 95,741 square yards of land, subject to chief rents amounting to £407 13s. 9d. per annum, with the buildings, plant, machinery, and apparatus erected thereon, capable of manufacturing monthly a product exceeding in the aggregate 2000 tons of soda-ash, bleaching-powder and liquor (by Weldon's patent process), and sulphuric acid, also caustic soda and muriatic acid. The amount expended in the construction of these works has been very large. A valuation was made with great care in 1874 by Messrs. Holmes and Son, the well-known valuers, of Manchester; their estimate then amounted to £99,900, and since that time a sum exceeding £10,000 has been expended. The whole of the works and plant have been well kept, and are conveniently arranged for working. The Bury, Bolton, and Manchester Canal, which forms one boundary of the property, affords facility for economical water carriage, and the Lancashire and Yorkshire Railway is within a very short distance. There is a plentiful supply of water for all manufacturing purposes, free of cost, from the river Croal, and coals are raised from pits in the immediate neighbourhood. Pyrites, salt, lime, and limestone are all brought by boats direct to the wharf on the canal. The works may be economically worked with a small capital, as arrangements may be made with the vendors for a very considerable portion of the purchase-money to remain upon mortgage. Large profits have been realised in the past, and the high reputation of the Prestolee manufactures in the market will ensure to an energetic man, even at the existing low prices, an ample return for his capital invested, and possession of works not surpassed for completeness or compactness by any in the kingdom. May be viewed till the sale. Printed particulars may be had at the works; at the Palatine and Queen's Hotels, Manchester; of Messrs. Wake-man and Bleek, Solicitors, Warminster; of Messrs. Christopher and Son, Solicitors, 28, Argyll Street, Regent Street, W.; and of Messrs. Fuller, Horsey, Sons, and Co., 11, Billiter Square, London, who are empowered to treat for the disposal by private contract, or an offer for renting would be entertained.

POLYTECHNIC.—MARY QUEEN OF

SCOTS.—The principal events in the life of the beautiful and unfortunate Mary Stuart, illustrated by Tableaux Vivants. The descriptive poem recited by Miss Alice Burnelle. The appropriate vocal music by a Choir of Glee Singers, under the direction of Mr. Stedman, daily at 3.30 and 8.30; Edison's Loud Speaking Telephone, &c., by Mr. Daniel, 10.30; the Zulu War and the Heliograph, by Mr. King, 11; London as it Was and Is, 11.30 and 8; the Electric Light, 12; Instrumental Concert by the Paggi Family, 2.30 and 7.30; Popular Optical Lecture, 3, by Mr. King: Phenomena of Light and the Ghost; by Mr. King, 6.30. Open 10 till 1, 2 till 5, and 6 till 10. Admission Reserved Seats, 2s., 1s., and 6d.

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CONTENTS.

The History of Evolutionism. Scientific Progress of the Past Year. An Address delivered before the Royal Society by Wm. Spottiswoode, D.C.L., LL.D., P.R.S. The Hon. Henry Cavendish; his Life and Character. English and American Physique. By George M. Beard, M.D. On Water and Air. By Prof. Tyndall, D.C.L., LL.D., F.R.S. The Thomas and Gilchrist Process for Eliminating Phosphorus in the Manufacture of Steel. By W. Mattieu Williams, F.R.A.S. Professor Barff's Process for Preventing the Corrosion of Iron. Analyses of Books, &c., &c.

London: 3, Horse-Shoe Court, Ludgate Hill.

THE CHEMICAL NEWS.

VOL. XLI. No. 1051.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Continued from page 2.)

III. USE OF THE PERIODIC LAW FOR THE DETERMINATION OF THE ATOMIC WEIGHTS OF THE LITTLE KNOWN ELEMENTS.

WHEN an element has very little power of combining with oxygen, and when only a small number of its other compounds are known, it is necessary, for finding the atomic weight of this element, to learn its equivalent (relatively to hydrogen), to determine the physical properties of the body in a simple state (such as its capacity for heat), or those of its compounds (such as vapour density and capacity for heat), or to find a case of isomerism. As some of these determinations have many practical difficulties, and because in a great number of cases the methods of working are defective, the atomic weights of many elements have been determined according to very uncertain data. Therefore, because we are not certain of a sufficient number of properties, we often give the formula RO to strongly basic bodies (this is the reason why the oxides of Ce, Yt, Di, and La, particularly, have been given this formula), the formula R_2O_3 to weak bases (as oxide of uranium), and RO_2 , R_2O_5 , and RO_3 to acid oxides. This method has been admitted as incorrect, since the formula of oxide of beryllium has been found to be BeO , with feeble basic properties, and the formulæ of the highly basic oxides of Th and Zr to be RO_2 .

Lastly, it will be sufficient to remember that the highest compounds of oxygen, such as OsO_4 and RuO_4 , have, only weak acid properties.

It is absolutely certain that the basic and acid properties of oxides are determined not only by the number of atoms of oxygen contained therein, but also by the properties of the element. The proof of it has been given in the preceding developments of the nature of the periodic law. The element which comes after Th (twelfth series) and whose atomic weight is about 235, ought to have basic properties, even under the form of oxide, R_2O_5 . It is for the same reason that many of the formulæ of oxides, above all the oxides of the rare elements, which have not yet been thoroughly studied, such as, In, Ur, Ce, La, Di, Yt, and Er, are not not legitimate. Hardly any clear cases of isomerism have been observed in the oxides, and some of the isolated facts of this kind, such as the isomorphism of oxide of zirconium, and oxide of beryllium with alumina, cannot lead us to any certain conclusions.

In general, it is necessary to observe that isomorphism gives only a feeble support to conclusions on the atomic composition of bodies; the case of isomorphism presented by bodies of different composition, and the phenomena observed by Marignac, which have been already mentioned, are already sufficient proof. Without pushing any further into this vast and obscure region, I will simply remark that even the capacity for heat, as much in simple as in compound bodies, does not always give us very clear results. Thus we have left only two kinds of permanent criteria for finding the atomic weights; the determination of the vapour density of a large number of compounds of the given element, and a means of controlling, of a purely chemical nature, or conclusions drawn from the composition of the different forms of oxides and from the discovery of analogies with sufficiently well-known ele-

ments, &c. The capacity for heat and isomorphism can only be looked upon as auxiliary means. These methods are almost completely wanting for the above-mentioned rare elements; because, except a few exceptions, they only give volatile compounds, which have been too little studied to authorise any conclusions on the analogies.

The periodic law comes to our aid in cases of this kind, by offering us a new regular relation between the chemical properties and the atomic weights. This law being admitted, we can determine the atomic weight of an element of which the equivalent and some properties are known. If we multiply the equivalent E of an element deduced from the highest oxide (its composition being E_2O , and that of the chloride ECl) by 1, 2, 3, 4, 5, 6, 7, we obtain the values of all possible atomic weights of this element, and one of these numbers expresses the true atomic weights; this number E_n corresponds to a place as yet unoccupied in the system, and at the same time to the atomalogy of the element. For, to judge of it according to what we know at present, a particular place in the system can only receive one element, and the atomic analogies are of a very simple nature.

Let us suppose that an element gives a basic oxide, not very energetic, and whose equivalent is 38. (It must not be forgotten that this number has a slight inevitable error.) We want to know the atomic weight of the element or the formula of its oxide. If we adopt the formula R_2O for the oxide, $R=38$ and the element belongs to the first group; but the place is already occupied by $K=39$; and, further, the atomic analogy indicates a soluble and energetic base for this place. If we adopt for the oxide of the formula RO the atomic weight = 76, the atomic weight will not allow it to be in the second group, because $Zn=65$, and $Sr=87$, all the places for elements of low atomic weights are filled in this group. If we adopt for the oxide the formula R_2O_3 , then the atomic weight, $R=144$, and the element goes into the third group, where there is in fact between $Cd=112$ and $Sn=118$ a place for an element whose atomic weight is near 114. The oxide of the given element should, according to its atomalogy with Al_2O_3 and Tl_2O_3 , as well as with CdO and SnO_2 , possess feebly basic properties. Therefore, this element should be placed in the third group. If we adopt for the oxide the formula RO_2 , the atomic weight = 152, the element in question should not be placed in the fourth group, because only an element having an atomic weight 162, and feebly acid properties, could adapt itself to the empty place in this group (to form the link between PbO_2 and SnO_2). There is still in the eighth group a place for an element whose atomic weight would be 152; but this element forming the transition from Pd to Pt, should have such properties like them, which could not remain hidden. If then the given element does not possess these properties, the above-mentioned atomic weight does not do for it, any more than the place in the eighth group. If we adopt for the oxide the formula R_2O_5 , then $R=190$, which will not do for the fifth group, because $Ta=182$ and $Bi=208$. Further, Ta and Bi possess, in the form of R_2O_5 , acid properties.

The forms of oxides RO_3 and R_2O_7 do not correspond any better with our element. The only atomic weight which does for it is, therefore, 114, and the formula of its oxide is R_2O_3 . Such an element is Indium. Its equivalent, according to Winkler, is 37.8; therefore we ought to accept the number 113 for its atomic weight, and the formula In_2O_3 for the composition of its oxide. Up to the present we have made $R=75$, and we have adopted InO for the formula of its oxide. Al and Tl are indium atomalogues of the third group, Cd and Sn atomalogues of the seventh series.

Let us compare the apparent properties of indium, deduced from atomalogues, with the properties really observed.

The atomalogues of indium, Cd, and Sn being easily reducible (they can even be reduced from their solutions by zinc), indium should also be reducible. As Ag (seventh

series, first group) is more difficultly fusible than Cd, and Sb more difficultly fusible than Sn, it is necessary, according to the atomalogy of Ag, Cd, In, Sn, that indium should be more easily fusible than cadmium. In fact, indium melts at 176° . Ag, Cd, and Sn are of a greyish-white colour, and so is indium. The density of Cd is lower than the density of Ag, the density of Sb is lower than that of Sn, the density of indium should then be a little lower than the average between Cd and Sn. In reality it is so. The density of Cd = 8.6 , that of Sn = 7.2 ; therefore the density of In ought to be less than 7.9 . It has been found to be 7.42 . As Cd and Sn oxidise at red heat, and do not rust in the air, these properties should also be found in In, but more feebly than in Cd and Sn, because Ag and Sb oxidise still more difficultly.

All that has just been said has been verified by experience. We arrive at the same conclusions in comparing In with Al and Tl; thus the density of Al = 2.67 , that of Tl = 11.8 , the average density would be 7.2 .

We now pass to the properties of the oxide, and to the reactions of the salts. Indium and its atomalogues are found in the odd series; this is why the higher oxides cannot be strongly basic; the basic character should be more feeble in In_2O_3 than in CdO and Tl_2O_3 ; it should also be stronger than in Al_2O_3 and SnO_2 . These conclusions are confirmed by the following facts: The oxides of Al and Sn dissolve in alkalies, forming definite compounds, while the oxides of Cd and Tl are insoluble in alkalies; In_2O_3 is soluble in alkalies, but without forming any definite compound. The oxides of Cd, Sn, Al, and Tl are difficultly fusible powders, like In_2O_3 . Hydrate of In_2O_3 forms, as could be foreseen, a colourless gelatinous mass. The oxides Al_2O_3 and SnO_2 are easily precipitated from saline solutions by carbonate of barium; it is the same with In_2O_3 . Sulphuretted hydrogen precipitates Cd and Sn from their acid solutions; In is also precipitated. All these reactions have been confirmed experimentally.

Here are a few more facts deduced from atomic analogies, although they have not been observed, or have not been sufficiently studied: Indium ought to furnish indium-ethyl InAe_3 , prepared in the ordinary way, because CdAe_2 and SnAe_4^* exist; this compound, according to the atomic analogies, ought to boil at about 150° . As two elements, Sn and Tl, atomically analogues to indium, each give (independently of the higher oxide) a lower more basic oxide, indium would apparently also give a suboxide, InO or In_2O , turning in the air into In_2O_3 . Apparently chloride of indium, InCl_3 or In_2Cl_6 , cannot evolve chlorine when heated, because SnO_4 does not possess that property. The sulphate of cadmium forming with sulphate of potassium a double salt of the form $\text{K}_2\text{Cd}(\text{SO}_4)_2$, and aluminium giving an alum $\text{KAl}(\text{SO}_4)_2$, indium would no doubt give an analogous double salt; in any case, it is impossible to foresee if this salt will be isomorphous with alum.† To control the exactness of the modifications to be introduced into the atomic weight of indium, and into the formula of its oxide, conformably to the developments given above, I determined the specific heat of this body, and I found it to be 0.055 , which is in accord with the periodic law.‡ A little time before Bunsen employed his new and elegant calorimetric process for determining the specific heat of indium. The two numbers agree. Bunsen found 0.057 . We can then, without the slightest doubt, apply the periodic law for correcting the atomic weights of elements

* Cadmium-ethyl has not been studied much; still, for many reasons, it merits research. Atomically analogous to Zn and Hg it should boil at 130° . The study of indium-ethyl and of thallium-ethyl would throw a new light on aluminium-ethyl, unhappily little known.

† Only having a small quantity of indium, I have, up to the present, only been able to make one incomplete experiment; an acid solution of sulphate of indium, with an equal quantity of K_2SO_4 , left under sulphuric acid, after the addition of alcohol, give crystals formed of microscopic cubes. I recognised it, not only by its form, but also by the absence of double refraction of light.

‡ Bulletin de l'Académie des Sciences de Saint Petersburg, 1870, p. 445.

which are little known. Because of this certainty I will offer some considerations relative to other elements. I will only remark now that the periodic law has assigned a determinate position to all the elements more or less known, at the present time, and that this fact, far from being of small value, corroborates the law emphatically. Before leaving indium I would notice this: In is an analogue of Zn and Cd up to a certain point; it accompanies them in nature, and it is near them in the system; it is nearly the same which takes place for Nb relatively to Ti and Zr.

ON THE SEPARATION AND ESTIMATION OF CADMIUM IN THE PRESENCE OF ZINC; WITH REMARKS UPON THE SEPARATION OF COPPER, CADMIUM, AND ZINC.

By CHRISTOPHER CLARKE HUTCHINSON, Assoc. R.C.Sc.

A SHORT time ago I conducted an investigation on the influence of mass in chemical reactions, particularly directing my attention to the influence which copper, cadmium, and zinc exercise upon each other, and their mutual relations during precipitation. The two latter metals seem to have many points in common with each other, and show great similarity in their behaviour towards most chemical reagents.

The investigation necessitated an examination of the methods in use for the separation and estimation of these metals. Most of such processes proved very unsatisfactory; I was therefore obliged to make some experiments in order to determine the methods most reliable. In this paper I shall give the results I obtained, and the methods found most accurate. Standard solutions of the pure crystallised sulphates made from the pure metals were used throughout.

Separation of Cadmium and Zinc.—If an acid solution containing both of these metals be treated with sulphuretted hydrogen, the cadmium should be precipitated as the sulphide, while the zinc should remain in solution. The reaction, however, is not so complete as indicated; and its behaviour is dependent upon the amount of acid present in excess. If too great an amount of acid be present, the cadmium will only be imperfectly precipitated; while, on the other hand, if the solution be not sufficiently acid, a portion of the zinc undergoes precipitation as sulphide with the cadmium. A method of separation based upon this behaviour will only yield satisfactory results when the relative quantity of each metal present is approximately known, so that the quantity of acid can be adjusted (whether the one or the other be present in a larger quantity), so as not to prevent the total precipitation of cadmium or favour that of zinc. Even when very carefully conducted, it is often found necessary to re-dissolve the sulphides obtained, and to re-precipitate, so as to thoroughly eliminate every trace of zinc.

The method of separation described by Fresenius, and usually given by other authorities, is also open to the objection of not yielding concordant results, even though every precaution be observed in the process and the quantity of the reagents be carefully adjusted. This method is based upon the insolubility of the hydrated oxide of cadmium in an alkaline solution containing tartaric acid. To the hydrochloric acid solution of the two, as near as possible neutral, tartaric acid is added; sufficient sodium or potassium hydrate is then added until the solution is distinctly of an alkaline reaction. If this solution be boiled for about two hours, the hydrated oxide of cadmium separates out on cooling, the zinc being left in solution. After filtration, the hydrated oxide is ignited and weighed as the oxide (CdO). The zinc may be precipitated as the sulphide by ammonium sulphide, or titrated with sodium sulphide.

Two examples may be given, showing the mutual variability in estimations which should each have yielded the same result. In each case the determination of the amount of cadmium contained in a mixed precipitate of zinc and cadmium was the object in view.

	Grm.		Grm.
No. 1 gave CdO ..	0.0805	or Cd ..	0.0704
„ 2 „ CdO ..	0.0615	„ Cd ..	0.0538
Differences ..	0.0190		0.0166

The next example given shows the result of experiments upon precipitates of the same nature, but containing a larger amount of each substance.

	Grm.		Grm.
No. 3 gave CdO ..	0.194	or Cd ..	0.1697
„ 4 „ CdO ..	0.1165	„ Cd ..	0.1019
Differences ..	0.0775		0.0678

The differences here shown, taken in relation with the comparatively small amount of the metal present, are serious, and indicate the method as unreliable. The subsequent zinc-estimation in all of these cases showed that in *no instance* was the whole of the cadmium present precipitated; the error, therefore, relatively to the *total* cadmium present, was even larger than above shown. This was amply confirmed by other experiments, in all of which the solubility of the hydrated oxide of cadmium, and its subsequent precipitation with the zinc as sulphide, was inevitably found; in many instances the quantity was so great as to impart quite a deep yellow colour to the otherwise white zinc sulphide.

Experiments were now made in which advantage was taken of the difference of behaviour of the two sulphides when boiled with a solution of potassium cyanide, sulphide of zinc being very *soluble* and the sulphide of cadmium *insoluble* in that reagent. This method appears, at first sight, to be both convenient and accurate. Several trials, however, showed that it was unsatisfactory; the difficulty of obtaining pure potassium cyanide free from iron, the subsequent destruction of the cyanide, and the large excess of potash salts left, with their effect upon the estimation of the zinc, gave results far from approaching the accuracy desirable.

After several trial experiments, the following method was adopted. It is based upon the difference of action of ammonium carbonate upon the precipitated metallic carbonates, and yields accurate and concordant results. From considerable experience it can be recommended by the author for the separation and individual estimation of the two metals.

The hydrochloric solution containing the two is evaporated to dryness on the water-bath, preferably in a platinum dish, so as to expel the excess of acid. The dried chlorides are then re-dissolved in about three times the amount of distilled water necessary for their complete solution; the aqueous solution is heated upon the water-bath almost to boiling, and a concentrated solution of pure sodium carbonate is added until there is a distinct alkaline reaction. At this temperature a granular precipitation of the carbonates of both metals occurs, which very quickly settles down and allows filtration almost immediately. After standing a short time the carbonates are thrown upon a filter and thoroughly washed, the nature of the precipitate rendering this very easy of execution. The washed carbonates are then re-transferred to the platinum dish with just a sufficient quantity of wash-water to completely remove them from the filter. If this be carefully done, it is by no means difficult to effectually remove the whole of the precipitate without loss from adherent portions. A saturated solution of pure ammonium sesquicarbonate is then added in considerable excess; after stirring, the mixture is allowed to stand in a warm chamber for about six hours. The whole of the zinc carbonate

passes into solution; the cadmium carbonate, being insoluble, rapidly settles down. It is then thrown upon the same filter as before used, and washed: the filtrate and washings contain the zinc. After drying, it is ignited in a porcelain crucible and weighed as the oxide (CdO). Care must be taken to avoid the presence of any small particles of filter-paper during ignition; otherwise reduction of the oxide will occur, and loss by volatilisation as the well-known brown cadmium vapour. The filter-paper is saturated with ammonium nitrate to prevent volatilisation of the small amount of adherent particles; it is then ignited in the usual way, either upon the lid of the crucible and not in contact with the oxide, or, more conveniently, in a smaller crucible: carefully conducted, little or no loss by reduction occurs. The filtrate containing the zinc is evaporated down to a small bulk to drive off the excess of ammonium salts, diluted to a convenient extent, and titrated with a standard solution of sodium sulphide in the usual manner.

The following examples may be given to show the success of this method. 10 c.c. of solution of each of the pure sulphates containing known amounts were mixed, excess of ammonia added, and their complete precipitation effected by addition of ammonium sulphide in excess. The washed sulphides were then dissolved in hydrochloric acid, and the solution treated as above described. The following are the results obtained in each instance:—

No. I.		Grm.
Cadmium actually present	0.112
„ „ found	0.117
Error	0.005+
Zinc actually present	0.065
„ „ found	0.065
No. II.		
Cadmium actually present	0.1163
„ „ found	0.1239
Error	0.0076+
Zinc actually present	0.065
„ „ found	0.065

To ensure rigorous comparison in each of the above cases, the amount of each metal present in the 10 c.c. of solution taken was directly determined by taking at the same time a corresponding separate quantity of each of the solutions: the zinc solution was titrated with sodium sulphide, and the cadmium precipitated as the carbonate, ignited, and weighed as the oxide.

Other examples could be given, in which the error in the cadmium estimation did not exceed 0.5 of a milligram. in 0.112 gm. total amount present; the zinc estimations were equally as favourable as the two examples quoted. In an extended series of separations of unknown quantities of the two sulphides the results were exceedingly concordant with each other, the relative quantity of each present being exceedingly variable.

Separation of Copper, Cadmium, and Zinc.—The methods of separation of copper from either cadmium or zinc, or both of these metals, were in most instances carefully examined. That which presents the most favourable features is by means of potassium sulphocyanide, being alike convenient and accurate.

The remarks upon the separation of zinc from cadmium by sulphuretted hydrogen in an acid solution apply in a similar manner to the present case, its separation from copper. Most processes which effect the separation of copper and zinc can be also applied in the case of copper and cadmium. Special attention was given to the two following methods of separation:—(1) That based upon the extremely sparing solubility of the sulphide of copper in a dilute solution of sulphuric acid (1 of H₂SO₄ + 5 of H₂O), and the complete solubility of the other two sulphides. (2) That founded upon the precipitation of the copper, in presence of the other two metals, as cuprous

sulphide by the reagent sodium thiosulphate ($\text{Na}_2\text{S}_2\text{O}_3$). Neither of these was found convenient, or the separation by any means so complete as is desirable, especially when small quantities are required to be operated upon.

The sulphocyanide process was conveniently conducted as follows:—The nitric acid solution containing the three metals, or any one of the remaining two with copper, was evaporated to dryness to expel the excess of acid. A sufficient quantity of sulphurous acid was then added to effect re-solution with an equal bulk of distilled water. The potassium sulphocyanide was then added in the smallest possible excess: this is accompanied with the formation and precipitation of the pink-white cuprous sulphocyanide, $\text{Cu}_2(\text{CyS})_2$. After allowing to settle, the precipitate was thrown upon a filter and thoroughly washed. It was then re-transferred to an evaporating basin, dissolved in a small quantity of nitric acid, evaporated to dryness, re-dissolved in water, excess of ammonia added, and finally titrated with a standard solution of potassium cyanide in the usual manner. The filtrate and washings containing the cadmium or zinc, or both, was evaporated to a small bulk on the water-bath, and at this temperature precipitated by pure sodium carbonate. Their separation was then proceeded with as before described. If cadmium be present, its precipitate is washed, dried, ignited, and weighed as the oxide. If zinc only be present it can be similarly treated, or re-dissolved in hydrochloric acid and titrated by sodium sulphide. Considerable experience proved this method to be the most expeditious, convenient, and accurate of those usually employed for the separation of these metals.

The experiments were made in Prof. Galloway's laboratory at the Royal College of Science.—*Philosophical Magazine*, December, 1879.

ON THE DETECTION OF ORGANIC MATTER IN WATER.

By F. TIEMANN and C. PREUSSE.

THE authors premise the statement that according to prevailing opinion putrescent organic matter is the most suitable medium for the development of disease ferments which may be distributed by air and water as well as by animals. Whilst the foreign mineral matter in water is easy to detect and to determine, the recognition of organic impurities, and still more their quantitative estimation, is attended with the greatest difficulties. Among these the bodies formed by fermentation from the albuminoids, the fats, and the carbohydrates deserve particular attention. Among the products of the albuminoids the following have been recognised:—Peptons, amido-derivatives of mono- and bibasic acids of the fatty series (leucin, asparagic acid, glutamic acid, &c.), acids of the fatty series (valerianic, butyric, &c.), trimethylamin, and the following compounds belonging to the aromatic series:—Phenol, cresol, indol, scatol, tyrosin, hydro-para-cumaric acid, &c. The fats are split up into glycerin and fatty acids rich in carbon, which are afterwards or simultaneously transformed by oxidation into acids of lower groups. From the carbohydrates there are formed by fermentation a number of alcohols (aldehyds) and acids of the fatty series. Waters in connection with cesspools may further contain the constituents of urine and their products of decomposition. Where the residues of vegetation are decaying, substances of the humus class, poor in nitrogen, will also be present. Hence the organic impurities of water may differ almost infinitely in their properties. Fixed and volatile bodies, permanent and unstable compounds, may occur together. Hence there cannot exist any simple method for determining the total amount of the organic matters in water. The following methods have been hitherto employed:—

I. Determination of Organic Matter by the Ignition of the Residue left on Evaporation at a Fixed Temperature.

The residues obtained after evaporation at 80° have been ignited with access of air, and the caustic lime thus produced re-converted into carbonate by means of ammoniac carbonate, the final loss of weight being taken as organic matter.

The authors remark that fixed organic substances may be partially decomposed during evaporation; that the mineral substances present may at 180° retain variable proportions of water; that silicic acid may expel carbonic acid which is not restored by treatment with ammoniac carbonate; and that small quantities of organic chlorides may be volatilised, whilst the organic matter may react upon nitrates and nitrites. Hence the result stands in no direct proportion to the fixed organic matter originally present in the water.

II. Method of Frankland and Armstrong.

For the description of this process and of the apparatus the reader is referred to Sutton's "Volumetric Analysis" (Second Edition, p. 259).

In this method its authors do not take into account the organic substances present in water and volatile in acid solutions. Nor do they regard the decompositions which the fixed organic compounds may undergo during the evaporation of the water under the influence of sulphurous acid and of the ferrous chloride generated. As the percentage of carbon and nitrogen in various organic compounds varies, the numbers found give no clue to the absolute quantity of fixed organic matter present in the water examined, and the results of a comparative examination of different waters by this method will be comparable only when the mixture of organic matter present in each case is alike in its nature. The method shows, however, whether nitrogenous matter, not volatile from acid solutions, is present.

III. Method of Dittmar and Robinson.

For a full description of the process the reader is referred to the *CHEMICAL NEWS* (vol. xxxvi., p. 26).

The authors consider that it can be executed more rapidly and with somewhat simpler appliances, but that it is open to the same objections. It is an approximation to the methods of F. Schulze and F. Bellamy.*

IV. Determination of Organic Matter by Means of Permanganate.

The authors describe the modifications proposed by Kubel, Schulze, and Tidy. The first of these, which they consider preferable, consists in acidulating with dilute sulphuric acid, and boiling for ten minutes with an excess of centinormal permanganate. The residue of the latter is decomposed by a centinormal solution of oxalic acid, and the excess of the latter titrated with permanganate. From the total quantity of the latter the proportion which has served for the oxidation of the oxalic acid is deducted, the remainder showing the permanganate decomposed by the organic matter of the water.

These methods take into account the volatile organic matter as well as that of a fixed nature. The different organic compounds, however, contain very different proportions of carbon, hydrogen, and nitrogen, and therefore consume very various quantities of oxygen. The authors consider that Tidy's process is "provided with a series of sources of error."

V. Fleck's Method.

In place of permanganate the authors used a solution of silver nitrate in hyposulphite of soda mixed with caustic soda. The standard solutions required in this process are very liable to change, and the final reaction is easily overstepped as in all "spotting" processes.

Further, the silver solution is directly inferior to permanganate as a reagent for organic matter.

* *Zeit. Anal. Chem.*, viii., 494 and 495.

In connection with the examination of the permanganate processes, the authors have found by direct experiment that volatile organic substances are to be found in waters, and they even recommend that special attention be paid to contaminations of this nature. Kubel's process, they admit, gives no especial clue to the presence or absence of nitrogenous matter, a problem which, to a certain degree, they regard as solved by the remaining process.

VI. Method of Wanklyn, Chapman, and Smith.

The authors describe briefly this method, omitting the addition of carbonate of soda for the first distillation, and remark that the process is simple and easy, and gives when the instructions of its inventors are closely adhered to, comparable results. Small portions of volatile organic matter may, they consider, escape prior to the addition of the alkaline permanganate. It is also not very easy to obtain this solution free from ammonia. The transformation of organic nitrogen into ammonia is most complete in the decomposition-products of the albuminoids. Just as was shown to be the case with the methods of Frankland and Kubel, this process shows the comparative quantities of nitrogenous matter in different waters only when similar mixtures of organic compounds are present.

In summing up, the authors consider it more probable that disease-ferments are present in a polluted than in a pure water, though it must not be inferred that every impure water must necessarily prove pernicious. When it becomes pernicious neither the physician nor the chemist can decide, though both may point out when it should be avoided as suspicious.—*Berichte der Deutschen Chemischen Gesellschaft.*

DETERMINATION OF CHROMIUM AND TUNGSTEN IN STEEL AND IN IRON ALLOYS.

By RUDOLF SCHÖFFEL.

THE author first removes the greater part of the iron by treating the comminuted material with the double chloride of copper and sodium, or of copper and ammonium. The residue, which contains all the chrome combined with small quantities of iron in a porous state, is rendered soluble by fusion with saltpetre and sodic carbonate. The melted mass, which may be coloured green by manganese, is digested with water till the rest appears pulverulent, whereby any manganic acid is decomposed, and the liquid is filtered. A solution is thus obtained containing the chrome as an alkaline chromate. If no important proportion of silica is present, as in some kinds of cast-steel, the liquid may be cautiously neutralised with nitric acid, precipitated with mercurous nitrate, and the chromium determined in the well-known manner; but as notable quantities of silica may be present in the solution, especially in crude chrome irons, it must first be removed by the usual process. The solution of the melted mass is neutralised with hydrochloric acid, a small quantity of alcohol being added, and evaporated to dryness. After filtering off the silica the chrome is precipitated with ammonia and ammonium sulphide in the usual manner.

This method is universally applicable for chrome-steel, but for crude chromium and chrome-iron alloys it is available to a certain limit only. If such a compound contains more than about 8 per cent of chrome, then on treatment with the double salt of copper a part of the iron is, indeed, dissolved, but the less the higher is the percentage of chrome. The residue does not assume that porosity which is required for the treatment with nitre and soda.

In such cases the only expedient is to digest the comminuted material for a long time in hydrochloric acid with the aid of heat. It is an erroneous supposition that on treating such iron with hydrochloric acid the chromium is found in the residue. If the proportion of chrome is low

it is entirely dissolved, even in dilute acid. In richer alloys a portion of the chrome remains undissolved. If the chrome exceeds about 30 per cent neither this metal nor the iron dissolves on prolonged digestion in hot acid. Such alloys are not attacked either by *aqua regia*, bromine, or cupric chloride.

The portion not dissolved in hydrochloric acid is fused in the usual manner with sodic carbonate and saltpetre, the melted mass is dissolved in water and hydrochloric acid, and the solution mixed with that already obtained by treating the iron with hydrochloric acid. The chrome is then determined in this solution by a process devised by E. Donath.

The solution is neutralised so far that it still remains distinctly acid, and mixed with sodic acetate, which should occasion no precipitate. Solution of permanganate is then added, and the whole heated to a boil, when the greater part of the iron is precipitated. The supernatant liquid after boiling should still appear distinctly red. A few drops of alcohol are then added till the colour disappears; sodic carbonate is then added to complete the precipitation of the iron, the liquid is again heated, and filtered. In the filtrate the chrome is found as alkaline chromate.

Or, instead of permanganate, the author adds to an acid solution in presence of sodic acetate bromine. If so much acetate is added that the liquid contains free acetic acid, but no hydrochloric acid, the oxidation takes place rapidly. The operation is best performed in a flask, which is closed after adding the bromine, and frequently shaken. After some hours the bromine is expelled by boiling, and the iron completely precipitated with sodic carbonate. This method is especially recommended where a simultaneous determination of manganese is required.

For the determination of tungsten in steels and tungstiferous iron the author employs the method given above for chrome. In the rare case of a notable proportion of silica the tungstic acid, after being weighed, is melted with potassic bisulphate, the melt treated with water, and the undissolved silica deducted from the gross weight of the tungstic acid. If the tungsten exceeds 12 per cent, which rarely occurs, this method is open to the same objections as in the case of chrome, except the material is very finely pulverised. The residue, after treatment with the double salt of copper, is washed, dried, and ignited in a crucible with exposure to air before fusion with nitre and carbonate of soda.—*Berichte der Deutschen Chemischen Gesellschaft.*

VOLUMETRIC DETERMINATION OF CERIUM.*

By Prof. FRANZ STOLBA.

EXACTLY like calcium oxalate, ceric oxalate can be volumetrically determined by means of potassic permanganate. Pure ceric sulphate, carefully dehydrated, was dissolved to the volume of a litre, and measured portions of the solution were precipitated with ammonium oxalate, the latter being determined in the known manner with standard permanganate, the solution of the latter being standardised by means of lead oxalate.

On the other hand, portions of the solution were taken for determination as cerium oxide. Using the atomic weight $Ce = 141.27$, as ascertained by Bührig, the quantity of cerous oxide was ascertained from the quantities of standard permanganate consumed.

The results were very concordant and satisfactory, and where differences appeared they fell within the inevitable errors of observation. Hence cerium, when freed from lanthanum and didymium, and separated as oxalate, may be conveniently and accurately determined as such. A corresponding quantity of sulphuric acid must be applied in titration and warm water used. During the process the

* Communicated by the Author.

quantity of undissolved matter decreases, and the change of colour at the conclusion is distinctly marked.

The author considers his results as confirmatory of Böhlig's atomic weight, $Ce = 141.27$.

PROCEEDINGS OF SOCIETIES.

NEWCASTLE CHEMICAL SOCIETY.

General Meeting, December 18, 1879.

Mr. JOHN GLOVER in the Chair.

THE minutes of the last meeting were read and confirmed.

Mr. J. Petrie was elected a member of the Society.

The CHAIRMAN—I believe the next business is the question of the amalgamation of the two Societies which have existed for some time in this district. As to the desirability of this amalgamation I think there cannot be two opinions. As the members are aware, there was a joint meeting of the two Committees held (at which I am sorry I was unable to be present) to consider the question, and as the result of that meeting I believe Mr. Swan has a resolution to submit to the Society this evening.

Mr. SWAN—I have to explain that the President is unavoidably absent; and, knowing that he could not be present, he wrote to me to ask me to submit to you a resolution on the subject of amalgamation. In accordance, then, with the unanimously expressed wish of the Committees of the two Societies, I beg to move the following resolution:—

“That this meeting cordially approves the project of amalgamation of the Newcastle Chemical Society with the Tyne Chemical Society, and requests the Presidents, Secretaries, and Committees of the said Societies to make the necessary arrangements for carrying this project into effect; and that in the meanwhile the President, Secretary, and Committee of the Tyne Chemical Society, not being previously members of the Newcastle Chemical Society, be invited to join in the deliberations of the Committee of the Newcastle Chemical Society, and that the members of the Tyne Chemical Society be invited to all the meetings of the Newcastle Chemical Society, and to read papers thereat.”

In framing this resolution I have had regard to two objects: first, to effect an immediate *practical* amalgamation—for it will be observed that the members of the Tyne Chemical Society will enter at once into the enjoyment of all the advantages which appertain to membership of this Society; and, second, to avoid any infringement of the existing rules of this Society. The formal union will be left to the deliberate arrangements made by the Presidents, Secretaries, and Committees, who will find out how the end can be attained without disregard of order and rule. I have no doubt that the members present will see the desirability of securing this object, and it is perhaps unnecessary for me to say anything further.

Mr. PROCTOR—I rise to second Mr. Swan's resolution. I don't think I have anything to add to what he has said. The resolution is worded so carefully as to avoid any appearance of irregularity or disregard of the rules in force in either Society. It is difficult, after having constituted our Society and framed rules, to take upon ourselves a step so unusual without in some degree infringing these rules, and if we were to work precisely by the rules we should have to postpone the legal amalgamation till the October meeting next year; *practical* amalgamation is, however, effected by the resolution, and I think that the way to get over any difficulties will be to dissolve our Society at the time when the amalgamation takes place. I don't know that there is anything to prevent us from dissolving and re-organising.

The resolution was then put to the meeting, and carried unanimously.

The CHAIRMAN—There is no doubt, I think, as to the feeling of the meeting on the subject: I hope at our next meeting we shall see the members of the Tyne Chemical Society, and perhaps have papers from some of them. We have now to listen to a paper by Mr. Swan “*On the Smoke of an Electric Lamp*.”

Mr. SWAN—When the time for discussion of Mr. Proctor's paper “*On the Smoke of an Electric Lamp*” came, I said I had then nothing to remark, but that I might afterwards have something to add to it, as I was experimenting on the subject. Therefore what I have is not a paper, but rather a few remarks in continuation of the discussion on Mr. Proctor's paper:—

At a recent meeting of the Society Mr. Barnard Proctor read a short paper “*On the Smoke of an Electric Lamp*.” It will, I believe, be remembered that the lamp referred to consisted of a globe of glass (about 3 inches in diameter) containing a rod of carbon: the rod was 0.04 inch in diameter and 1.2 inch long. It was held in a central position in the globe by sockets of platinum attached to platinum wire, which passed through opposite sides of the globe, and were hermetically sealed in it. The globe was carefully exhausted of air by a Sprengel pump. A current of electricity had been passed through the carbon rod, which raised it to a state of intense incandescence, and even bent it in such a manner as suggested that it had perhaps been softened by the intense heat. After the lapse of about a minute, during which a light was emitted sufficiently brilliant to illuminate a room about 40 feet square, the intense heat to which the carbon had been raised communicated itself to the platinum sockets and melted them, and so caused an interruption of the electric current and extinction of the light. When the globe came to be examined it was found to be coated with a deposit having the appearance of carbon; but as it was difficult to imagine how carbon could have got there, it was desirable that it should be proved whether it was really carbon or not, and my friend, Mr. Barnard Proctor, took in hand to examine into the nature of the deposit. The result of that examination has already been laid before you, as I have mentioned, and you will remember that the deposit proved to be for the most part carbon. That was one point settled. Another point which remained to be settled was the question how the carbon deposit got where it was found—whether contrary to common belief carbon was volatile, or whether its transport could be explained in any other manner? The object of my notice is to state that this last point has also been settled. Experiments which I, with the co-operation of my friend Mr. Stearn, have recently made, conclusively prove that the deposit which occurs under the conditions described is due to the mechanical transport of carbon particles, the residual air contained within the globe being the medium. We find the less air there is left in the globe the less deposit takes place, and we have succeeded in so nearly exhausting some globes that no deposit whatever takes place after a long-continued ignition of the carbon rod. I hope to illustrate this fact experimentally next meeting.

Two globes were exhibited by Mr. Swan, one, which had been more highly exhausted than the other, showing much less deposit.

Mr. PROCTOR—It was very interesting to observe in one of the globes, when the carbon was intensely ignited, that the ingress of a little air through some minute leak in the apparatus caused little streams of smoke to circle rapidly through the globe, and these increased as the quantity of air increased. Chemically, we should expect the more air less smoke; and as the deposits became less as the vacua were made more perfect, I think there can be no doubt that the deposit was mechanically transported carbon.

Mr. SWAN—The practical bearing of this is somewhat important, for if we can succeed in preventing absolutely the formation of this deposit, which now seems practi-

cable, we should have an electric lamp free from the irregularities of the present electric lamps.

Mr. STEVENSON—I feel very ignorant of the whole matter; but the question, as it appears to me, is—what is smoke? We are told that this deposit is not smoke, not volatilisation, yet it is transport of carbon: in what form is the carbon transported if not in the form of smoke?

Mr. SWAN—I don't think I used the term smoke in describing this appearance; but Mr. Proctor referred to smoke. I simply referred to the deposit, comparable to the deposit in a chimney, which is the result of smoke. The transport of particles is so slow that you don't see any smoke, but the glass gradually becomes coated with a deposit which consists of solid particles of carbon.

Mr. PROCTOR—I do think there is a distinction between this deposit and smoke. I should define smoke to be a deposit of carbon in the solid form from some body which contained it in the form of gas or vapour; but these carbon particles have never been in the gaseous condition, have been transported in the solid form, and are hence dust rather than smoke.

The CHAIRMAN—The discussion appears to resolve itself into the question, What is smoke? As Mr. Proctor says, smoke (ordinary coal-smoke at least) is a deposit of carbon from some gaseous hydrocarbon which has been imperfectly burnt. Now this process did not occur in the lamp, and hence the deposit cannot properly be termed smoke. It is a transmission through space of solid particles of carbon, and I think Mr. Swan said the less air the less deposit, and *vice versa*, which of course shows it could not be smoke in the ordinary acceptation of the term. The paper is one of a kind which gives value to the discussions of a society like ours, and forms one of the best arguments for its existence. It is an observation of an isolated phenomenon, out of which we never know what may come. Indeed Mr. Swan has already pointed out one practical result of the observation in the form of a steady electric lamp.

Prof. HERSCHEL—I think Mr. Swan has established the fact that the carbon was not volatilised, but mechanically transported. I should like to ask what kind of vacua is the deposit formed in? Is the exhaustion as high as in Mr. Crookes's apparatus for the exhibition of molecular streams, for instance? The transference of these carbon particles may be a manifestation of some kind of force similar to that which Mr. Crookes has discovered in the negative pole, and it may be dependent on the degree of exhaustion. Mr. Swan's explanation, however, appears the most rational I have heard.

Mr. SWAN—The original degree of exhaustion in the globe when sealed was about that of an ordinary Geissler tube. But there is occluded in the carbon, and possibly in the platinum, gaseous matter which is expelled at the high temperature of the lamp, and this reduces the vacuum to an unknown extent.

The CHAIRMAN—What was the gas which was occluded?

Mr. SWAN—That I cannot say.

Prof. HERSCHEL—Is the smoke produced in inert gas of ordinary atmospheric pressure?

Mr. SWAN—Yes, in a nitrogen atmosphere it is formed very readily.

The CHAIRMAN—We have next a paper by Mr. H. R. Proctor.

"Note on the Use of the Slide Rule for Chemical Calculations," by HENRY R. PROCTOR, F.C.S. I should perhaps apologise for bringing a matter before our Society which cannot in any sense be said to be new, and which no doubt is a perfectly familiar one to many of our members. But doubtless there are others who, like myself not long ago, have never actually used the instrument, however well they may know it by name; and as there are few devices which save more tiresome and routine labour, I may be excused for pressing it on their attention, and the more so because for our calculations its use is so simple that ten minutes'

study will enable anyone to employ it, and to read at sight results which would take at least many minutes to work out on paper.

The slide rule in its simplest form, which for our purpose is all that is needed, consists of two sliding scales running from 1 to 10 or 1 to 100, and divided decimally in logarithmic proportions. It is of course the characteristic property of logarithms that their addition and subtraction is equivalent to the multiplication and division of the corresponding common numbers, and hence these processes may be performed by simple addition and subtraction of lengths on the scale. From this follows the fact that any question of "rule of three" (and under this category come all ordinary chemical problems) may be solved at sight by its aid. Let 2 on one scale be over 3 on the other, and 4 will be over 6, and 6.66 over 10, and so on throughout.

Supposing we want to know what weight of sulphur corresponds to 0.595 grm. of baric sulphate, we must say—

"As $\frac{\text{BaSO}_4}{233}$ is to $\frac{\text{S}}{32}$ so is 0.595 to answer," and multi-

ply the second and third term together, and divide by the first."

To work it on the rule we set 32 over 233, and read the answer, 0.0817 grm., over 0.595, having saved the writing of at the very least thirty-two figures, and corresponding possibilities of error. Again, supposing 2 grms. of substance have been used in the analysis, all that is needed is to divide the atomic weight of S by 2, and set 16 over 233, and read the percentage of sulphur over any weight of BaSO_4 found.

Again, supposing we have to titrate anything with a standard acid not exactly normal,—say 978 c.c. equals 1000 c.c. of truly normal, all that is necessary is to set 978 on one scale above 1000 on the other, and under any quantity of acid used read the exact corresponding quantity of normal. It is obvious that we may in this way use any random standard solution almost as conveniently as a truly normal one, so long as its actual strength is known.

Another convenient plan, of almost universal application when a series of analyses with the same quantities and materials have to be made, is the following:—A single analysis is carefully calculated out in the usual way, either from actual results or from assumed ones chosen to facilitate the reckoning; and all others are reckoned, by simple proportion to this one, on the rule. This is accurate, however complicated the original calculation, provided all the conditions are really the same in each case, and is specially useful where random standard solutions must be used. To choose an instance familiar to myself, say 5 grms. of valonia are exhausted with a litre of water, 10 c.c. are used for titration with a permanganate (of which perhaps 49 c.c. correspond to 0.63 oxalic acid, or to 0.92355 of a suppositional tannin), and require 7.35 c.c. of the permanganate for oxidation. This naturally takes some calculating; but if I find that it corresponds to 30.4 per cent, I set 7.35 above 30.4, and in any succeeding analysis read the percentage direct below the c.c. of permanganate used.

It may be objected that a rule of moderate size will not read further than three figures, or per cents and one decimal, while it is customary to write two decimals of a per cent. It has, however, been shown in a previous paper ("On Errors of Weighing, and their Influence on Results," vol. ii., p. 187), that the errors, even of an excellent set of weights, completely vitiate the second place of decimals; so chemists who do not correct for these and the other unavoidable sources of error there pointed out, may guess at a second place with perfectly easy minds, assured that they are quite as likely to be right as if they had calculated it. Seriously, one place of decimals is for all practical purposes enough, except in cases of accurate determinations of scientific constants, where no trouble can be considered, and all possible corrections must be made. Even

in these cases the slide rule is invaluable for checking over the calculations, and often detects errors that might be passed over in simply going through the figures.

I do not know whether it is out of order to mention that the rule I use is made by Messrs. Tavernier and Gravet, of Paris, and supplied by Mr. Robson, of Dean Street; and for excellence and accuracy of graduation I have found it completely satisfactory. The price is from 8s. to 12s., the more expensive ones being provided with trigonometrical and other scales in addition to the simple logarithmic ones required for chemical purposes.

Two slide rules were exhibited by Mr. Robson at Mr. Procter's request, to illustrate the paper.

Mr. STEVENSON—I should strongly deprecate any chemist using this rule in place of ordinary arithmetical calculation, though I am quite ready to admit that it may be very useful as a check. I quite agree with Mr. Procter as to the uselessness of stating the second decimal; and if that were kept in mind we might save many unnecessary figures in calculations.

Mr. B. S. PROCTOR—Thirty years ago I used a scale such as Mr. Procter recommends, but eventually gave it up and took to arithmetical calculation. Multiplication is more accurate than the scale, if you don't make mistakes: while the advantage of the scale is that it never does mistakes.

The CHAIRMAN—I remember that scales similar to these, but very much longer, used to be common many years ago in all the laboratories in this district, but latterly I have not seen any. We always used them, however, simply as a check on our own calculations.

Votes of thanks were then given to the authors of papers.

At the close of the meeting Mr. Swan exhibited a very sensitive dry plate. The plate was exposed under a negative to the light from a small bat's-wing burner at a distance of about 10 feet, for three-quarters of a minute; and on development produced an excellent positive, which had no appearance of insufficient exposure.

CORRESPONDENCE.

SIZING OF COTTON GOODS.

To the Editor of the Chemical News.

SIR,—The writer of your review of Mr. Thompson's work on sizing makes an assertion which (having appeared before in the newspapers) has misled a great number of cotton merchants and manufacturers with regard to the cause of mildew when chloride of magnesia is used for sizing purposes. In the review in question (CHEMICAL NEWS, vol. xli., p. 9) the writer says—"Some substances most largely used in sizing, such as China clay, have no tendency to foster mildew. Others, such as the chloride of magnesium, have this undesirable property in virtue of the deliquescent nature, which keeps the tissue damp. The more chloride of magnesium is used, the greater is the need for chloride of zinc." The portion of the above quotation which I take exception to is the part which asserts that chloride of magnesium fosters mildew in virtue of its deliquescent nature. This is only partially true, because if the phrase has any meaning it is that substances which are deliquescent will foster mildew. This is negated by the very fact that chloride of zinc is as deliquescent as chloride of magnesium, yet the former prevents instead of fostering mildew. The fact is, that chloride of magnesium as a salt has no antiseptic property, or very little. When chloride of magnesium is used alone, it absorbs moisture, keeps the tissue damp, and having no antiseptic property it does not prevent the cloth mildewing. On the other hand, chloride of zinc also absorbs moisture and keeps the tissue damp; but the tissue does not mildew, because chloride of zinc as a salt has antiseptic properties. It is

not generally known among cloth merchants and manufacturers that chloride of zinc is a *deliquescent salt* because it is sold in solution. Chloride of magnesium being sold in the solid condition, the user can see the rapid absorption of moisture for himself. In my articles on sizing* I have classified magnesian chloride as a weight-giving substance, in virtue of its power of absorbing moisture and its want of antiseptic property. Chloride of zinc is a weight-producing substance also, but it has powerful antiseptic properties as well.—I am, &c.,

GEORGE WHEWELL,

Author of "History of Sizing."

Regent Chambers, Blackburn,
January 6, 1880.

THE PETROLEUM ACT, 1879.

To the Editor of the Chemical News.

SIR,—It will be noted that the schedule of the Petroleum Act, 1879, which you recently published in your columns contains no description of the "lead-line or pendulum," referred to in the Directions for Applying the Flashing-test. Prof. Abel has, however, informed me, in reply to a letter pointing out the oversight, that "a pattern pendulum or lead-line, as a guide for the lengths of pendulum to be used in connection with the petroleum test, has been deposited at the Standards' Office, Old Palace Yard, with a label upon which is specified that the pendulum is two feet in length from the point of suspension to the centre of gravity of the weight."

Where large numbers of samples have to be tested, I would recommend the use of a metronome, such as is used in teaching music, adjusted to beat synchronously with the pattern pendulum, the distinct sound emitted at each oscillation forming, in my experience, a better guide than the noiseless swing of the lead-line.—I am, &c.,

BOVERTON REDWOOD, F.C.S., F.I.C.,
Chemist of the Petroleum Association.

January 14th.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin,
No. 14, 1879.

On Polymerised Non-saturated Acids.—Rudolph Fittig.—The author's researches have been chiefly directed to isotropasic and cinnamic acids.

Action of Potassium Carbonate upon Isobutyl-aldehyd.—F. Urech. Continued from vol. xii., p. 193.—The author has examined the sparingly mobile liquids formed by this reaction at common temperatures.

Behaviour of Para-isobutyl-aldehyd with certain Reagents.—F. Urech.—The reagents in question are sulphuric acid, concentrated soda-lye, and chromic acid mixture.

Theoretical Considerations on the Polymers of Isobutyl-aldehyd.—F. Urech.—Incapable of useful abridgment.

Action of Iodine upon Oil of Turpentine.—H. E. Armstrong.—The mixture of hydrocarbons formed by the action of iodine and camphor—the so-called camphine of Claus—contains as its main constituent, in addition to cymol and higher homologues of the benzol series, a paraffin-like hydrocarbon of the formula $C_{10}H_{20}$.

* *Textile Manufacturer* vol. v., for April, May, June, July, August, and September, 1879.

Action of Sulphuric Acid upon Hydrocarbons of the Formula $C_{10}H_{16}$.—H. E. Armstrong and W. A. Tilden.—The authors have examined tereben anew, and have succeeded in erasing it from the list of known compounds.

Formation of Cymol and of the Hydrocarbon $C_{10}H_{20}$ by the Action of Sulphuric Acid upon Terpens.—H. E. Armstrong.—These two constituents are chiefly, if not exclusively, formed by the action of sulphuric acid upon oil of turpentine.

On the Absolute Expansion of Liquid and Solid Bodies.—H. F. Wiebe.—For the entire scale of aggregate conditions one common law of expansion holds good. At the boiling- and the melting-points all substances possess an equal cohesion. If we therefore multiply the absolute expansion of the temperatures of these points, increased by the inverse coefficient of expansion (0.00365), we obtain comparable numbers which are all multiples of the coefficient of expansion. Probably the number of the atoms which combine to a liquid or solid molecular group stand in simple proportions in members of the same chemical group.

On Para-nitro-phenyl-acetic Acid.—T. Maxwell.—This compound, $C_8H_7NO_4$, is readily soluble in alcohol, ether, and benzol; sparingly soluble in cold, but readily in hot water. It melts at 151.5° to 152° . Several of its salts are here described.

Quantitative Determination of the Oxygen Dissolved in Water.—Ferd. Tiemann and C. Preusse.—Certain portions of this important memoir are given in full. (See p. 30).

Behaviour of Sulpho-chlorides with Amines.—W. Michler and F. Salathé.—The authors describe the mutual reactions of α -naphthalin sulphochloride and dimethyl-anilin; of fuming nitric acid upon α -naphthyl-dimethyl-amido-phenyl-sulphon; and of β -naphthalin-sulphochloride upon dimethyl-anilin.

Behaviour of Sulpho-Chlorides with Amines.—W. Michler and K. Meyer.—A continuation of the last paper.

Action of Nitrous Acid upon Mono- and Diethylen-diphenyl-diamin.—H. F. Morley.—An account of ethylen-diphenyl-dinitrosamin, dinitroso-diethylen-diphenyl-diamin, and diethylen-diphenylen-tertramin. The salts of the last-mentioned body, in solution, give with ferric chloride a deep violet colouration.

Certain Derivatives of Dimethyl Meta-toluidin.—C. Wurster and C. Riedel.—The authors describe nitroso-dimethyl-meta-toluidin, nitroso-cresol, nitro-dimethyl-meta-toluidin, brom-dimethyl-meta-toluidin, dimethyl-toluylen-diamin, and tetra-methyl-toluylen-diamin.

On Dimethyl-para-phenylen-diamin.—C. Wurster and R. Sendtner.—An investigation of the action of bromine upon dimethyl-para-phenylen-diamin, and the behaviour of nitrous acid upon the oxaminic-ethyl ether of dimethyl-para-phenylen-diamin.

Action of Oxidising Agents upon Tetramethyl-para-phenylen-diamin.—C. Wurster and E. Schobig.—An examination of the blue colouring-matter obtained by the above reaction. The authors describe the behaviour of nitrous acid with tetramethyl-para-phenylen-diamin, trimethyl-para-phenylen-diamin, the nitrosamin of nitro-trimethyl-para-phenylen-diamin, and, lastly, trimethyl-triamido-benzol.

On Tetramethyl-meta-phenylen-diamin.—C. Wurster and H. F. Morley.—Tetramethylated meta-phenylen-diamin is much more permanent than the corresponding para-compound, and does not yield the same colour-phenomena.

On Brom-dimethyl-anilin.—C. Wurster and A. Scheibe.—An account of the action of nitrous acid upon the brom-dimethyl-anilin melting at 55° , of the nitrosamin of monobrom-monomethyl-anilin. On the reduction of nitrosamin and monobrom-monomethyl-anilin with tin and

hydrochloric acid there is formed a body which dissolves in alcohol with a red colour, and is probably a dimethylated rosein. Lastly, the authors describe meta-brom-dimethyl-anilin.

On Para-brom-dimethyl-anilin.—C. Wurster and A. Beran.—The authors obtain this compound by heating pure para-brom-anilin (melting-point 61°) in a closed tube in the water-bath, with somewhat more than 3 mols. iod-methyl and an excess of soda-lye.

Action of Nitric Acid upon Tribrom-benzol.—C. Wurster and A. Beran.—On treating tribrom-benzol with 10 parts of red fuming nitric acid (sp. gr. 1.534), the authors obtained mono-nitro-tribrom-benzol.

Ferro- and Ferri-cyanides of Certain Tertiary Bases, and of their Substitution-products.—C. Wurster and L. Roser.—Not susceptible of useful abstraction.

On Diastase.—M. Baswitz.—A study of the action of pressure, temperature, and concentration upon the action of diastase.

Solubility of Ozone in Water.—A. R. Leeds.—The author establishes the solubility of ozone in water, and shows that when in solution it displays its maximum oxidising power.

The Reduction of Carbonic Acid by Phosphorus at Common Temperatures.—A. R. Leeds.—This reduction is accompanied with the formation of carbonic oxide and of hydrogen phosphide.

Oxidation of Carbonic Oxide of Air over Moist Phosphorus at Common Temperatures.—A. R. Leeds.—This reaction is established by the author's experiments.

Vapour Density of Stannous Chloride.—T. Carnelley.—The boiling-point of this salt was determined by Carleton Williams, and the author is the real cause of the low numbers (7.47 and 6.88) which Rieth obtained.

The Spontaneous Oxidation of Nitro-lactic Acid.—L. Henry.—The products are hydrocyanic acid, oxalic acid, and water.

Addition of Free Oxygen to Non-saturated Compounds.—L. Henry.—The author describes a case of such addition.

Dry Distillation of Sodium Trichlor-acetate.—L. Henry.—The products obtained are carbonic oxide, carbonic acid and phosgen gas, trichlor-acetyl-chloride, and as secondary products trichlor-acetic acid and its anhydride, and as by-products the chlorides of carbon, C_2Cl_4 and C_2Cl_6 .

The Isomeric Toluol-mono-sulphonic Acids.—P. Claesson and K. Wallin.—The meta-acid is not, as Fahlberg states, a mixture of the ortho- and para-acids, but is perfectly well characterised.

On Guanidins of Ortho-toluidin and their Cyanogen Derivatives.—F. Berger.—An account of diorthotolyl guanidin, dicyan-diorthotolyl guanidin, diorthotolyl-oxalyl guanidin, diorthotolyl-parabanic acid, triorthotolyl guanidin, α -dicyan-triorthotolyl guanidin, triorthotolyl-oxalyl guanidin, and its behaviour with hydrochloric acid.

Ethylen Ethers of Pyrogallic Acid.—G. Magatti.—A notice of pyrogallic mono-ethylen ether, and certain of its derivatives.

Determination of Chromium and Tungsten in Steel and Alloys of Iron.—R. Schöffel.—(See p. 31.)

Determination of Cobalt and Nickel.—E. Donath.—(See page 15).

Substituted Chlorides of Nitrogen.—H. Kœhler.—Not adapted for abstraction.

Direct Production of Propylen-glycol from Glycerin.—A. Belchoubek.—This reaction is produced by the dry distillation of a gummy mass which remains after glycerin has been treated with sodium amalgam.

Molecular Re-arrangements of the Isobutyl Group.—B. Brauner.—A speculative paper.

Action of Silver Cyanate upon Isobutyl-iodide.—B. Brauner.—The product consisted of two-thirds trimethyl-carbinol-amin and one-third isobutyl-amin.

Octyl Derivatives.—E. Eichler.—The author describes mercury dioctyl, mercury iodoctyl, chlor-mercury-chloride octyl, mercury octyl-hydroxide, dioctyl, nitro-octan-octyl-nitrolic acid, octyl-amin, and octyl cyanide.

Rectification Residues of the Distillation of the Tar of Lignites, and on Certain New Derivatives of Chryson.—A. Adler.—The derivatives in question are mono-nitro-chrysen, tetra-nitro-chrysen, bibrom-chryso-quinon, dinitro-chryso-quinon, tribrom-binitro-chrysen, and chryso-quinon bisulphate of barium.

The Serpentine Masses of Reichenstein, and the Minerals therein occurring.—R. Hare.—An enumeration of the minerals and an account of their composition.

Analysis of the Upper Well of Fleinsberg, in the Iserkamm, Silesia.—Th. Poleck.—The character of this paper may be judged from its title.

Detection of Organic Matter in Water.—F. Tiemann and C. Preusse.—(See p. 31).

Gazzetta Chimica Italiana.

Anno 9, Fasc. 8 and 9, 1879.

Cymen of Cuminic Alcohol.—E. Paterno and P. Spica.—The author's experiments leave no doubt that the hydrocarbon obtained by the reduction of the chloride of cymile is identical with the cymen of camphor, and the cymen obtained by Kraut on heating cuminic alcohol with zinc.

Brief Notice on Cymen-carbonic Acid.—E. Paterno and P. Spica.—The acid in question is formed by heating carbocymenic amide with hydrochloric acid to 180° in a sealed tube. It is isomeric with the homocymenic acid of Rossi, but melts at a higher temperature.

Decomposition of the Hydrochlorate of Ethyl-amin by Heat.—M. Fileti and A. Piccini.—The decomposition commences at 270°, and becomes more active about the melting-point of lead. The residue consisted of ammonium chloride and of unaltered hydrochlorate of mono-ethyl-amin, mixed with the hydrochlorate of di-ethyl-amin. The gases evolved were the chloride of ethyl and ethylen.

Gasometric Analyses and Gasometric Methods.—D. Amato and P. Figuera.—An examination of the gases given off from a lake in the Valle del Bove.

Artificial Amelioration of the Leaves of Indigenous Tobacco by causing it to Imbibe the Juices of Exotic Leaves.—Antonio de Negri.—The nature of this paper appears sufficiently from the title.

Action of Potassic Bichromate upon Acetic Acid and Potassic Acetate.—Dr. Leobaldo Danesi.—The carbon of the acetic acid is totally or partially burnt by the oxygen of the chromic acid, as is the case with formic acid. If potassic acetate is boiled with potassic bichromate the reaction is similar.

Phenol Tolylate.—Dr. G. Mazzara.—The author obtains this compound by heating chloride of tolyl along with phenol in presence of zinc powder.

Oxy-azo-benzin and Para-methyl-oxy-azo-benzin.—Dr. G. Mazzara.—An account of the preparation and properties of these two bodies.

Meta-amido-cinnamic Acid.—Dr. G. Mazzara.—The author obtains this acid by the action of hydrochloric acid and metallic tin upon the meta-nitro-cinnamic acid of Schiff.

Insecticide Powder of the Flowers of Chrysanthemum Cinerariæfolium (Trew).—Prof. G. Dal Sie.—A preliminary notice. The author describes certain other insecticide powders obtained from the leaves of various species of *Pyrethrum*.

Synthesis of Phenyl-cumarin.—Dr. A. Ogliaro.—The author obtains this compound, along with acetyl-phenyl-ortho-cumaric acid.

The Sulphacids of Cymen and a New Cymo-phenol.—Dr. Pietro Spica.—A lengthy paper, not susceptible of useful abstraction.

Artificial Production of Oligiste from the Lava of Vesuvius.—The author steeped fragments of lava in a concentrated solution of common salt, placed them in a bottomless crucible, and heated them over a Bunsen burner. After a few days of constant heating the pieces of lava became covered with hæmatite, whilst laminæ of oligiste were sparingly scattered over their surface.

Production and Oxidation of the Alcoholic Derivatives of Natural and of Synthetic Tymol.—E. Paterno and F. Canzoneri.—The alcoholic derivatives of synthetic tymol are more easily oxidised than the corresponding derivatives of crystalline tymol.

A New Organic Acid, the Lithobilic found in the Oriental Bezoar along with the Lithofellic Acid.—Dr. G. Roster.—The author ascribes to this acid the formula $C_{30}H_{58}O_6$.

A New Method of Preparing Phenol-glycolic Acid, and on Pyro-gallo-tri-glycolic Acid.—Dr. P. Giacosa.—The former acid may be obtained by heating together in the water-bath equivalent quantities of phenol and of mono-chlor-acetic acid, and gradually adding for each part of phenol 4 parts of soda-lye (sp. gr. 1.3). After the reaction is over the sodic salt of the acid is deposited as a crystalline mass.

Resistance of Seeds, and especially those of Lucerne, to the Prolonged Action of Gaseous and Liquid Chemical Agents.—Italo Giglioli.—This extensive memoir does not admit of abstraction. The author's experiments relate principally to the seeds of lucerne.

Lapacic Acid.—E. Paterno.—This acid is represented by the formula $C_{15}H_{14}O_3$. It is not a glucoside, and on treatment with bromine it is converted into a substitution product, $C_{15}H_{13}BrO_3$, which forms splendid orange-coloured laminæ. By nitric acid it is almost entirely converted into phthalic acid.

Colouring-matter of Cucumis Anguria and of the Tomato.—Antonio and Giovanni de Negri.—The colouring-matter in question, rubidin, is extracted from the fruits by means of ether, which dissolves it along with a yellow colouring-matter. The ether is evaporated away and the residue treated with anhydrous alcohol, in which the yellow compound dissolves, whilst rubidin is left undissolved in the form of small crystals.

Chemiker Zeitung.
No. 52.

Determination of the Proportion of Water in Alcohol.—Aug. Vogel.—The author utilises a property of the alcoholic solution of the sulphocyanide of cobalt, which is changed from a blue to a red colour on the addition of water. By adding a few drops of the blue solution to a mixture of equal volumes of water and of alcohol, of sp. gr. 0.830 (=87.7 per cent), the liquid is at once turned red. This change of colour is also produced by a mixture of 60 vols. alcohol and 40 vols. water, but if 80 vols. of the alcohol are added to 20 of water the liquid remains blue. The author believes that this method may be developed so as to give approximate results.—*Bayerisch. Indust. und Gewerb. Bl.*, ii., 372.

Quantitative Determination of Fats and Resins in Cements and Lubricants.—The mixture is heated upon the water-bath with a sufficient quantity of soda-lye and alcohol until thoroughly saponified. The alcohol is expelled by heat, the residue taken up in water, and decomposed with hydrochloric acid. When heated on the water-bath the fatty acids, which have been liberated, and the

resins collect together, and after cooling may be separated from the liquid, washed with cold water, covered with several vols. of water, and heated to 50° or 60° with the gradual addition of sodic bicarbonate in small portions. A soap is thus formed from the fatty acids and dissolves, whilst the resin floats in the liquid or is deposited on the sides of the vessel, and may be separated by filtration. The fatty acids in the solution of soap are determined in the usual manner.—*Pharm. Central Halle*, xx., 446.

Reimann's Färber Zeitung,
No. 47, 1879.

Destruction of Vegetable Fibres Mixed with Wool.
—After discussing the proposed use of the chlorides of aluminium and magnesium, the writer concludes that such methods are applicable only to a superior class of goods and are useless where much vegetable matter is present. The only processes of practical value at present are treatment with sulphuric acid, and with hydrochloric acid gas, where the neighbourhood will permit.

No. 1, 1880.

"Acid Rubin."—Dr. E. Jacobsen converts the hydrochlorate of rosanilin into a sulphacid by acting upon the anhydrous salt with chlorinised sulphuric acid, SO_2Cl . This compound is an exceedingly valuable agent for the production of sulphacids. It acts less violently than the fuming sulphuric acid commonly employed, and yet forms sulpho compounds with ease. The colouring-matter thus produced can be used in woollen and silk dyeing in acid baths, and can be advantageously associated with extract of indigo, &c., for the production of various browns, modes, &c.

La Croix Rouge. December, 1879.

Trichinosis.—This disease appears to become more prevalent. Italy, Greece, and it is believed other countries, have been induced totally to prohibit the introduction of American hams, bacon, &c.

NOTES AND QUERIES.

Drying Blood.—Can any of your numerous readers inform us of the best way to dry blood.—W. H. M. G.

ERRATUM.—In the notice of Letts's Diaries for 1880 in our last issue we should have said that this is the 70th year of publication instead of the 30th.

MEETINGS FOR THE WEEK.

MONDAY, 19th.—London Institution, 5.
Medical, 8.30.
TUESDAY, 20th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
Civil Engineers, 8.
Zoological 8.30.
WEDNESDAY, 21st.—Society of Arts, 8. Henry Carr, "Domestic Poisons."
Meteorological, 7. (Anniversary.)
Geological, 8.
THURSDAY 22nd.—Royal Institution, 3. H. H. Statham, "Modern Architecture since the Renaissance."
Royal Society Club, 6.30.
Royal, 8.30.
Society of Arts, 8. Prof. Perry, "The Teaching of Technical Physics."
Zoological, 4.
London Institution, 7.
FRIDAY, 23rd.—Royal Institution, 8. Dr. Carpenter, "Sea and Land in Relation to Geological Time," 9.
Quekett, 8.
SATURDAY, 24th.—Royal Institution, 3. Prof. T. Rupert Jones, "Coal."
Physical, 3. Dr. O. J. Lodge, "On the Theory of Prof. Hughes's Induction Balance." C. N. Boys, "On a Liquid Voltaic Arc." J. Fater "On a Talking Machine."

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LOMAX, SONS, & MILLS, will SELL by PUBLIC ACUTION (by order of the Mortgagees), at the THATCHED HOUSE HOTEL, New Market Place, in the City of Manchester, on Tuesday, the 20th day of January, 1880, at Two for Three o'clock in the afternoon (unless previously disposed of by private contract), subject to such conditions as will be then and there produced,—

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THE CHEMICAL NEWS.

VOL. XLI. No. 1052.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Continued from page 28.)

THE constitution of uranium compounds again gives rise to numerous doubts, although the classic work of Pélitot has cleared up the most important points of the history of this element. The formula of the oxide, such as was announced by him, and as is generally accepted, is Ur_2O_3 , and its atomic weight is 120. In admitting these as true we cannot give uranium any determinate place in the system, either according to its atomic weight (for from $\text{Ag}=108$ to $\text{I}=127$ there is no empty place in the seventh series), or according to its properties; all the properties of uranium are even in opposition to the generally accepted hypothesis on the subject of its atomic weight. Although slightly resembling, in some of its properties, the metals of the iron group (particularly by its property of forming oxides, UrO , Ur_2O_3 , Ur_3O_4), uranium is distinct from them on several points; its density is 18.4; it gives a volatile chloride, UrCl_2 ; its oxide only gives salts corresponding to the composition UrOX , and not UrX_3 ; having a higher atomic weight than iron it is more difficultly reducible than this metal; the basic properties of the oxide are more feeble than those of oxide of iron. These considerations lead us to imagine that uranium has a different atomic weight to that which was mentioned above. In starting, as I explained fully in the case of indium, from the known equivalent (40 for Ur, which nearly corresponds with 38 for In), we find that Ur ought to be in the sixth group, and that, therefore, the oxide ought to have the formula UrO_3 , and the element should have for atomic weight 240. If so, its place is in the twelfth group, where thallium is. The greatest objection that we find here is that oxide of uranium is as rich in oxygen as we have only been accustomed to find the acid oxides.

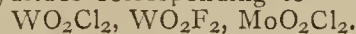
1. The atomanalogues of Ur (in the even series) are—Th=230, with a strongly basic oxide, ThO_2 ; and Cr, Mo, and W, which, like Ur, give oxides like RO_3 . In these oxides, as in those of the other groups (see Chapter I.), the basic character increases and the acid character diminishes as the atomic weight increases. Thus the acid properties are more sharply defined in CrO_3 than in MoO_3 or WO_3 . We can see already that MoO_3 and WO_3 have less acid properties, inasmuch that these oxides are capable of forming different salts with several acids, and with phosphoric, silicic, or sulphuric acid, they give bodies analogous to salts.*

We also notice more acid properties, less marked, it is true, in oxide of uranium. The following facts give us the proof of it:—

2. The precipitates formed by the caustic alkalis in solutions of salts of oxide of uranium are not hydrated oxides, but definite compounds of oxide of uranium with the alkalis. From this is derived the name *uranic acid*, by which name oxide of uranium is sometimes called. The composition of precipitates containing alkalis, $\text{R}_2\text{Ur}_2\text{O}_7$ ($\text{Ur}=240$), corresponds completely with the composition of the bichromates, and of corresponding salts of molybdic and tungstic acids. The alkaline salts of oxide of uranium do not separate from the alkali, either under the influence of water or of acids.

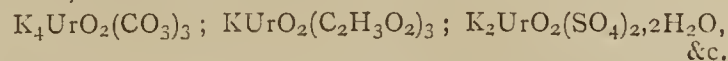
* Phosphoric acid has nearly the same relation with oxide of uranium as it has with molybdic and tungstic acids, which leads us to compare these three last oxygenated compounds with the first. For further information on this subject, see my "Principles of Chemistry" (in the Russian language), vol. ii., p. 281 to 285.

3. The acid character of oxide of uranium betrays itself already in the ordinary composition UrO_2X_2 of all the salts that this body gives. X means an acid residue, $\text{Ur}=240$. These salts, according to their composition, ought to be considered as basic, although they possess strictly acid properties; they recall at the same time the acid chloranhydrides corresponding to—

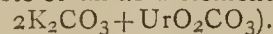


UrO_2Cl_2 is decomposed by water; and so is the double salt, which crystallises easily, $\text{R}_2\text{UrO}_2\text{Cl}_4 \cdot 2\text{H}_2\text{O}$. The double salt just mentioned will only crystallise in an excess of HCl; therefore it corresponds to TiK_2Cl_6 and to other analogous compounds of the acid chloranhydrides.

4. Oxide of uranium gives perfectly crystallisable double salts, such as—



The salt of oxide of uranium in these double salts apparently plays the rôle of an acid element (as in—



5. The acid character of uranium is corroborated by the volatility of chloride of uranium, UrCl_4 ($\text{Ur}=240$). Another circumstance which confirms this character is that this latter, like the acid chloranhydrides, is decomposed by water, so also are MoCl_4 , TiCl_4 , SnCl_4 , and ThCl_4 .

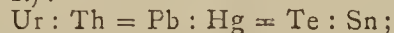
We can further offer the following considerations to point out the necessity and the advantages of a modification of the atomic weight of uranium.

The atomic volume of Cr=7.6, of Mo=11, of W=10,

$$\text{of Ur} = \frac{240}{18.4} = 13;$$

the atomic volume therefore increases absolutely the same as in the other groups, according as the atomic weight increases. It is thus that the atomic volume increases rapidly in the group K, Rb, Cs; less in Ti, Zr, Th; still less in Fe, Ru, Os, &c.; until at length the atomic volumes of Cl, Br, and I are nearly equal.

We can establish the following proportion for uranium (see Chapter I.):—



for, although these elements belong to different groups, that is to say, they form oxides of different formulæ, the chloric compounds of each couple have similar composition and properties. UrCl_4 is analogous to ThCl_4 , so is PbCl_2 to HgCl_2 , and so is TeCl_4 to SnCl_4 .

The salts of suboxide of uranium are not isomorphous with the salts of magnesia, as we might think they were if we adopted the generally-accepted atomic weight, and according to which uranium would be analogous with iron. This difference is seen distinctly by suboxide of uranium forming a double salt, $\text{UrK}_2(\text{SO}_4)_3 \cdot \text{H}_2\text{O}$, with sulphate of potassium. In this formula $\text{Ur}=240$. We see that for 2K there is 3SO_4 and not 2SO_4 , as in the salts of the magnesia group.

When chloride of uranium (UrCl_4) is calcined in hydrogen it gives a body containing more than its half of chlorine. This phenomena is explained if we admit the possibility of the formation of UrCl_3 or Ur_2Cl_3 , and it remains unexplained if we keep the accepted atomic weight (120) for Ur, because it is difficult to believe that Ur_2Cl_3 can be formed from UrCl_2 .

Regnault found that the specific heat of suboxide of uranium (which was thought to be the metal) is 0.062. If we admit the formula for the suboxide to be UrO ($\text{Ur}=120$), the molecular heat of this body is—

$$136 \times 0.062 = 8.4,$$

that is to say, it is less than that of ZnO (10.1) or of HgO (11.2), although the atomic weight of uranium is comprised, according to the above-mentioned hypothesis, between the atomic weights of Hg and Zn.*

If, on the contrary, we adopt the doubled atomic

* See my observations on the subject of specific heat in the "Principles of Chemistry," Second Part, Chapter III. Also my memoir published in the *Journal de la Société Chimique Russe*, 1870; and an abstract in the *Zeitschrift für Chemie*, 1870, p. 200.

weight, and if we give the formula UO_2 to the suboxide the molecular heat = 16.9; it is, then, as we might expect, greater than the molecular heat of MnO_2 (13.8), and of SnO_2 (14.0), and less than that of PbCl_2 (18.5). The determination made by Regnault is then more favourable to the formula RO_2 than to the formula RO (for the suboxide of uranium).

The proposed modification of the atomic weight of uranium makes us regard nature and her works from a new point of view, and it invites new researches on the degree of analogy with Cr, Mo*, and W.

The following experiments seem to me to be particularly interesting:—

1. To determine the specific heat of metallic uranium. I have long been wishing to make this determination, but H. Bauer and I, who worked together, have not been able to prepare cast-uranium; we always obtained it in the state of powder. Not being sufficiently convinced of the purity of this product I did not think it good enough for this determination.

2. Find the vapour-density of volatile chloride of uranium.

3. Observe exactly how it behaves when red-hot in the presence of hydrogen.

4. Examine the salts of suboxide of uranium, and see if they are not isomorphous with the corresponding salts of ThO_2 , SnO_2 , ZrO_2 , TeO_2 .

5. Compare the crystalline forms of the corresponding compounds of UO_3 , MoO_3 , and WO_3 . It would be particularly interesting to pursue the study of $\text{R}_2\text{UO}_2\text{F}_4$ (Carrington Bolton), and to make it the object of as complete a work as Marignac did on $\text{R}_2\text{WO}_2\text{F}_4$.

6. We can foresee the existence of a soluble oxide of uranium (metoxide) corresponding to the modifications of molybdc and tungstic acids.

7. It would be very interesting to submit the physical properties of corresponding compounds of Cr, Mo, W, and Ur to a comparative study, because we notice a great analogy in the composition, and even in the colour of corresponding compounds, above all of Cr and of Ur.†

The interest presented by a new study on uranium becomes greater if we modify the atomic weight in the manner indicated, because in this case the atom of uranium would become the heaviest of all known elements. I have arranged below the formulæ of some of the compounds of uranium according to the present atomic weight, and the values that they would have if the atomic weight were doubled:—

Ur=120.	Ur=240.
Compounds of protoxide— Ur_2Cl_3 ; Ur_4O .	$\text{UrCl}_3(\text{Ur}_2\text{Cl}_6?)$; Ur_2O_3 .
Compounds of suboxide— UrCl_2 ; UrO . $\text{UrSO}_4 \cdot 2\text{H}_2\text{O}$. $\text{K}_2\text{Ur}_2(\text{SO}_4)_3 \cdot \text{H}_2\text{O}$.	UrCl_4 ; UrO_2 . $\text{Ur}(\text{SO}_4)_{2,4} \cdot \text{H}_2\text{O}$. $\text{K}_2\text{Ur}(\text{SO}_4)_3 \cdot \text{H}_2\text{O}$.
Green oxide— $\text{Ur}_3\text{O}_4 = \text{UrOUr}_2\text{O}_3$.	$\text{Ur}_3\text{O}_8 = \text{UrO}_2 \cdot 2\text{UrO}_3$.
Analogous to— FeOFe_2O_3 .	Analogous to the blue oxide of molybdenum. $\text{MoO}_2 \cdot 2\text{MoO}_3$.
Compounds of the oxide— Ur_2O_3 ; UrOCl . $\text{UrO}(\text{NO}_3)_3 \cdot \text{H}_2\text{O}$. $2\text{UrONO}_3 \cdot 3\text{H}_2\text{O}$. $\text{Ur}_2\text{O}_2(\text{NH}_4)_4(\text{CO}_3)_3$. $\text{UrO}(\text{C}_2\text{H}_3\text{O}_2)_2 \cdot \text{H}_2\text{O}$. $\text{Ur}_2\text{O}_2(\text{C}_2\text{O}_4)_3 \cdot \text{H}_2\text{O}$.	UrO_3 ; UrO_2Cl_2 . $\text{UrO}_2(\text{NO}_3)_2 \cdot 6\text{H}_2\text{O}$. $\text{UrO}_2(\text{NO}_3)_2 \cdot 3\text{H}_2\text{O}$. $\text{UrO}_2(\text{NH}_4)_4(\text{CO}_3)_3$. $\text{UrO}_2(\text{C}_2\text{H}_3\text{O}_2)_2 \cdot 2\text{H}_2\text{O}$. $\text{UrO}_2(\text{C}_2\text{O}_4)_3 \cdot \text{H}_2\text{O}$.

(To be continued.)

* In pointing out the experiments, based on the periodic law, which I think would be interesting to undertake, I do not claim the right of priority for doing them.

† This fact and some other also (the analogy of Pt and Pd, of Nb and Ta, &c.) suggest the idea that independent of the large and small periods there are still quadruple periods, formed of two large periods. If so, the elements of the sixth group ought in reality to pass from the eighth series to between Cr and Ur. If so, we can explain in a more satisfactory manner, some differences between MoO_3 and CrO_3 , as well as the analogy of MoO_3 with WO_3 .

ON CHEMICAL REPULSION.*

By EDMUND J. MILLS, D.Sc., F.R.S.

WHILE engaged in some researches on the propagation of chemical change, I have incidentally encountered a new order of phenomena, which the title "chemical repulsion" may serve provisionally to designate. A brief outline of the experiments is given in the following paragraphs.

Upon a glass plate, laid in a horizontal position, is poured enough solution of baric chloride to cover it completely to a considerable depth. On this solution is placed another glass plate, provided with a small centre perforation; when the two plates are firmly pressed together with the hands, most of the solution is extruded, and only a very thin layer of it left between the plates. All excess of the solution having been removed from the outer surfaces of the plates as well as from the perforation, some dilute hydric sulphate is now introduced into the perforation. This reagent attacks the baric chloride, throwing down a white precipitate of sulphate; and, proceeding partly by diffusion, partly by flow, does not cease to widen in every direction its figure of advance, until the edges of the plates are attained. If the perforation is circular, the figure of advance is circular; in other words, the chemical development of a circle is a circle.

Let us now suppose the two plates to be square and equal, and let the upper one have two circular perforations, equidistant from the centre of the square, and situated upon its diagonal. Let also two circular developments of baric sulphate be caused to proceed, as before, from the two perforations simultaneously. At first, nothing remarkable is observed, but in a short time, the two growing circles begin to exercise a visible retardation on each other's progress; so that the figure of advance is no longer circular, but oval. [This retardation is of course observed only between the perforations; and not outside them, where the motion is entirely free.] As the development of the figures continues, so also does the retardation at their neighbouring edges increase; the final result being (however long the experiment may be prolonged), that the other diagonal of the square is completely and permanently traced out in a line of no chemical action.

The above experiments are of fundamental importance, and they obviously admit of endless variety. Of this, a few illustrations may suffice.

If the upper plate have three perforations, situated on the points of a central equilateral triangle, there are three repulsion lines; these end at the centre of the triangle, where they form a trilocular point, and traverse its sides midway at right angles.

When the upper plate has four perforations, situated on the points of a central square, there are four repulsion lines; these end at the centre of the square, where they form a quadrilocular point, and traverse its sides midway at right angles.

A very beautiful modification of the preceding experiment consists in simultaneously developing a circle from a (fifth) central perforation. This last circle has no means of escape from the surrounding four. The result is that it eventually forms a square figure bounded by repulsion lines, and having four symmetrically situated repulsion lines at its corners.

It is easy to demonstrate that the chemical repulsion in these experiments does not depend upon flow. Two superimposed triangular plates for instance, in neither of which is any perforation, give three repulsion lines on immersion in dilute hydric sulphate. From each corner a line proceeds midway (if the triangle be equilateral) to the centre. In this effect, diffusion is alone concerned.

In addition to hydric sulphate and baric chloride, other pairs of reagents may be used with success; and I anticipate no difficulty in obtaining results in which precipitation is not concerned. A beginning has also been made with experiments in tridimensional development.

* Abstract of a paper read before the Royal Society, Jan. 15, 1880.

The complete explanation of what I have termed "chemical repulsion," will probably demand a varied and considerable amount of experimental work. From some incidents of the investigation, so far as it has hitherto proceeded, I am disposed to believe that the motion in any plane chemical figure is not along the radius, but at right angles to the radius; and this supposition will, if verified, explain the repulsion. The existing results afford proof of the following propositions, viz.:—(1.) *Chemical action can take place at a distance*; and (2.) *Two or more chemical actions, identical except in position, completely exclude one another.*

ON THE HEATING POWER OF COAL-GAS OF DIFFERENT QUALITIES.*

By Dr. W. WALLACE,
Gas Examiner for the City of Glasgow.

THE extensive employment of coal-gas for heating purposes, and especially for cooking, gives an interest to the question whether the heating power varies like the illuminating power, and if so, to what extent, in the gas used in different towns? As regards illuminating power, we know that it varies exceedingly. In Aberdeen and Edinburgh it is 30 candles for 5 cubic feet per hour; in Glasgow, Paisley, and Greenock, about 26 or 27 candles; in Liverpool, Manchester, and Carlisle, about 20; in London and Dublin, 16; in Birmingham and many other English towns, 14; and in some as low as 12 candles. But as regards heating power we have no definite information, although there is a general belief that a rich gas has a greater calorific effect than one of poorer photogenic quality.

My attention having been directed to the subject by a correspondent, I caused a small apparatus to be made, in order, when opportunity offered, to test the matter practically. It is of very simple construction, consisting of a cylindrical tin-plate vessel, 8 inches in diameter and 6 inches high, with a cylindrical opening in the centre, $1\frac{1}{2}$ inches in diameter, through which the whole of the products of combustion pass. It contains exactly one gallon of water. Below is a brass tube bent into a circle 5 inches in diameter, and pierced with 34 small holes, from which the gas burns with flames about $\frac{3}{4}$ of an inch high. This tube is placed 1 inch from the bottom of the vessel, and is surrounded by an outer case having a sufficient number of small holes for the admission of air. On the top of the vessel there are two openings, in one of which a delicate thermometer is placed, while the other is provided with an open glass tube.

The opportunity of using the apparatus occurred recently, when I had occasion to test, on the same day, three samples of coal, which gave gas of 33, 26, and 15 candle-power respectively. Each experiment occupied, as nearly as possible, thirty minutes, and consisted in raising one gallon of water from 60° to 160° F., and measuring the quantity of gas consumed in the operation. The system is by no means a perfect one, but the results are strictly comparative. These are arranged in the following table:—

Illuminating Power of Gas in Standard Candles.	Durability of 1 C. Foot by flame 5 inches high.		Specific Gravity Air = 1000.	Comparative Value for Lighting.	Gas consumed in Heating Experiment. Cub. Ft.	Heat Units Fabr. for 1 C. Foot.	Comparative Value for Heating.
	Mins.	Secs.					
33·07	64	45	574	4/10	1·916	522	4/5
26·24	54	40	525	3/10	2·200	455	3/10
14·75	39	35	442	2/2	2·888	347	2/11½

* Read at the Meeting of the Philosophical Society of Glasgow, December 3, 1879.

The heat units represent the number of pounds of water heated 1° F. by the combustion of one cubic foot of gas. I have taken Glasgow gas of 26 candles at 3s. 10d. per 1000 cubic feet as the standard of value, and it will be seen at a glance that, while the heating value rises and falls with the lighting value, the amount of difference is by no means so great in the former as in the latter.

Having made these simple but instructive experiments, I naturally wished to compare the results with the theoretical heating values obtained by calculation from the composition of the gases. Unfortunately, I had no apparatus at the gas-works where the experiments were made for analysing the gases, and the best I have been able to do is to make the calculations from two analyses by the late Dr. Letheby, one of 12 candle gas, and the other of London Cannel gas, the illuminating power of which is not stated, but which may be assumed to be somewhere between 22 and 23 candles. The analyses are given only, as is usual with gaseous mixtures, by volume, but I have, for convenience, calculated the composition by weight.

	Common Gas. 12 Candles.		Cannel Gas. 22 Candles.	
	Vol.	Wt.	Vol.	Wt.
Olefiant gas, &c., C ₂ H ₄ ..	3·8	9·5	13·0	24·9
Marsh gas, CH ₄	39·5	56·5	50·0	54·8
Hydrogen	46·0	8·2	27·7	3·8
Carbonic oxide	7·5	18·7	6·8	13·0
Carbonic anhydride	0·6	2·4	0·1	0·3
Nitrogen	0·6	1·5	0·4	0·8
Aqueous vapour	2·0	3·2	2·0	2·4
	100·0	100·0	100·0	100·0
Specific gravity (calculated)		387		506
Weight of a cubic foot in pounds ..		0·0296		0·0387
Calorific power, C.		11,790		11,754
" " F., pounds of water heated 1° by the combustion of 1 pound of gas		21,222		21,157
Heat units for 1 cubic foot, C.		345		455
" " " F.		621		819
Pounds of boiling water evaporated by 1000 cubic feet of gas		639		842

We see from this tabular statement that, while the heating power of the two gases is almost identical, weight for weight, the practical result when we take the same measure of gas is very different, the Cannel being much heavier than the common gas. I calculate that in my practical comparative test I have realised about 55 per cent of the theoretical heating power, which is pretty nearly the proportion of effective heat obtained by the combustion of coal in a fairly well-constructed steam boiler.

The question naturally suggests itself, What is the comparative cost of heating by gas and coal? The calculation is a simple one. The theoretical heating power of ordinary soft coal may be taken at 13, that is, the number of pounds of boiling water evaporated by 1 lb., equal to 29,120 lbs. for a ton of coal, value say 11s. 6d., being the price of 3000 cubic feet of Cannel gas, the heating power of which is, say 2500. The gas, therefore, costs 11½ times as much as the equivalent quantity of coal, or, in round numbers, a penny-worth of coal gives as much heat as a shilling's-worth of gas. When, however, we consider the handiness, the cleanliness, and the convenience of gas, it is not surprising that it is extensively employed as a source of heat, as an illustration of which I may state that in my own laboratory from 250,000 to 300,000 cubic feet are consumed annually, almost the whole of which is burned for the production of heat.

The Royal Institution.—Prof. Dewar, M.A., F.R.S., will begin a Course of Eight Lectures on "Recent Chemical Progress," next Thursday afternoon, Jan. 29, at the Royal Institution.

DETERMINATION OF SILVER IN GALENA.

By C. BALLING.

ACCORDING to the supposed proportion of silver, from 3 to 5 grms. of the ore, finely powdered, are well mixed with three to four times their weight of a flux made up of equal parts of soda and saltpetre. The mixture is introduced into a porcelain crucible of suitable size, covered, and heated over the lamp till the contents are melted, when the mixture is thoroughly stirred up with a glass rod. It is then allowed to cool, and the crucible is placed in a capsule partly filled with water. The softened melt is then removed from the crucible into the capsule, heated over a lamp, and the aqueous solution is filtered. The residue on the filter, after being well washed, is rinsed back into the same capsule, mixed with dilute nitric acid, and evaporated to dryness: the dry residue is taken up in water acidulated with nitric acid, heated over a lamp, and filtered into a flask, washing well with hot water. The filtrate is allowed to cool in the flask, mixed with ferric sulphate or iron-alum, and titrated with a solution of ammonium sulphocyanide according to Volhard's process, 1 c.c. of the solution representing 1 m.grm. silver. The presence of small quantities of copper is not objectionable; lead is even advantageous, as the sulphate of lead which is deposited shows the approach of the final reaction. Large proportions of iron interfere with the accuracy of the process. The treatment of galena with nitric acid does not effect the complete solution of the silver present.—*Oest. Zeitschrift Berg. u. Hütten.*

DETECTION AND DETERMINATION OF
NITROUS ACID IN
DRINKING-WATER, ACIDS, &c.

By A. R. LEEDS.

CONCERNING Griess's method, as further developed by Preuss and Tiemann, in which the nitrous acid is colorimetrically determined by means of meta-di-amido-benzol, the author remarks that the colours obtained with this reagent in acidulated solutions of nitrites very closely resemble dilute solutions of caramel, and proposes (as he

formerly did for the colorimetric determination of ammonia by the Nessler test) the use of a hollow prism filled with a dilute solution of caramel, so as to give a scale of the colours produced by meta-di-amido-benzol in nitrite solutions of known strength, and to use this prism instead of comparative solutions.

On examining Trommsdorff's method of determining nitrous acid colorimetrically after admixture with zinc-iodide-starch solution and sulphuric acid, Prof. Leeds finds that in the dark, with free access of air and with equal proportions of acid the quantity of the iodine liberated increases with the proportion of dissolved iodide, whilst in the light the more iodine is liberated according to its intensity and its time of action. If air is absolutely excluded, light produces no decomposition, even at temperatures above the boiling-point. As the upper limit of the applicability of the reaction, the author gives 0.05 or 0.04 m.g. N_2O_3 in 100 c.c. In exceedingly dilute solutions the colorimetric determination is less accurate. According to Kübel and Tiemann, the water to be tested by Trommsdorff's method should contain not more than 0.04 and not less than 0.01 N_2O_3 in 100 c.c. Tinctorial organic bodies have an especially disturbing effect upon the reaction, as they not merely mask the changes of colour, but consume a part of the oxygen necessary for decomposing the iodides in the acid solution. Solutions of nitrites which are colourless and free from organic matter may be determined colorimetrically, but in coloured solutions and in presence of organic matter the air must be completely withdrawn and a volumetric method applied. The method employed by Prof. Leeds for the latter purpose is very simple, and consists of two comparative tubes fitted up with stoppers and tubes like washing-bottles. The exit-tube of the first, in which the reagents are placed, can slide up and down. The liquid to be examined is placed in the second vessel, through which carbonic acid, previously washed in potassium iodide, is passed for an hour, so as to expel all the air. The potassium iodide solution and sulphuric acid are driven from the first vessel into the second by sliding down the exit-tube, and a slight current of carbonic acid is passed for a considerable time through the liquid to prevent the action of the air. The iodine liberated is titrated with sodium hyposulphite. It is absolutely necessary that all air must be expelled from the liquid before adding the reagents.—*Zeitschrift Anal. Chemie und Chemiker Zeitung.*

COMPOSITION AND QUALITY OF THE METROPOLITAN WATER.

DECEMBER, 1879.

THE following are the returns of the Society of Medical Officers of Health:—

	Appearance in 2 foot Tube.	Ammonia.		Nitrogen as Ni- trates, &c.	Oxygen used to Oxidise Organic Matter.	Total Solids.	Lime.	Magnesia	Chlorine	Sulphuric An- hydride.	Hardness on Clark's Scale		
		Saline.	Organic.								Before Boiling.	After Boiling.	
													Grss.
<i>Thames Water Companies.</i>													
Grand Junction	Clear	0.000	0.008	0.195	0.060	22.50	7.160	0.329	1.224	1.330	15.4	4.2	
West Middlesex	Clear	0.000	0.008	0.210	0.056	22.90	7.840	0.396	1.152	1.200	15.4	4.2	
Southwark and Vauxhall	Clear	0.000	0.008	0.180	0.049	23.10	7.890	0.360	1.152	1.200	14.8	4.2	
Chelsea	Clear	0.000	0.008	0.180	0.052	21.10	7.890	0.432	1.152	1.200	14.3	4.2	
Lambeth	Clear	0.000	0.008	0.180	0.052	22.10	7.400	0.360	1.152	1.330	14.3	4.2	
<i>Other Companies.</i>													
Kent	Clear	0.000	0.004	0.450	0.012	31.10	10.810	0.576	1.944	2.700	21.2	7.0	
New River	Clear	0.000	0.007	0.200	0.023	21.90	7.890	0.360	1.152	1.200	13.7	3.7	
East London	Clear	0.000	0.008	0.195	0.073	23.50	7.840	0.468	1.296	1.330	14.8	4.6	

The quantities of the several constituents are stated in grains per imperial gallon.

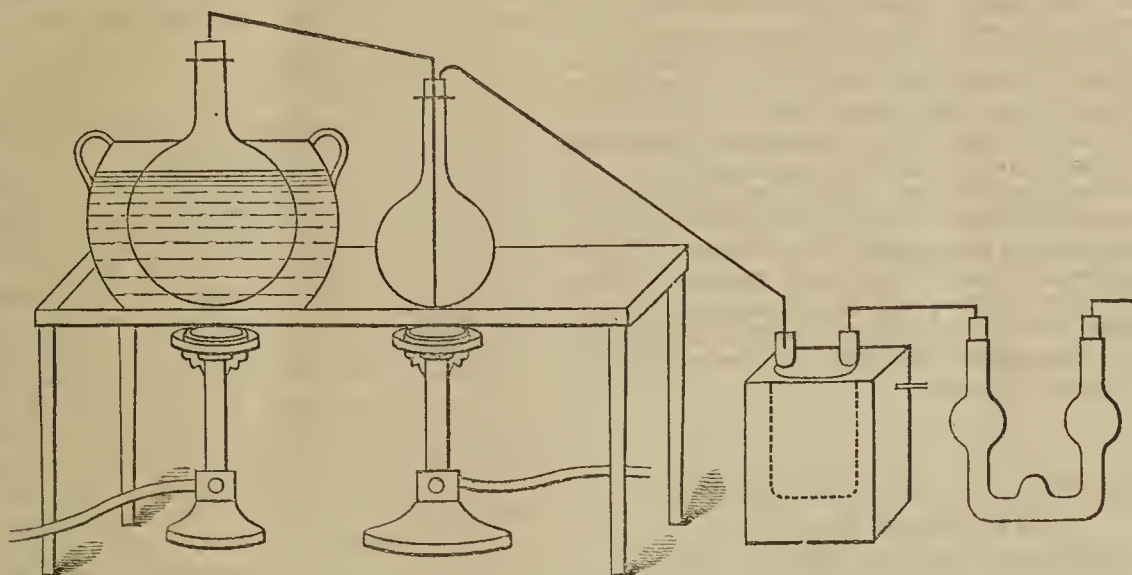
NOTE.—The amount of oxygen required to oxidise the organic matter, nitrites, &c., is determined by a standard solution of permanganate of potash acting for three hours.

C. MEYMOTT TIDY, M.B.

ON THE
ESTIMATION OF THE NITROUS COMPOUNDS
IN THE VARIOUS STAGES OF THE
MANUFACTURE OF SULPHURIC ACID.*

By JAMES MACTEAR, F.C.S., F.I.C.
(Continued from p. 17.)

The apparatus used in the operation is thus arranged :—
As the danger of breakage of the flasks is considerable when evaporated nearly to dryness, I use a bath of calcic chloride, which enables a higher heat to be employed, and prevents the breakage of the flasks. (In practice I use copper flasks for the daily testing).



To the large evolution flask which is immersed in the calcic chloride bath is connected a smaller flask, as a wash bottle, and to this again a receiver with a wash tube, as shown in sketch, and the method of conducting an experiment is as follows :—

About 30 grms. iron filings (which should be well boiled with caustic soda to remove grease, or simply roasted), and about 100 grms. granulated zinc, with about 50 grms. caustic soda (free from nitrates), and about 100 c.c. of water, are placed in the flask, with the solution containing the nitrous compounds, say, 200 c.c. of the solution previously mentioned in connection with "total acids" method of estimation, and after connection with the condensing

arrangement it is distilled to dryness in the bath of calcic chloride.

The absorbers contain, say, 50 c.c. $\frac{1}{2}$ normal sulphuric acid; and after the distillation is finished, they are washed out and titrated with $\frac{1}{2}$ normal soda; the solution is then boiled with a known excess of standard soda until the ammonia has all been driven off, and again titrated with standard acid.

The following are the results of the special set of experiments referred to in 3rd. (See below.)

From No. 9 downwards all these have been done in the calcic chloride bath, the temperature of which ranged from 240 degrees to 275 degrees F. during the experiments.

Abstracting the figures we have (see next page) :—

It will be at once seen that time is required in all cases for the decomposition, but the operation does not necessitate constant attention; on the contrary, once set agoing, it may be left to itself while other work is carried on, and thus the objections to it as a tedious process are got over.

It will be granted, I think, from these results, that Dr. Lunge was in error in condemning this method in the strong way he did, and concluding "that the process of estimating nitrates by reduction in an alkaline solution cannot be depended upon."

6. For the ordinary purpose of testing nitrous vitriol, there is no doubt that the process he recommends so highly is the most convenient; but it is rendered quite as

SUMMARY OF RESULTS.

Experiment.								
1	used	NaNO ₃	1	grm., alone	{ Stood for 36 hours on steam bath, then distilled,	100 used, gave 99.70
2	"	"	1	"	"	..	5 $\frac{1}{2}$ hours distilling	" " 99.66
3	"	"	$\frac{1}{2}$	"	2 grms. Na ₂ SO ₄	6	" "	" " 99.80
4	"	"	$\frac{1}{2}$	"	alone	..	5	" " 99.90
5	"	"	$\frac{1}{2}$	"	"	..	1	" " 98.90
6	"	"	$\frac{1}{2}$	"	2 grms. Na ₂ SO ₄	3 $\frac{1}{2}$	" "	" " 99.80
7	"	"	$\frac{1}{2}$	"	alone	..	5	" " 100.20
8	"	"	$\frac{1}{2}$	"	{ 5 grms. NaCl 5 grms. Na ₂ SO ₄	4	" "	" " 100.10
9	"	"	$\frac{1}{2}$	"	alone	..	1	" " 99.98
10	"	"	$\frac{1}{2}$	"	"	..	1	" " 99.87
11	"	"	$\frac{1}{2}$	"	{ 5 grms. NaCl 5 grms. Na ₂ SO ₄	$\frac{1}{2}$	" "	" " 98.50
12	"	"	$\frac{1}{2}$	"	do.	do.	$\frac{1}{4}$	" " 97.60
13	"	"	$\frac{1}{2}$	"	do.	do.	1	" " 99.70
14	"	"	1	"	do.	do.	1	" " 98.60
15	"	"	$\frac{1}{2}$	"	do.	do.	1	" " 99.40
16	"	"	$\frac{1}{2}$	"	do.	do.	1 $\frac{1}{2}$	" " 99.60
17	"	"	1	"	do.	do.	2	" " 99.10

* A Paper read before the Newcastle-upon-Tyne Chemical Society.

1.—With $\frac{1}{2}$ grm NaNO_3 alone in 1 hour—No. 9 .. 99.98 per cent.	No. 10 .. 99.87 ..
Mean 99.92 ..	
2.—With $\frac{1}{2}$ grm. $\text{NaNO}_3 + \left\{ \begin{array}{l} 5 \text{ grms. NaCl} \\ 5 \text{ grms. Na}_2\text{SO}_4 \end{array} \right\}$ in $\frac{1}{4}$ hour—No. 12 .. 97.60 ..	
3.—With $\frac{1}{2}$ grm. $\text{NaNO}_3 + \left\{ \begin{array}{l} 5 \text{ grms. NaCl} \\ 5 \text{ grms. Na}_2\text{SO}_4 \end{array} \right\}$ in $\frac{1}{2}$ hour—No. 11 .. 98.50 ..	
4.—With $\frac{1}{2}$ grm. $\text{NaNO}_3 + \left\{ \begin{array}{l} 5 \text{ grms. NaCl} \\ 5 \text{ grms. Na}_2\text{SO}_4 \end{array} \right\}$ in 1 hour—No. 13 .. 99.70 ..	No. 15 .. 99.40 ..
Mean 99.55	
5.—With 1 grm. $\text{NaNO}_3 + \left\{ \begin{array}{l} 5 \text{ grms. NaCl} \\ 5 \text{ grms. Na}_2\text{SO}_4 \end{array} \right\}$ in 1 hour—No. 14 .. 98.90 ..	
6.—With $\frac{1}{2}$ grm. $\text{NaNO}_3 + \left\{ \begin{array}{l} 5 \text{ grms. NaCl} \\ 5 \text{ grms. Na}_2\text{SO}_4 \end{array} \right\}$ in $1\frac{1}{2}$ hour—No. 16 .. 98.60 ..	
7.—With 1 grm. $\text{NaNO}_3 + \left\{ \begin{array}{l} 5 \text{ grms. NaCl} \\ 5 \text{ grms. Na}_2\text{SO}_4 \end{array} \right\}$ in 2 hours—No. 17 .. 99.10 ..	

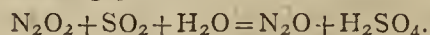
liable to error as the other by the presence of nitric acid in the nitrous vitriol from the Gay-Lussac columns. Dr. Lunge no doubt says, "I am inclined to believe that nitrous vitriol really does not contain any nitric acid except perhaps traces in any case;" and previously, "It is not, in the face of it, clear how nitric acid can get into nitrous vitriol, since it cannot very well bodily traverse the chambers without being reduced to lower nitrogen oxides; and since it is not regenerated either from the latter in the presence of sulphuric acid, the oxidation in this case never going beyond N_2O_4 , as proved by Winkler himself."

It is quite incomprehensible to me how Dr. Lunge seems to have forgotten the fact that N_2O_5 is formed from the decomposition of N_2O_4 in presence of water. His experience of sulphuric acid making must indeed be a happy one who has never seen the disastrous results of the excessive formation of N_2O_5 in this way in the last chambers of a series when excess of steam has produced weak acid and pale colour, which state of things is quite to be looked for if we are anxious to obtain the maximum production.

The decomposition is to some extent dependent on temperature, but may be stated thus:—

$2\text{N}_2\text{O}_4 + \text{aq} = \text{N}_2\text{O}_5 + \text{N}_2\text{O}_3$ at low temperatures.
and $3\text{N}_2\text{O}_4 + \text{aq} = 2\text{N}_2\text{O}_5 + \text{N}_2\text{O}_2$ at higher ..

And should we have SO_2 present we have the further decomposition taking place:—



part at least of the loss of nitrous compounds taking place in this form.

7. It being granted that Dr. Lunge found no N_2O_5 in his specimen of Gay-Lussac acid (which, by the way, is quite abnormal in its character, being of sp. gr. 1.691 = 138°T. , and containing in 100 c.c. equal to 4.13 grms. N_2O_3), nothing beyond this bare fact is proved, and no general conclusions can be drawn.

Even in the case of his artificial nitrous acid, where pure potassic nitrate was employed, the difference in the permanganate used in the blank experiment required 1 per cent of the permanganate in excess, an error which is quite absurd where a decided opinion as to the existence or non-existence of even 0.25 per cent N_2O_5 is to be given, and inclines me to believe the statement made by Gay-Lussac that nitrates treated out of contact of air with oil of vitriol evolve nitric oxide gas, while the liquid takes up N_2O_4 and N_2O_5 .

Winkler concludes that no N_2O_2 is absorbed by hydrated sulphuric acid, but Gmelin gives authority for the statement that it is absorbed in quantity sufficient at least to give a red colour to the vitriol containing ferrous sulphate, this colour being only, however, found occasionally in the nitrous vitriol from Gay-Lussac columns.

In presence of oxygen no doubt the N_2O_2 would oxidise to N_2O_4 , which again would possibly split up into N_2O_3 ,

and oxygen, which would escape through the Gay-Lussac column.

I think there is some probability of the view taken of the composition of N_2O_4 by Priestley, Berthollet, Sir H. Davy, and Thomson, in regarding N_2O_4 as really a combination of N_2O_5 and N_2O_2 proving, to some extent, correct; and this is, I think, supported by its peculiar irregularity in vapour-density at various temperatures.

I do not doubt but that as the solubility of nitric oxide (N_2O_2) in hydrated sulphuric acid seems to be comparatively small, a very considerable portion of the loss of nitrogen compounds takes place in this form in many cases, especially where the oxygen in the escaping gases is kept as low as possible.

Professor Hofmann, in the introduction to the third group in the German Catalogue of the Vienna Exhibition of 1873, says, referring to the investigations of Weber, P. W. Hoffmann, and C. A. Winkler, that the former view of the action, in the presence of an insufficient quantity of air, in which a part of the nitric oxide (N_2O_2) was reduced by the SO_2 to the state of nitrous oxide (N_2O), and consequently was lost, has, by these recent researches, been shown to be incorrect, that while in the presence of water in excess this decomposition does take place, for example, with acid of sp. gr. 1.510 (= 102°T.), in the case of the presence of a stronger acid, say of sp. gr. 1.690 (= 138°T.), the lowest compound formed with the oxygen is nitric oxide (N_2O_2).

Now, this being so, and the strength of the last chamber of a series being seldom above 100°T. , there can be little doubt that some of the loss of nitrogen compounds takes place in this way, as N_2O . A sufficiency of air in the chambers is the best safeguard against this loss.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, January 15, 1880.

Mr. WARREN DE LA RUE, President, in the Chair.

THE following certificates were read for the first time:—
A. Lloyd, P. Mathews, G. H. Morris, H. F. Pasley, A. G. Salamon, E. Wilcock, W. H. Wood.

The following papers were read:—

"On the Effects of the Growth of Plants on the Amount of Matter removed from the Soil by Rain," by J. H. PREVOST.
The soil used was taken from a plot of ground which had been richly manured with stable-manure and ashes in

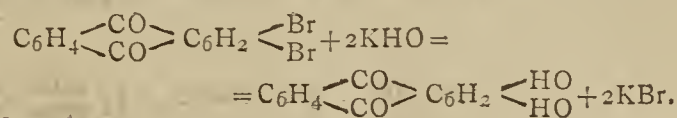
October, 1878. It was placed in two glazed earthenware pans, the depth of soil being 3 inches. The surface exposed was 17 inches in diameter. The drainage-water was collected, and muslin covers tied over the pan. Each pan contained 4300 grms. soil. In one was sown 4 grms. of white clover seed, the other was left blank. The seed was sown on July 21, and the two pans exposed till October 4. The clover plants had grown up but had not flowered. The drainage from the clover soil contained 48·125 grains per gallon; that from the blank pan amounted to 220 grains per gallon. These residues were analysed. The authors states that it is impossible from the meagre data in hand to draw any definite conclusions, except that the results do not very widely differ from those Dr. Voelcker obtained with the drainage from a wheat-crop; the principal points of interest being that rain removes much more matter from a soil containing no plants than from one which is cropped, and that the proportions in which the salts are removed do not greatly differ. Further researches are promised.

Mr. WARINGTON said that the method of investigation just described was at one time much used in Germany. It was what might have been anticipated, that the loss on a cropped soil would be much less than on an uncropped soil; this had been abundantly proved by the experiments at Rothamstead. Many striking results had been obtained during the past wet season. The best advice to give to farmers was that if they wished to prevent loss of the constituents of the soil they should always keep a crop on it. He had noticed that Dr. Prevost had found phosphoric acid in the drainage. This was exceedingly unusual, as in most cases all was retained. It might be accounted for by the small depth of soil. The soluble organic matter required much investigation.

Mr. WYNTER BLYTH then described a simple apparatus for the treatment of substances in open dishes by volatile solvents. It consisted of a circular cast-iron pan of suitable size, open at the top. The bottom was not flat, but pushed up in a dimple like the bottom of a wine-bottle. The evaporating dish was placed inside, and covered by a glass bell-jar, the edge of which rested in the circular trough formed by the projecting dimple. Mercury is poured in until the edge of the bell-jar is covered, and thus an air-tight mercury joint formed. To the upper end of the bell-jar is fitted a condenser, which may be vertical and return the condensed liquid to the dish, or horizontal if evaporation is desired. The author had used such an apparatus with great convenience for more than twelve months, the escape of vapour being thus completely prevented. The apparatus was exhibited.

The next paper was read by Mr. W. H. PERKIN, "On Dibrom-anthraquinons." Graebe and Liebermann give two processes for the preparation of dibrom-anthraquinon, one by brominating anthraquinon, the other by oxidising tetra-brom-anthracen. These processes yield, however, two different products which are isomeric; they are named by the author α - and β -dibrom-anthraquinon. The α body is prepared by heating bromine with anthraquinon. It crystallises from naphtha in flat needles; it is nearly insoluble in alcohol, and fuses at 245° C. The β isomer is prepared by boiling tetra-brom-anthracen with chromic acid, dissolved in a large excess of glacial acetic acid, for some time. On re-crystallising six or seven times from naphtha, the body was obtained in needles fusing at 275° C., quite different from the crystals of the isomeric α substance. The substance is soluble with difficulty in alcohol. The author then studied the action of caustic alkalis on these two substances, and in both cases alizarin (identified by its dyeing properties, its spectrum, and by analysis) was obtained. The decomposition of the α body furnished, besides alizarin, a colouring matter somewhat resembling alizarin. It dissolves in alkalies with a blue-violet colour, and its alkaline solution is precipitated by lead acetate. It has, however, no dyeing properties. Its absorption-spectrum gives two weak bands. It is not quinizarin nor anthrapurpurin; it may be dioxy-anthra-

quinon, with the hydroxyls in the same position as the bromine in the dibrom-anthraquinon. A small amount of another colouring seems to be present, dyeing mordants. The author is at present engaged in the study of these substances, the quantity prepared in the present research being too small for a complete investigation. The author then discusses the formation of the same dioxy-anthraquinon from the two isomeric dibrom-anthraquinons. In the case of the β compound, as no secondary matter seems formed, the reaction is—



In the case of the β isomer, probably bromo-monoxy-anthraquinon is first formed. The further action of potash produces brom-alizarin, with the liberation of hydrogen, which attacks the bromine and removes it as hydrobromic acid, and thus alizarin is formed. Probably, too, a certain amount of destruction takes place between the potash and the alizarin, some hydro-anthraquinon being formed. At the end of the paper the author states that he has recently examined Auerbach's original paper on isopurpurin, and concludes that that substance, from the properties given, is a mixture of flavopurpurin and anthrapurpurin, the former substance preponderating. Isopurpurin is not, therefore, identical with anthrapurpurin, as has been stated by the author of the present paper.

The SECRETARY then read a paper "On the Melting- and Boiling-Points of certain Inorganic Substances," by T. CARNELLY and W. CARLETON WILLIAMS. The authors have determined the melting-points of various substances by the specific heat method (*Chem. Soc. Jour.*, 1876, I., 489). The list includes tellurium, with its chlorides and bromides, cuprous bromide, &c. The boiling-points were determined by the method described in the *Chem. Soc. Jour.*, 1878, Trans. 281. The melting-points of silver and copper have been recently re-determined by Violle, and, in consequence, some corrections have been necessitated in the last paper of the authors (*Chem. Soc. Jour.*, 1879). The authors point out that in the mercuric compounds the melting-point sinks and the boiling-point rises with an increased molecular weight: the reverse is the case with cuprous compounds. Thus—

		Chlorides.	Bromides.	Iodides.
Mercuric	Melting-point ..	287	244	241
	Boiling-point ..	303	319	349
Cuprous	Melting-point ..	434	504	601
	Boiling-point ..	954	861	760

Mr. WARINGTON then contributed some Notes on some Practical Points connected with his Laboratory Experience. The first point considered was the modification proposed by Dr. Frankland some time since as a substitute for the steel blocks which form the air-tight connectors between the laboratory and the measuring-tubes in the gas apparatus. It will be remembered that Dr. Frankland causes the measuring-tube to end in an upright funnel in which fits roughly a conical stopper which terminates the laboratory tube. The joint was made tight by a piece of sheet india-rubber tied on the lower end of this conical stopper, the india-rubber being turned back, applied to the sides of the funnel, and the joint covered with mercury. This modification the author has found extremely convenient. One difficulty was the constant wearing out of the india-rubber: this was traced to its rubbing up and down as it was not absolutely fixed on the stopper. By tying the india-rubber above as well as below the stopper this has been obviated, and the rubber lasts many months instead of a week. The author strongly recommends the coating of laboratory benches, mercury troughs, &c., with paraffin. The wood is made quite hot by a Bunsen flame or in an oven, and the paraffin rubbed in. Thus the wood is completely protected from the action of acids. In the

determination of nitric acid by Frankland's process the author has observed that the action is often incomplete when the attack on the mercury is feeble: by adding a drop of dilute hydrochloric acid the reaction is always vigorous and the results good. Diphenylamin solution in strong sulphuric acid forms a magnificent blue colouration in the presence of a trace of nitrate. The author has thus detected 1-10,000th of a milligram. of nitrogen as nitric acid; the absence of other oxidising agents must be secured.

The Society then adjourned to February 5.

SOCIETY OF PUBLIC ANALYSTS.

The annual meeting of this Society was held on Wednesday evening, at Burlington House, the President, Dr. MUTER, in the Chair.

A ballot took place for the election of officers, when the following were elected:—Dr. Muter, President; Dr. Dupré, Dr. Tripe, and A. Wynter Blyth, Vice-Presidents; C. W. Heaton, Treasurer; C. Heisch and G. W. Wigner, Hon. Secretaries; and Messrs. Carter-Bell, Campbell Brown, C. A. Cameron, Bernard Dyer, Otto Hehner, W. Morgan, and W. Wallace, in place of the outgoing Members of Council.

Professor C. R. C. Tichborne was also elected a member of the Society, and Mr. Bruce Warren was proposed for election.

After the PRESIDENT had delivered his Annual Address, Mr. WIGNER read a paper by himself and Prof. CHURCH, "*On Two Ancient Samples of Butter*," of which the following is an abstract:—

The first was a sample of Irish bog butter, and its probable age was judged to be about one thousand years. The sample contains nearly 4 per cent of curd, which consisted partly of vegetable matter derived from the bog, but contained quite enough animal matter to prove that the butter had been originally made from animal milk, and was not a mere artificial fat. Its fatty character had, however, been entirely changed, and the glycerides of which the fat had originally consisted had been decomposed so as to leave simply a mixture of the fatty acids, which constituted the acid portion of animal fats. The butter had in fact become changed into a substance closely resembling in character and composition the substance of which good composite candles are composed. The result is singular, as showing that length of time combined with exposure to moisture will effect the decomposition which the manufacturer of stearine has to effect by the agency of heat and acids. The other and older sample of butter had been taken from an alabaster vase in an Egyptian tomb; it had evidently been melted and poured into the vase, and carefully sealed over. This sample was probably about 2500 years old, but the preservation had been so perfect that it was only slightly rancid, and had fully retained the chemical properties of genuine butter, the fats not having been decomposed to any sensible extent. This sample possessed a decided taste and smell of butter, while the sample from the bog was cheesy rather than buttery in smell.

The following papers were also read:—

"*On the Composition of Unfermented Wines of Commerce*," by Mr. CARTER BELL.

"*On the Estimation of Fat in Milk*," by Mr. W. M. HAMLET.

The members and several friends afterwards dined together at the Café Royal.

Presence of the Diamond in an Ophitic Rock from South Africa.—F. Fouqué and A. Michel Lévy.—A single specimen of this rock presents the diamond along with opal.—*Comptes Rendus*.

NOTICES OF BOOKS.

Year-Book of Pharmacy, from July 1st, 1878, to June 30, 1879. London: J. and A. Churchill.

THIS volume consists of two distinct portions. We have, first, a collection of abstracts of papers taken from British and Foreign Journals and from the *Transactions* of learned societies. This section is subdivided into the heads Pharmaceutical Chemistry, *Materia Medica*, Pharmacy, and Notes and Formulæ, and contains a valuable assortment of matter, much of which, however, the readers of the book will doubtless have seen elsewhere. We can scarcely give our approbation to the system of classification followed by the compilers. Many of the papers ranking under the head "Pharmaceutical Chemistry" seem to us to belong rather to chapters on analytical, general, and industrial chemistry. What, for instance, is the pharmaceutical bearing of papers on the colouring matters of santal and calliatura wood, and of *Lithospermum erythrorhizon*? Analytical processes—and especially methods for the detection and quantitative determination of impurities or sophistications in articles of food, drugs, and chemicals—appear to figure somewhat indiscriminately under the heads "Pharmaceutical Chemistry" and "Pharmacy." We submit that a more careful arrangement of matter would increase the value of the book by rendering it easier for the reader to find what he is in search of.

The second portion of the book—the account of the British Pharmaceutical Conference, which met at Sheffield simultaneously with the British Association—contains matter of permanent interest, which it might not be very easy to find elsewhere. The opening speech of the President, Mr. Schacht, touching principally on the position of the pharmacist in society, and of the relations between pharmacy and what he rightly calls the other branches of the medical art, are most important. We feel deeply obliged to him for using, consistently with the practice of all other European nations, the definite and appropriate term "pharmacist" instead of the vague and misleading word "chemist." We call the latter vague and misleading because, logically speaking, it belongs to all men engaged in the study of chemistry, or in the practice of any of its applications. His remark that in modern medical schools chemistry is an "off-subject," and is very generally done "any-how or no-how" is but too true, as we have long known and repeatedly lamented.

We cannot help regretting that neither the President nor any of the members expressed an opinion on a modern practice injurious to the pharmaceutical profession, and liable to result in serious danger to the public. We refer to the sale of so-called patent medicines—many of which contain poisonous ingredients—by drapers, grocers, and other totally unqualified persons. In fact the whole question of patent medicines calls for the re-consideration of the Legislature.

Dr. Hamberg's paper on the capacity of different organs to absorb and retain arsenic in cases of chronic poisoning excited a small display of anti-vivisectionist feeling, but must be pronounced exceedingly valuable. We fail to see how the results could possibly have been attained by any other method than that pursued by the author.

We are somewhat surprised to learn that Panama bark (*Quillaria saponaria*) figures in the *Materia Medica*. Having once thoughtlessly chewed a small fragment whilst engaged with some experiments on its uses in dyeing, we were greatly annoyed by its persistent irritating action upon the throat.

The papers by Mr. A. H. Allen on "Petroleum Spirit,"—so-called "benzoline," and by Mr. E. Davies, on the estimation of water in iodine, are of general interest to professional chemists.

CORRESPONDENCE.

BLUE FLAME IN COAL FIRES.

To the Editor of the Chemical News.

SIR,—I think Col. Ross has mistaken the question that was at issue, in his letter with the above heading (CHEMICAL NEWS, vol. xli., p. 22). I am not aware that the blue flame usually observed in coal fires, and which is due to carbon monoxide, has ever been attributed to CuCl_2 or sulphur. It was the blue flame produced by common salt that formed the subject of debate (as early as 1876); and in a communication to the CHEMICAL NEWS, vol. xxxix., p. 141, and also in *Nature*, March 27, 1879, I gave some reasons for attributing it to HCl, which supposition I afterwards confirmed (*Nature*, May 1, 1879).

Col. Ross's theory is ingenious, but unfortunately for it the question arises—Where is the coal-gas to come from when all the bituminous matter has been burned away and nothing but carbon left?

That the burning CO is not seen till the luminous flames have disappeared seems chiefly due to the small quantity of light which it emits. It is a popular saying that the fire "burns blue" in frosty weather, and I have frequently noticed that the saying is more or less true. I endeavour to explain it by the fact that larger fires than usual are made in cold weather, and that a large body of red-hot cinders is most favourable for the production of CO.—I am, &c.,

A. PERCY SMITH.

Temple Observatory, Rugby,
January 10, 1880.

THE TESTING OF VITRIOL-CHAMBER EXITS.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS (vol. xli., p. 16) you publish a paper by Mr. J. Macfear on the testing of vitriol-chamber exits. Now, in the beginning of May last year, I found myself called upon to test a vitriol-chamber exit without having any standard solutions at hand, and I then hit upon a method which I have used ever since, and as I have found it both very convenient and accurate, a description of it will perhaps be of interest in connection with Mr. Macfear's paper.

I had previously been accustomed to use the method which Mr. Macfear aptly calls that of "total acids," which, however, is open to the objections that the estimation of the S compounds is made by the always troublesome processes of precipitation, filtration, and weighing; that any error in estimating the SO_3 gives rise to a greatly more serious one in the reckoning of the N compounds; and that as it takes no account of HCl the accuracy of the N compounds determination is still further impaired. My method gets rid of these and other objections, and is as follows:—

I absorb in a solution of carbonate of soda, with addition of peroxide of hydrogen, as recommended by Mr. G. E. Davis (CHEMICAL NEWS, vol. xxxix., p. 221), after taking the precautions there mentioned by him for ascertaining the strength and purity of the H_2O_2 . When finished, the absorbing tubes are emptied into a basin, and the solution evaporated to dryness to ensure complete expulsion of free O. I then add standard H_2SO_4 to neutralisation without previous addition of water, in order to keep the bulk of liquid as small as possible, using litmus-paper instead of solution to avoid introducing organic matter. The point of neutrality should not be passed for fear of loss of vapour of HNO_3 on boiling to expel CO_2 .

The solution, which should not amount to more than 3 to 5 c.c., is then placed in the mercury tube, and the HNO_3 estimated according to Crum's method. For this latter purpose the same apparatus is used as is employed now pretty generally for the estimation of N_2O_3 in nitrous

vitriol (*vide* Mr. G. E. Davis, CHEMICAL NEWS, vol. xxxvii., p. 45, and Dr. Lunge, vol. xxxviii., p. 19).

It is best to place the solution in the tube without cooling completely. The addition of the O.V. raises the temperature still further, so that the reaction generally commences at once—otherwise it may hang fire for a considerable time, in which case warming the tube over a flame will start the reaction, which then finishes in a minute or two, and the NO is read off on cooling.

By following the foregoing course we get the N compounds estimated directly; the S compounds by difference; any HCl reckoned with the S; and the whole done very expeditiously and neatly. Of course, by dividing the solution we can estimate the three each directly, the SO_3 with barium and the HCl with silver.

I usually take for absorbing 50 c.c. of Na_2CO_3 solution containing 5 grs. Na_2CO_3 , add 10 c.c. H_2O_2 , and aspirate 3 to 4 cubic feet per twenty-four hours.

For calculating 1 c.c. NO = 0.0365 gr. Na_2CO_3 ;
= 0.0262 gr. N_2O_3 ;
= 0.0585 gr. NaNO_3 .

Mr. Warrington, in a paper read before the Chemical Society, May 1, 1879, has shown that to vitiate Crum's test HCl must be in very great excess, but that organic matter may be objectionable.—I am, &c.,

T. E. VASEY.

Church Street Chemical Works,
Hunslet, Leeds.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 26, December 29, 1879.

On Copper Hydride: a Reply to M. Wurtz.—M. Berthelot.—The author points out that M. Wurtz, though admitting that the so-called hydride contains the elements of copper phosphate, hesitates to accept the necessary conclusion. To obtain the hydride of copper in a state of purity, and to show that the oxygen and the phosphorus are not inherent in the constitution of the compound, are tasks incumbent upon M. Wurtz and not upon the author.

Heat of Formation of Gaseous Chloral Hydrate: a Reply to M. Wurtz.—M. Berthelot.—The author announces that the temperature of the double glass case in which M. Wurtz mixes his gaseous masses is not constantly the same, before each experiment, as that of the bath surrounding this double case. The temperature of the water-bath employed, without proper enclosures, being brought, as he has done, to a point other than that of the ebullition of water, is not constant. The temperature realised by the mixture of the two gaseous masses is neither constant nor equal to the mean temperature. The variations are such as to exceed the slight rise of temperature which might result under these conditions from the combination of the vapour of water with that of chloral. The errors are still greater on operating as M. Wurtz has done under pressures less than that of the atmosphere, since the masses brought into action are thus diminished at the same time as the operations relating to the exact mixture, and the escape of the gases are rendered more complicated. These masses, if reduced to water, cannot surpass a few grammes in the experiments of M. Wurtz. Even the very mixture of the gases is necessarily imperfect in an apparatus arranged like that of M. Wurtz, a circumstance which must reduce the elevation of temperature observable.

Oxidation of Alcohol by the Ammoniacal Bin oxide of Copper.—A. Letellier.—The author heated to 180° in

a sealed tube a mixture of alcohol and of binoxide of copper dissolved in ammonia. The liquid became colourless and the alcohol was converted into acetic acid. All the alcohols, glycerin, benzol, and oil of turpentine, if heated with ammoniacal cupric oxide, decolourise the liquid. The products formed have not yet been examined.

A New Electric Burner.—M. Perruche.—The burner itself, or candle, is composed of three cokes, of which two are cylinders, whilst the third has a square section. Two of them are in close contact during the combustions and form only a single electrode. The third is placed upon a line bisecting the angle formed by the other two, and forms the other electrode. The regulating apparatus is described at some length.

A New Phoneidoscopic Process by Means of Coloured Rings.—A. Guebhard.—In a recent communication (Dec. 8, 1879) the author described the production of coloured rings by the condensation of watery vapour on the freshly cleansed surface of impure mercury. He finds that these bands of colour may be applied to the study of the internal movements of masses, and of the vibration of masses, being much more sensitive than pulverulent bodies. Vocal currents, in particular, impress upon the mercury characteristic diagrams, some of which are figured.

Action of Potassium Permanganate upon Potassium Cyanide.—E. Baudrimont.—If standard solutions of these bodies are allowed to react upon each other, the author finds: That the decolouration of the permanganate has a necessary limit; this decolouration is facilitated by an elevation of temperature and the concentration of liquids; its limit is attained when 2 equivs. KCy are in contact with 5 equivs. of permanganate. The action is very rapid when the liquids are acidified with sulphuric acid. The products formed by the mutual reaction of the salts differ in their proportions if not in their nature. Those whose presence has been ascertained are urea, the carbonic, nitrous, nitric, oxalic, and formic acids, and ammonia, due to the decomposition of the urea. The quantity of urea increases the more the mixture is acidified with sulphuric acid.

Action of the Hydracids upon Isopren; Reproduction of Caoutchouc.—G. Bouchardat.—Isopren in contact with the hydracids behaves like valerylen, and, like the latter, fixes one or two molecules of acid, forming identical or isomeric compounds, but closely allied in their properties. Isopren, however, yields with the acids an elastic polymer, possessing most of the attributes of caoutchouc. It is insoluble in alcohol, swells up in ether, and in the sulphide of carbon, in which it dissolves in the same manner as natural caoutchouc. If submitted to dry distillation it yields the same volatile carbides as caoutchouc.

— — —
Journal des Savants. November, 1879.

Studies on the Physical Functions of Leaves.—E. Chevreul.—Among the conclusions which the author regards as established are:—That the transpiration by both surfaces of the leaf is greater in the sunshine than in the shade. With the exception of the oleander and the maize plant, which exhibit in this respect an anomaly, it appears that in the numerous species experimented upon if the transpiration from the upper side of the leaf is represented by unity, that of the reverse equals 4·3. In the shade, however, the proportions are as 1 : 2·4. The leaves of the chestnut, of maize, and of the periwinkle, transpire equally on both surfaces. It has been already demonstrated that plants absorb solutions of the salts necessary for their growth, not merely by the leaves, but even by the petals of their flowers.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin,
No. 15, 1879.

Partial Synthesis of Milk-sugar, and Contribution to the Synthesis of Cane-sugar.—E. Demole.—Setting out with the idea that the glucose molecule of cane- and of milk-sugar are different, the author endeavoured to take a further step in the synthesis of these sugars by combining, on the one hand, levulose with dextro-glucose, and, on the other, galactose with lacto-glucose. In the second case he has succeeded. Milk-sugar was first resolved into two isomeric bodies, galactose and lacto-glucose, by the action of dilute acids. The product of this reaction, after removal of the acid, was evaporated and carefully dried, and was found to possess all the properties of a mixture of equal mols. galactose and lacto-glucose. It was treated with three parts of acetic anhydride in an apparatus provided with a cohobator, and heated to the boiling-point until totally dissolved. On subsequent treatment a body was obtained possessing all the properties of octacetyl-lactose.

A Preliminary Communication.—J. Moritz.—The author has found that sulphur dusted over vines as a remedy for *Oidium Tuckeri*, is converted into sulphurous acid.

On Carbohydrates.—A. P. N. Franchimont.—The author has investigated animal cellulose (tunicin), glucose, and vegetable cellulose. The first-mentioned substance was converted into glucose exactly as is vegetable cellulose. It is still, however, possible that the two bodies are not identical, and that their difference, if it exists, may depend upon polymerism.

Meta-isatinic Acid (Meta-amido-phenyl-glyoxylic Acid).—L. Claisen and C. M. Thompson.—The author describes the formation of this acid and the properties of certain of its derivatives.

On H. O. Pawel's Communication concerning Roussin's Salt.—W. Demel.—The author points out that H. Pawel has criticised him without having read his memoir in full.

Reply to the Remarks of H. H. Salkowski.—W. Demel.—An apology for an oversight in a question of priority relating to the arseniates of zinc and cadmium.

On Roussin's Salt.—O. Pawel.—The author maintains that the compound described as nitric oxide-iron-sulphide is simply a mixture of several salts with sulphur as an impurity, and always containing an alkali metal. He shows at some length the true nature of Roussin's salts.

Decomposition of Mesoxalic Acid by Hydrogen Sulphide.—C. Böttiger.—The mesoxalic acid is first resolved into carbonic and glyoxylic acids, the latter of which is then transformed into thioglycolic acid. The oxalic acid found among the products is probably due to a secondary reaction.

Mendelejeff's Periodic Law and the Magnetic Properties of the Elements.—T. Carnelly.—The author shows that those elements which belong to the even series in Mendelejeff's classification are always paramagnetic, whilst those of the odd series are as constantly diamagnetic. He concludes that the investigation of the magnetic properties of newly-discovered or imperfectly-known elements is of importance for determining their place in a general classification. The uneven elements of a group seem to become more and more diamagnetic as their atomic weights increase. In the eighth group the paramagnetism seems to decrease with the increase of the atomic weight.

Di- and Tri-Derivatives of Naphthalin.—Raphael Meldola.—An account of α - β -di-brom naphthyl-amin, α - β -di-brom-naphthalin, and of the constitution of α -sulpho-brom-naphthalic acid.

A Correction.—C. Counciler.—This note relates to the constitution of fluoboric ethylen.

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THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF

(Continued from page 40.)

THE three elements which are found in cerite, viz., cerium, lanthanum, and didymium, have very nearly the same equivalents (about 45)* and similar properties; it is therefore difficult to determine the atomic weights of these elements. Involuntarily we think that they are analogous to those of the iron group, particularly because cerium has some of the properties of manganese. However, we can only admit the analogy if we have simply a superficial knowledge of the metals from cerite. If we admit the formula RO for their ordinary oxides, their atomic weights will be approximatively 92, which will not agree with the eighth group. Even if we give their oxides the formula R_2O_3 , these elements will not enter the eighth group, because their atomic weights would then be about 138; and the reason of this exclusion is not only that the elements of group VIII., series 8, should have an atomic weight averaging between Pd and Pt (about—

$$150 = \frac{105 + 195}{2},$$

but it is above all because the known properties of these elements do not correspond with the properties of the missing members of the eighth group. The metals of cerite are more difficultly reducible—they only give a small number of oxides; the ordinary oxides possess very strongly basic properties; as far as we know they do not give such characteristic ammoniac and cyanic compounds; in a word, they are not the atomanalogues of the metals of the eighth group (see Chap. I.).

Let us give still one more proof:—

According to Wöehler, the density of cerium is 5.5. If we accept CeO as the formula of the ordinary suboxide, $Ce=92$, the atomic volume = 17; but if we admit that the suboxide is represented by Ce_2O_3 , then $Ce=138$, and the atomic volume = 25. These two volumes do not correspond with the members of the eighth group, which have much smaller atomic volumes. If we take RO as the formula of the ordinary oxides of the metals of cerite, we cannot place them either in the second or third group. It only remains for us to try if other forms of oxides are applicable, as we did before when determining the atomic weight of indium. For this purpose we will firstly consider cerium only, then afterwards we will examine lanthanum and didymium, superficially and simultaneously, because cerium has been better studied than the other two metals, and it gives at least two basic forms of oxidation. This circumstance facilitates the determination of the atomic weight very much.

If we admit, according to the general opinion, that the atomic weight of cerium is 92, the ordinary strongly basic oxide, which is the lower or suboxide, is represented by CeO, and the highest oxide, which give salts with the acids, is $Ce_3O_4 = CeO, Ce_2O_3$. By attributing this composition to the highest oxide, we still admit an oxide of the formula Ce_2O_3 ,† although this oxide has never yet been

obtained and none of its salts are known. Rammelsberg, Hermann, Holzmann, Zschiesche, &c., have obtained not only simple salts of the oxide Ce_3X —example, the yellow salt $Ce_3(SO_4)_4 \cdot 8H_2O$ —but also different salts of suboxide of cerium. Their composition is expressed by the formula $Ce_3X_8, nCeX_2$; however, we do not know of any salts which contain more acid residues than what corresponds to the form Ce_3X_8 . Therefore we should only admit there to be two actual forms of oxidation of Ce; these are—

CeO and Ce_3O_4 ($Ce=92$).

Its tenacity for oxygen is in the relation 3:4; therefore I think it would be more natural to give the formula Ce_2O_3 to the first of these oxides, and the formula Ce_2O_4 or CeO_2 to the second.* This is as good as giving Ce the atomic weight 138 (46×3), and giving it the corresponding place in the fourth group; this also agrees with the new formula of the highest oxide. The element Ce takes its place in the eighth series; it has as atomanalogues Ti, Zr, and Th in the fourth group, and Cs = 135 and Ba = 137† in the eighth series. The transition from CeO_2 to Ce_2O_3 corresponds to the transition from TiO_2 to Ti_2O_3 , and, better still, from PbO_2 to Pb_2O_3 and to PbO .‡ The higher oxide of cerium exhibits the properties of a peroxide, and its reactions resemble those of PbO_2 , Tl_2O_3 , and TeO_3 .

We can explain the feeble basic properties of CeO_2 by the fact that it is placed in the fourth group after TeO_2 , whose basic properties are very uncertain, and also after ZrO_2 , whose basic character is very doubtful. In the case of ThO_2 , which comes after CeO_2 , these properties are more doubtful still, its atomic weight being relatively higher than that of Ce. The strongly basic properties of Ce_2O_3 find their explanation in this circumstance, that PbO_2 , Tl_2O_3 , TiO_2 , and CuO also give more basic bodies when reduced. The fundamental properties of the oxides of cerium are explained by reason of the position of Ce in relation to the system: in an even series after Cs and Ba, whose properties are strongly basic. To the atomalogy of Ce with Ti, Zr, Th, corresponds their association in a great number of minerals—rare, it is true.

To show more completely that the place given to cerium in the system, if we adopt $Ce=138$ or 140, agrees in reality with its properties, I am going to show, comparatively, the known volumes and densities of the element and of the oxide. If, in an even series, we start from the first group, we notice an increase of density, such as the density of Rb = 1.5, that of Sr = 2.5, that of Zr = 4.2. The density of Cs is unknown; however, according to conclusions which I shall communicate later on, it ought to be about 2.5; the density of Ba = 3.6; of Ce = 5.5. The volume of Ti = 9.3, of Zr = 22, of Ce = 25, of Th = 30; consequently the volume increases here, as in the other groups, according as the atomic weight increases. The volume of $TiO_2=20$, of $ZrO_2=22$, of $CeO_2=24$, of $ThO_2=29$, that is to say, that in this series as in the parallel one, viz., $SiO_2=22$, $SnO_2=22$, $PbO_2=26$, the volumes of the higher oxides increase regularly, although only slightly, as the atomic weight becomes greater.

If, as is thus shown, the new position of cerium in the system corresponds to its properties, both chemical and physical, there are two motives why we should search for a new means of confirming the exactitude of the hypothesis given above, relative to a modification of the atomic weight of cerium. Firstly, CeO_2 is easily reduced to the

* Up to the present I do not see how it is possible to give the oxides the formulæ CeO_3 and CeO_4 , thus making $Ce=276$.

† There is good reason for the supposition that, in future experiments, the atomic weight of Ce will be found to be much greater, for at present it is very near that of Ba, and we can better afford to admit errors in the number found for Ce than in the carefully determined atomic weight of Ba. Slight impurities, such as La and Di, the difficulties of analysis, to which Marignac has called attention, as well as the difficulty in obtaining the compounds of suboxide of cerium free from compounds of oxide of cerium, are quite sufficient to account for the low atomic weight admitted by the periodic law. This is the reason I have provisionally given cerium the atomic weight 140.

‡ We can imagine that the ordinary oxides of Zr and Th will give basic suboxides Th_2O_3 and Zr_2O_3 . As far as the latter is concerned I purpose doing the necessary experiments, for I have recently received a considerable quantity of zircons from the Ilmen mountains (Siberia.)

* According to the determinations of Haeringer, Marignac, Hermann, Bunsen, and Rammelsberg, the equivalent of $Ce=46$; according to Wolff it is 45.66. For lanthanum, Rammelsberg found the figure 44.4; Zschiesche found 45.1; Holzmann found 46.3; Czudnowicz 46.8; Marignac from 46 to 47; and Erk 45.1. For didymium Zschiesche found from 46.6 to 48.1; Hermann, 46.7; Marignac, 48; and Erk, from 47.4 to 47.8.

† Popp and Hermann have mentioned other oxides, such as Ce_3O_5 and CeO_2 , but these assertions have not been confirmed by any other chemists, and Erk has contradicted them.

state of Ce_2O_3 . The highest atomic analogue of cerium, namely, Th, has not, as far as we know, a similar property,* although, from what has been previously said (see Chap. I.), the highest members of a group are the most easily reducible. Secondly, there are other elements which possess the same equivalent as Ce (46 in the suboxide, 34.5 in the oxide), or a very similar one (such as La and Di as suboxides, and Yt as oxide), which ought to be able to occupy a different position in the system. Therefore it seems to me that a more profound knowledge of the compounds of cerium is necessary before we can be certain of the exactitude of the modified atomic weight. Up to the present I have only made one determination of the specific heat of metallic cerium.† This metal was obtained from H. Schuchardt, of Goerlitz. The number 0.5—found from small half-melted lumps which had been dried in hydrogen—shows that the specific heat of cerium is less than that of tin, and equal to that of metallic barium found at the same time. The number found coincides better with the modified atomic weight than with the old one, for $0.05 \times 138 = 6.9$, whilst $0.05 \times 96 = 4.8$. However, it is desirable to repeat this experiment.

(To be continued.)

REPORT TO THE LOCAL GOVERNMENT BOARD AS TO TREATMENT OF SEWAGE.

By Dr. ANGUS SMITH, F.R.S.

As requested, I have the honour to submit a careful examination of five effluents, the results of processes for the purification of sewage water by different methods.

1. From Aldershot, where irrigation alone is used.
2. From Coventry, where precipitation by alum and iron is used first and irrigation afterwards.
3. From Birmingham where precipitation by lime is employed.
4. From Burnley where also lime is employed.
5. From Aylesbury where the A B C process is used, a precipitant of alum being employed, also of clay.

I shall extract from the larger tables some of the more important points; but I wish it to be observed that in this report I refer to quality of effluents only, and not at all to the ultimate value of processes.

FREE AMMONIA, A.

	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per cent of Total.	Parts per 100,000.	Per cent of Total.
Aldershot, <i>b</i> ..	11.755	97.96	0.245	2.04
Coventry	2.06	79.8	0.520	20.2
Aldershot, <i>a</i> ..	2.913	80.10	0.747	19.9
Aylesbury	2.980	74.5	1.020	25.5
Birmingham, <i>a</i>	1.425	52.9	1.275	47.1
Burnley	0.120	8.3	1.330	91.7
	added	added		
Birmingham, <i>b</i>	0.520	18.8	3.220	118.8

NOTE.—Aldershot (*a*) specimen collected in wet weather; (*b*) in dry weather; Birmingham (*a*) after lime precipitation and irrigation; (*b*) after lime alone.

Free includes saline ammonia. Its existence is of no disadvantage so far as the effect on the atmosphere is concerned, but its absorption by the soil is important as manure. Its amount indicates decomposition. So far as free ammonia is concerned, the first on the list is Aldershot during dry weather, specimen *b*; Coventry stands

* We can hardly believe that this phenomenon could have escaped detection if thorium had the property to any great extent, for it has been studied by Berzelius, Chydenius, and Deiafontaine, and their results agree. Unhappily, thorium cannot be studied much on account of its rarity.

† Bulletin de l'Académie des Sciences de St. Petersburg, 1870, p. 445.

second. There two processes are used, both precipitation (with alum, iron, and lime) and irrigation. The next is Aylesbury. The lime processes are certainly behind.

ALBUMINOID AMMONIA, B.

	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per cent of Total.	Parts per 100,000.	Per cent of Total.
Aldershot, <i>b</i> ..	5.195	98.95	0.0550	1.05
Coventry	1.639	96.40	0.061	3.60
Aylesbury	0.59	89.40	0.070	10.60
Aldershot, <i>a</i> ..	1.543	88.2	0.207	11.8
Birmingham, <i>a</i>	0.840	80.0	0.210	20.0
Burnley	0.895	74.6	0.305	25.4
Birmingham, <i>b</i>	0.390	37.1	0.660	62.9

On this table Aldershot on a dry day stands best, but not on a wet day. Coventry with its double system is next best. Aylesbury with precipitation alone is almost the same as Coventry with its double purification.

Perhaps this expression "on a dry day" is not quite fair, it is meant to show that in a case of an overflow there may be little or no purification, but there is generally some unless the flood be great.

RESIDUAL AMMONIA, C.

	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per cent of Total.	Parts per 100,000.	Per cent of Total.
Aldershot, <i>b</i> ..	11.36	100.0	0.00	0.00
Coventry	5.081	99.8	0.009	0.2
Aylesbury	4.269	88.0	0.581	12.0
Aldershot, <i>a</i> ..	3.177	83.0	0.636	17.0
Birmingham, <i>a</i>	3.351	61.4	2.109	38.6
Burnley	1.200	29.65	2.503	70.35
Birmingham, <i>b</i>	1.530	28.0	3.930	72.0

Aldershot (dry) is again the best; Coventry next; Aylesbury third.

The two chief ammonias in a sanitary point of view are the albuminoid and residual. They are therefore added here.

TABLE D.—TOTAL ORGANIC AMMONIA, *i.e.*, Albuminoid and Residual; both Ammonias may be called "possible."

	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per cent of Total.	Parts per 100,000.	Per cent of Total.
Aldershot, <i>b</i> ..	16.555	99.65	0.055	0.35
Coventry	6.720	98.97	0.070	1.03
Aylesbury	4.859	88.20	0.651	11.80
Aldershot, <i>a</i> ..	4.720	84.84	0.843	15.16
Birmingham, <i>a</i>	4.191	64.40	2.319	35.60
Burnley	2.095	42.73	2.808	57.27
Birmingham, <i>b</i>	1.920	29.60	4.590	70.40

TABLE E.—TOTAL AMMONIA.

	Sewage.		Effluent	
	Parts per 100,000.	Per cent of Total.	Parts per 100,000.	Per cent of Total.
Aldershot, <i>b</i> ..	28.31	98.9	0.300	1.1
Coventry	8.78	93.6	0.590	6.4
Aldershot, <i>a</i> ..	7.633	82.7	1.590	17.3
Aylesbury	7.839	82.4	1.671	17.6
Birmingham, <i>a</i>	5.616	60.9	3.594	39.1
Burnley	2.215	34.9	4.138	65.1
Birmingham, <i>b</i>	1.400	15.2	7.810	84.8

It is seen here that the lowest effluent for total ammonia is from the irrigation farm in dry weather. Coventry next. Aldershot (wet) and Aylesbury stand third nearly the same.

NITRIC ACID.

	Sewage.		Effluent	
	Parts per 100,000.	Per cent of Total.	Parts per 100,000.	Per cent of Total.
Aldershot, <i>b</i>	9.23	
Aylesbury	3.19	
Aldershot, <i>a</i>	2.79	
Birmingham, <i>b</i>	2.13	
Coventry	1.22	
Burnley	1.20	
Birmingham, <i>a</i>	1.19	

ANALYSES OF SPECIMENS OF WATERS FROM SEWAGE WORKS, EXPRESSED IN PARTS PER 100,000.

Samples from Burnley, collected June 16, 1879. (See Report by Dr. R. Angus Smith.)

	Organic Carbon.	Free Ammonia.	Albuminoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity as SO ₃ .	Hardness Before Boiling.	Hardness After Boiling.
Stream above sewage works.	0.476	0.025	0.024	0.075	0.124	0.50	3.26	1.28	17.0	3.0	20.0	7.50	10.81	7.78
Do. below ditto.. ..	0.987	0.236	0.070	0.219	0.525	0.56	4.12	1.90	18.5	6.5	25.0	9.00	12.64	8.22
Crude sewage	20.33	1.45	1.20	3.703	6.353	—	6.35	5.12	49.0	148.0	197.0	13.00	—	—
Effluent from sewage works.	3.737	1.33	0.305	2.503	4.138	1.20	7.38	5.06	40.0	14.0	54.0	13.00	20.92	17.16

Samples from Birmingham, collected June 23, 1879.

Crude sewage	20.44	2.70	1.05	5.46	9.21	none	25.23	24.16	102.0	75.0	177.0	13.83	45.0	44.0
Effluent from settling tanks.	4.09	3.22	0.66	3.93	7.81	2.13	22.65	15.78	74.0	26.5	100.5	11.32	39.0	38.4
Effluent after irrigation	2.303	1.275	0.21	2.109	3.594	1.19	18.19	11.46	88.5	24.0	112.5	37.77	66.8	29.22

Samples from Coventry, collected June 24, 1879.

Crude sewage	11.33	2.58	1.700	5.09	9.370	none	11.33	6.85	59.0	31.0	90.0	25.35	32.80	9.85
Effluent from settling tanks (filtered).	1.418	2.40	0.245	0.511	3.156	none	7.21	6.30	55.0	16.0	71.0	22.34	37.15	15.62
Effluent after irrigation (drain out of order).	0.587	2.12	0.205	0.895	3.220	1.22	13.21	5.73	58.0	19.0	77.0	12.62	35.45	17.65

Sample from Coventry, collected July 10, 1879.

Effluent after irrigation	0.618	0.52	0.061	0.009	0.590	1.62	13.39	4.99	58.5	12.0	70.5	21.0	40.70	19.72
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Samples from Aldershot, collected July 1, 1879 (wet weather).

Crude sewage	16.90	3.660	1.750	3.813	9.223	traces	2.04	4.38	41.0	32.0	73.0	9.40	6.71	3.38
Effluent "A" from sewage farm.	—	0.920	0.276	0.803	1.999	2.69	5.14	4.54	24.0	16.0	28.0	2.50	6.57	6.29
Effluent "B" from sewage farm.	2.282	0.880	0.200	0.788	1.868	1.59	1.89	3.14	15.0	11.0	26.0	1.90	5.00	4.57
Effluent "C" from sewage farm.	2.240	0.440	0.144	6.318	0.902	4.08	2.40	3.20	20.0	14.5	34.5	0.70	7.86	6.86

Collected September 13, 1879 (dry weather).

River Blackwater ..	0.536	0.0108	0.03	0.0542	0.095	1.56	2.40	2.46	32.50	6.00	38.50	20.40	28.55	10.88
Crude sewage (camps)	43.76	12.00	5.25	11.36	28.61	—	4.29	12.08	48.0	93.0	141.0	16.40	11.70	4.43
Effluent from sewage farm.	0.970	0.245	0.055	—	0.286	9.23	4.48	15.53	24.5	16.5	41.0	Neutral.	12.28	12.00

Samples from Aylesbury, collected July 2, 1879.

Crude sewage	6.76	4.00	0.66	4.85	9.510	none	14.42	7.97	76.0	29.0	105.0	35.50	48.6	20.0
Effluent from third settling tank.	0.633	1.02	0.07	0.581	1.671	3.19	30.21	5.44	76.0	13.5	89.5	11.20	66.40	37.1

Nitric acid shows the effect of oxidation on the organic matter, and here the great action of air and of a porous soil shows itself remarkably.

The best seems to be irrigation in dry weather when the land has full opportunity to act.

The use of precipitation has a decided advantage in wet weather as it raises the purity of the Coventry water above the Aldershot wet specimen. The precipitation method by alum, &c., at Aylesbury, is better in Tables B and C than in A. It is the best of the single processes in wet weather. Aldershot is the best of the single processes in dry. Coventry, the double process, is the best in wet weather. Those which are good in wet weather would probably show still better in dry if the specimens were taken frequently during the year. The precipitation processes, when alum or iron is used, have an advantage in wet weather since the act of precipitation becomes an act of disinfection.

The effluent specimens were examined as to their tendency to putrefy.

Those with lime changed most.

Birmingham (effluent) ammonia rose in 23 days from 3.222 to 4.2; then began to fall; Burnley in 27 days from 1.33 to 1.92; Aylesbury changed none in a week; Aldershot (dry), may be said not to have changed in 25 days.

The capacity to froth when shaken is a very useful mode of finding the comparative sewage matter rapidly. It may be said to have been absent in Aldershot (dry),

	Date.	Free Ammonia.	Albuminoid Ammonia.
Burnley, collected	June 20, 1879	1.33	0.305
	July 17, "	1.92	0.23
	June 16, 1879..	1.92	0.30
	Sept. 4, "	2.15	0.13
Birmingham, collected, June 23, 1879.. ..	June 25, "	3.222	0.662
	July 17, "	4.20	0.336
	" 18, "	4.15	0.336
	" 28, "	3.90	0.324
Aldershot (wet), effluent C ..	" 7, "	0.44	0.144
	" 28, "	0.364	0.130
	Sept. 4, "	0.140	0.092
Aldershot, dry ..	" 16, "	0.245	0.055
	Oct. 11, "	0.240	0.058
Aylesbury, collected July 2, 1879.. ..	July 23, "	0.96	0.067
	" 26, "	1.00	0.06
	" 29, "	0.96	0.06
	Sept. 4, "	0.045	0.035

Coventry, and Aylesbury effluents. The clearness of the liquid is very important although a popular indicator. In the Coventry sample there were a few white floating particles; when these fell the water was clear and colourless. Three specimens—Aldershot (dry), Coventry, and Aylesbury were without colour, when they had stood in a large colourless glass vessel.

The total solids are in this order in the effluents; the advantage is in being low, of course:—

Aldershot, average of three (wet) ..	29.5
Aldershot, dry	41.0
Burnley	54.0
Coventry	70.5
Aylesbury	89.5
Birmingham, <i>b</i>	100.5
Birmingham, <i>a</i>	112.5

During floods the volatile solids stand thus:—

Coventry	12.0
Aylesbury	13.5
Aldershot, average of three (wet) ..	13.8
Burnley	14.0
Birmingham, <i>a</i>	24.0
Birmingham, <i>b</i>	26.5

The chlorine has not been found the same in the effluent as in the sewage. This, I suppose, is owing to the effluent being from sewage of a time previous to that of the specimen of sewage.

The question has been asked which of these specimens is most suited for passing into a sewer. None of these effluents can be called sewer-water in the ordinary sense. The Birmingham stream froths readily, and is not pleasant to the eye. The Burnley water was pretty clear, would do well in appearance as a stream by itself, but caused a milky deposit in the river into which it fell, arising we may suppose from the free lime taking up some carbonic acid from the river water and precipitating carbonate of lime. The Coventry effluent went into a stream which was very impure, and it might, so far as appearance went, pass into a shallow mountain stream without being noticed. So of Aldershot and Aylesbury.

In every respect we may say that the best result has been obtained by irrigation when the weather is not so wet as to cause overflowing. Still it was not found highly successful after lime at Birmingham.

That the precipitation with alum or alum and iron compounds is next.

That in wet weather there is an advantage in precipitation because the action is largely, if not wholly, independent of dilution.

That the lime process is valuable but not equal to the above precipitation processes.

I must repeat that in this report I allude only to the merits of the effluent. There is a good deal to be examined before pronouncing on the ultimate value of the processes, and of course a large proportion, if not all of the remaining reasoning, must be left for Mr. Rawlinson, for whose report this may be considered as a preparation.

Manchester, October 16, 1879.

ON THE USE OF THE SPECTROSCOPE IN DISCRIMINATING ANTHRACENS.

By BENJ. NICKELS, F.C.S., F.I.C.

In the conversion of anthracen into anthraquinon by the method known as "Meister, Lucius, and Brüning's Anthraquinon Test with Appendix," results, or products, are obtained as varied, perhaps, as the crude anthracen originally submitted to examination, in place of well-formed and perfectly crystalline chinons, as might be anticipated from the very large excess of chromic acid employed in oxidising, the product is mostly an indefinite mixture of crystalline and uncrystalline substances, the latter well-known and generally referred to as "amorphous particles."

Varied opinions have been held concerning the nature of the substances and the inference to be derived from their presence; that they are not without considerable influence as regards the quality of an anthracen affording

them there is little doubt, and the appearance presented by a given sample after oxidation is generally taken into consideration in forming an estimate of its character and quality, indeed of classifying it.

In order, however, to determine these points it becomes necessary to carry through the special test referred to, and await the crystallisation and dilution of the oxidised product. To do this involves several hours, or a day at least, and in cases where it is wished to arrive at a rapid conclusion inconveniently long.

For the guidance of those interested in the question (and from the number of enquiries put to me, I believe there are many), I am able to state that the information sought may within a very close limit be arrived at without chromic acid oxidation and within a few minutes, by simply subjecting the given sample to spectroscopic examination. Pure anthracen, and most of the hydrocarbons associated with it in the crude state, afford clear and continuous spectrums, free from absorption bands. Certain substances, however, also present with it in that state, possessing very high fusing-points, exhibit deep and broad absorption bands. The former bodies in no wise affect the crystallisation of anthraquinon as formed under the oxidation test; the latter, on the other hand, seriously interfere with that property, and afford the so-called and objectionable "amorphous particles," frequently present in sufficient quantity to obliterate all traces of crystallisation in the oxidised product.

As a result of long-continued experience in anthracen testing, and in which I have employed the spectroscope as an adjunct, I am in a position to state that the intensity of the absorption bands afforded by a given sample of crude anthracen are in close proportion to the quantity of "amorphous substances" to be found and remaining after the chromic acid oxidation.

In a communication to follow this paper I purpose giving details and method of examination as written some fifteen months back, but withheld from publication in deference to views held at the time.

104, Leadenhall Street, E.C.

ON THE ESTIMATION OF THE NITROUS COMPOUNDS IN THE VARIOUS STAGES OF THE MANUFACTURE OF SULPHURIC ACID.*

By JAMES MACTEAR, F.C.S., F.I.C.

(Continued from p. 44.)

8. In a more recent paper, communicated to the Tyne Chemical Society, Dr. Lunge criticises some remarks made by Dr. Affleck in his address at the beginning of the session, in reference to the impurities of the nitrous vitriol affecting the testing by the permanganate process, as follows:—

"But these impurities which are mentioned by Dr. Affleck are really of no importance. The percentage of arsenic present in sulphuric acid is very small in any case, but seeing that by the action of the higher oxides of nitrogen, arsenious acid is converted into arsenic acid, and, that the latter would not be affected by the permanganate, we may safely leave the arsenic out of consideration when testing the nitrous vitriol of our absorbing columns. It is true that this very fact, the oxidation of arsenious to arsenic acid, will cause a slight loss of nitre in sulphuric acid making, a matter not very generally noticed, but the question of testing an absorbing column acid is not affected thereby to an appreciable extent, as the latter must contain its slight percentage of arsenic nearly, if not all, in the form of arsenic acid."

The assumption that the amount of arsenic "is very small in any case," is not, I think, warranted by the facts.

* A Paper read before the Newcastle-upon-Tyne Chemical Society

The analyses published by H. A. Smith* are pretty conclusive on this point. He gives as a mean of 15 analyses the amount calculated as arsenic trioxide of 1.051 per cent, the maximum being 1.112, and the minimum 0.800 per cent. He gives the following useful table:—

Tons.		Tons.
		As ₂ O ₃ .
100	Hard Norwegian pyrites contains	
	before burning	1.649
	Ditto after burning	0.465
100	Ditto made 140.875 H ₂ SO ₄ con-	
	taining	1.481
140.875	Sulphuric acid made 104.9 HCl	0.724
"	Ditto 204.12 Na ₂ SO ₄	0.059

This amount of 1.051 per cent, if calculated on the N₂O₃ in the Gay-Lussac acid, will be actually some 20 per cent of the N₂O₃ present, a quantity which cannot be considered "very small," being quite sufficient to cause a notable error, more especially as it by no means has been proved to exist as arsenic acids.

Gmelin gives a list of investigators, and says it apparently exists in sulphuric acid, for the most part, as arsenious acid; and Hofmann quotes Bussy and Buignet and others as to the existence of both arsenious and arsenic acids, and also quotes Blondlot as recommending manganate of potash as a means of converting the arsenious into arsenic acid prior to its removal by distillation.

I think the balance of proof is clearly in favour of the opinion that the arsenic compounds present do interfere with the permanganate of potash process as the "most accurate of all" the processes for the testing of nitrous vitriol, as claimed by Dr. Lunge.

It is not to be wondered at that there should be arsenious acids present, as it is not by any means unusual to find SO₂ in the Gay-Lussac acid. In fact the "nitrous vitriol" may be looked on as containing, under various conditions, the following compounds:—

Nitrous acid	N ₂ O ₃
Nitric acid	N ₂ O ₅
Nitric oxide	N ₂ O ₂
Sulphurous acid	SO ₂
Arsenious acid.. .. .	As ₂ O ₃
Arsenic acid	As ₂ O ₅

With traces of metallic salts, and it may be—

Nitric peroxide	N ₂ O ₄
-------------------------	-------------------------------

But as this latter will act simply as a mixture of nitric and nitrous acids, it may be considered as coming under these heads.

Now, in testing with permanganate, the amount required will be in proportion to the oxygen required to oxidise these compounds, and may be shown in this way. Taking N₂O₃ as a standard, and assuming it to require a quantity of permanganate equal to 100, we would have for—

N ₂ O ₃	100.00
N ₂ O ₄	41.30
N ₂ O ₂	190.02
SO ₂	59.38
As ₂ O ₃	38.38

Suppose a case where the permanganate required in testing a sample of nitrous vitriol was equal to 5 vols. per cent of N₂O₃, let us examine the effect on the test caused by the presence of these various impurities, assuming that they exist to the extent of 1 vol. per cent, then—

	Per Cent.	
N ₂ O ₃ by permanganate	5.000 vols.	= 100.00
N ₂ O ₅ no effect	5.000	= 100.00
N ₂ O ₄ will give results as	4.586	0.414 too low = 91.72
N ₂ O ₂	6.900	1.900 too high = 138.00
As ₂ O ₃	4.616	0.384 too low = 92.32
SO ₂	4.406	0.594 too low = 88.12

* "The Chemistry of the Sulphuric Acid Manufacture."

These facts are sufficient to show that the permanganate method of testing cannot be looked upon as an absolutely perfect method; still there is no doubt that for technical purposes of ordinary testing of nitrous acid it is much the more convenient, and sufficiently accurate for such purposes.

The presence of arsenic in sulphuric acid was first announced by Dr. G. O. Rees,† who states the amount found by him as 22½ grains in 20 fluid ounces of supposed pure acid (stated as white arsenic); and in another sample he found even more than this amount. H. Watson‡ stated in the same journal that he had known for some months of the presence of this impurity, and had made examinations of various samples; he states the minimum quantity which he found at 35½ grains in a pint of acid, and his latest result as giving 5½ grains in 100 grains weight of acid = 0.55 per cent. Dr. O'Reilly§ gives 0.60 per cent as the amount he found. Smith gives the results already referred to. More recently I have had some tests made of the acid in the works of my firm at Hebburn, which show the quantity present under the ordinary conditions of manufacture of sulphuric acid from pyrites. The set of chambers from which the samples were drawn for examination was arranged in a series of three.

The arsenic is all calculated to arsenious acid—

SERIES A₃, 11TH DECEMBER, 1877.

	Per cent.
Acid from denitrating column (Glover tower)	0.286 by vol.
" bulk of 1st chamber	0.292 "
" " 2nd	0.060 "
" " 3rd	0.010 "
" absorbing column (Gay-Lussac)	0.200 "

In order to check these results, a second series of examples were drawn and tested.

SERIES A₃, 19TH DECEMBER, 1877.

	Per cent.
Acid from denitrating column (Glover tower)	0.210 by vol.
" bulk of 1st chamber	0.224 "
" " 2nd	0.031 "
" " 3rd	0.010 "
" absorbing column (Gay-Lussac)	0.280 "

It is not unusual to find crystals of arsenious acid in the flues leading to the chambers; and I have here a specimen which I obtained lately of crystalline deposit from the vessel under the drip of an inlet pipe from the Glover tower to the first chamber. Some of the crystals in this specimen are quite large, and their form is perfectly distinct. Arsenic acid has been found crystallised on the bottom of the leaden chambers.¶

The question as to the state of oxidation of the arsenic is a most difficult one to decide. The deposit in the pipe leading from the Gay-Lussac tower to the chimney was examined with the following result:

Deposit from draft-pipe from Gay-Lussac towers to chimney. A whitish mud-like deposit, evidently containing a considerable quantity of nitrous compounds, which were given off on adding water to a portion of the mud. After washing until free from acid reaction it was dried, and then found to contain in the dry state 59.7 per cent of arsenious acid.

This shows that whether it had passed through all the series of plant or no, that it can exist as As₂O₃ in presence of nitrous compounds passing away from the Gay-Lussac towers and condensing in the main outlet flue.

The question as to the presence of N₂O₅ in sulphuric acid is, I think, quite beyond a doubt; and the following experiments are conclusive on the point:

I turned on full steam in the last but one and the last chambers of a series of vitriol chambers, composed in all

† London Medical Gazette, February 5, 1841.

‡ Ibid., February 19, 1841.

§ Dublin Medical Journal, 1842, 442.

¶ H. A. Smith, "Sulphuric Acid Manufacture."

of eight chambers. Within five minutes the colour of the gases, as seen through glasses, originally of a normal rich tint, became decidedly pale; in ten minutes the gases were reduced to a slight straw tint; samples were then collected over about fifteen minutes from—

(a) The drip in connection pipe between the second last and last chambers.

(b) The layer of dilute acid formed on the floor of last chamber (this at end of fifteen minutes.)

(c) The drip at last outlet from chamber to Gay-Lussac column.

(d) The Gay-Lussac acid.

These were then examined by testing with permanganate, and also by the ammonia process with zinc and iron previously described, and the following were the results obtained:—

(a) This sample contained a good deal of sulphate of lead, evidently washed off the sides of the connection pipe by the increased flow of dilute acid. It settled rapidly, and was then remarkably clear and brilliant. Its strength was 38° T. It smelt *strongly* of nitrous compounds. A portion of it on being shaken in a bottle gave off a considerable quantity of gas, which was apparently colourless, but became coloured on contact with air.

(a) It gave on examination in 100 c.c.:—

1. By ammonia process 0.42280 grm. N
2. „ permanganate „ 0.00077 „ N=0.00209 N_2O_5

Excess N. from 1 0.42203 „ N=1.6278 N_2O_5

99.91 per cent of the total N being in the state of N_2O_5 .

(b) This sample presented much the same characteristics as a, with the exception of its not containing sulphate of lead in appreciable quantity. Its strength was 73° T.

It gave in 100 c.c.:—

1. By ammonia process 1.2348 grm. N
2. „ permanganate „ 0.0135 „ N=0.03664 N_2O_5

Excess N from 1 1.2213 „ N=4.71067 N_2O_5

98.91 per cent of the total N being in the state of N_2O_5 .

(c) This sample smelt *very* strongly indeed of nitrous compounds, and gave off gas in quantity on agitation as in a. Its strength was 57° T.

It gave in 100 c.c.:—

1. By ammonia process 1.23760 grm. N
2. „ permanganate „ 0.00721 „ N=0.01957 N_2O_5

Excess N from 1 1.23039 „ N=4.7457 N_2O_5

99.42 per cent of the total N being in the state of N_2O_5

(d) This sample was perfectly normal in appearance. Its strength was 149° T.

It gave in 100 c.c.:—

1. By ammonia process 0.3469 grm. N
2. „ permanganate „ 0.326375 „ N=0.08859 N_2O_5

Excess of N from 1 0.020525 „ N=0.07917 N_2O_5

5.92 of the total N present being in the state of N_2O_5

(To be continued.)

Action of Ozone on Certain Noble Metals.—Prof. A. Volta.—Silver quickly becomes covered with a layer of black oxide. Gold is perfectly unaffected by ozone, whether moist or dry. Platinum gave the same results as gold. Palladium, if perfectly freed from hydrogen, is polarised negatively in ozone, like the other noble metals, and even in a moist atmosphere it does not decompose ozone. Mercury is superficially oxidised by ozone, whether moist or dry.—*Gazzetta Chimica Italiana*.

ANCIENT METHODS OF FILTRATION.*

By H. CARRINGTON BOLTON, Ph.D.,

Professor of Chemistry in Trinity College, Hartford.

THE separation of a liquid from solids suspended in it, by straining through some material previous to the one and impenetrable to the other, was a familiar process in the remotest antiquity. Observations of various processes in nature, such as the purification of water by trickling through sandy soil, or perhaps the accidental passage of rain-water through an out-stretched cloth, a garment, or a tent-cover, would obviously suggest the simple expedient. History fails to record the period of the invention or the name of the individual who first put it in practice. Etymological considerations show that filters were early made of fulled wool or felt; the Latin *filtrum*, a “filter,” being closely connected with *feltrum*, “felt,” or compressed wool, and both are related to the Greek *πιλος*, signifying hair.

Several writers on the history of science make casual reference to the operations performed by the early chemists: some state in a general way their acquaintance with the processes of distillation, sublimation, filtration, &c.; others are more explicit. Hermann Kopp, in his exhaustive “Geschichte der Chemie” (vol. ii., p. 26), states that “filtration as a chemical process was first accurately described by Geber, who calls it by a special name, *destillatio per filtrum*, ‘trickling through a filter,’ in contrast to the collection of a liquid by ordinary distillation.” Ferdinand Hoefer, in his “Histoire de Chimie” (vol. i., p. 335), writing of Geber, mentions that he recognises two kinds of distillation—with and without fire—the former being “per alembicum,” and the latter consisting of “une simple filtration.”

Now, we propose to show that the ancients carried on the operation of filtration in two ways, essentially distinct in principle and in the manner of execution, and that these methods were characterised by two different expressions which have been confounded by the authors named. Moreover, we shall establish this by quotations from writings covering a period of more than two thousand years.

In the first place, an examination of the very passage in Geber’s Works, referred to by Kopp and by Hoefer, shows that the method therein described differs radically from filtration as ordinarily conducted at the present time. We quote the passage as found in the Works of Geber, “the most famous Arabian Prince and Philosopher,” faithfully Englished by R[ichard] R[ussell], and printed at London in 1678. In the thirteenth chapter of the fourth part of the first book of the “Summe of Perfection,” Geber treats of the three kinds of distillation—by an “Alembic,” by a “Decensory,” and “by Filter.” After describing in quaint language the well-known method of using the alembic and the decensory (which differs chiefly in the application of heat on the top of the apparatus), Geber writes thus of filtration:—“The *Disposition* of that which is made by *Filter* is, that the *Liquor* to be *Distilled* be put into a *Stone Concha*, and the wider part of the *Filter* put into the said *Liquor*, even to the *Bottom* of the *Concha*, but the narrower part of it hang out over the *Orifice* of the said *Vessel*. And under that end of the *Filter* must be set another *Vessel* for receiving the *Distillation*. Therefore when the *Filter* begins to *Distill*, the *Water* with which it was moistened will first *Distill* off; which ceasing, the *Liquor* to be *Distilled* succeeds. Which *Liquor*, if it be not as yet serene, it must so often be put into the *Concha* again and redistilled, as until it be *Distilled* most serene.”

This dates from the eighth century, and evidently describes a sort of capillary syphoning. The expressions “placing the wider part of the filter” into the liquid and allowing the “narrower part of it to hang out over” the

* Read before the New York Academy of Sciences, October 13, 1879

Vessel admit of no other interpretation. For convenience of distinguishing this method of filtration from that in which porous sacs are employed, we propose to name the former *anethisis*, a word made from *ἀνά*, "upward," and *ἡδισις*, "a straining off." A study of the chemical works of the middle ages further shows that the expression "*destillato per filtrum*" is invariably used to describe *anethisis*, while "*filtratio*" is applied to ordinary filtration. We shall give quotations proving this, but first make brief reference to early records.

The ancient Egyptians portray in their rock-cut memorials the operation of filtration in connection with the manufacture of wine. Their simple wine-press consisted of a bag in which the grapes were placed, and squeezed by means of two poles turned in contrary directions. Small colanders of bronze have been found at Thebes. Views of the interior of an Egyptian kitchen, cut in the tomb of Rameses III. at Thebes, represent syphons in use for drawing off liquids of various kinds. (Wilkinson.) The ancient Romans employed strainers and colanders (*colum*) made of a great variety of materials. Wine-strainers were made of silver and bronze; the poorer classes used linen, and where nicety was not required they used those made of broom or of rushes. Strictly speaking, however, percolation through colanders is not filtration, for capillary action plays no part.

It is interesting to note that the earliest mention of filtration which a brief search has disclosed refers to the method we have ventured to call *anethisis*. This occurs in Plato's "Symposium," written about four hundred years before the Christian era. The passage is as follows:—"Socrates then sitting down, observed, 'It would be well, Agatho, if wisdom were a thing of such a kind as to flow from the party filled with it to the one who is less so, when they touch each other, like water in vessels running by means of a thread of wool from the fuller vessel into the emptier.'""*

Aristotle, the pupil of Plato, in his essay "De Generatione Animalia," refers to the other process in the following words:—"Flesh is produced, therefore, through the veins and pores, the nutriment being deduced in the same manner as water through earthen vessels not sufficiently baked."

This passage, together with others occurring in Plato, shows that both systems of filtration were employed at that early period.

Geber, whose clear description of *anethisis* we have quoted, was followed by the celebrated Arabian physician, Rhazes: he uses the same expression, "*destillatio per filtrum*," in the following passage:—"Dissolve as much [common salt] as you wish in five times as much warm soft water, and distill per filtrum and congeal [*i.e.*, crystallise]."† Rhazes died about 930 A.D.

Among the early writers on alchemy, no one is oftener quoted than Raymund Lulli, surnamed the Enlightened Doctor (born 1235, died 1315 A.D.) In his works we find the following characteristic passage:—"Take, in the Name of God, great Bay Salt, as it is made of the Sea; take a good quantity and stamp very small into a stone mortar: then take Cucurbites of Glass and pour your salt therein: then take fair Well-water, and let your salt resolve into clear water; being all dissolved then distil it by Filter; that is to say, hang a jag Felt or Woolen cloath in the Cucurbite; and let the other end hang in another glass beside it, set as it were under it, that the water may drop into it that the Felt or Cloath may draw out and that shall be clear as silver."

This unmistakable description of *anethisis* occurs in the first chapter of a booklet bearing the following title: "Philosophical and Chymical Experiments of the Famous Philosopher Raymund Lulli . . . wherein is contained . . . the admirable and perfect way of making the great Stone of the Philosophers as . . . sometime practised in

England by Raymund Lulli in the time of King Edward the Third." London, 1657.

Thomas Aquinas, the eminent scholastic teacher of the thirteenth century, who is best known by his theological and metaphysical works, also paid some attention to scientific pursuits, possibly acquiring this taste from his learned master, Albertus Magnus. Aquinas, or the divine Thomas, as he was called by his admirers, defines distillation to be the "purification of waters falling drop by drop, and effected by placing a filter cut in the shape of an iron dart in the little dish containing the water to be distilled."*

Libavius, in his remarkable work, "Alchymia," sometimes called the first text-book of chemistry (published in 1595), devotes two entire chapters to the subject of distillation and of filtration.† In the fourteenth chapter he describes, with much attention to detail, the manner of filtering by means of pieces of felt (*lacinia*) shaped like an ox-tongue, the broader portion of which is placed in the vessel containing the liquid to be filtered, and "the apex in the recipient, or, if the vessel has a narrower neck, in a suitable funnel." This demonstrates that the method was not resorted to on account of the want of proper funnels, and suggests that perhaps a special virtue was attributed to a liquid thus purified.

Libavius's work contains rude woodcuts illustrating different methods of procedure. For perfecting the purification a series of four vessels was used. These were placed on steps, one above another, and the liquid passed through a capillary syphon from the uppermost to the one immediately below, and thence by another syphon to the third vessel, and so on to the fourth. This series of vessels can be enclosed in a glass-covered box for filtering volatile liquids. Another woodcut represents the lower end of a capillary syphon hanging into a funnel inserted in the neck of a flask. The method thus clearly portrayed is called *destillatio per lacinias*, and is evidently regarded as a process of distillation; ordinary filtration through porous stones and through bibulous paper is treated in another and following chapter.

Libavius makes reference to "Hippocrates' Sleeves,"‡ by which name were designated conical felt bags used in filtration.

Sir Robert Boyle, in his "Experiments touching the Spring of the Air," writes as follows: "Some learned mathematicians have of late ingeniously endeavoured to reduce filters to syphons, but still the true cause of the ascension of water and other liquids, both in syphons and in filtration [requires], a clever discovery and explication."§ And in another place he gives this "explication": "The parts of the filter that touch the water being swelled by the ingress of it to their pores are thereby made to lift up the water till it touch the superior parts of the filter that are almost contiguous to them; by which means, these being also wetted and swelled, raise the water to the other neighbouring parts of the filter till it have reached the top of it, whence its own gravity will make it descend."||

These passages can only apply to *anethisis*, which was apparently a common method of filtration in Boyle's day.

Again, to trace this process still later, Juncker, in his "Conspēctus Chēmiæ," published in 1730, describes seven kinds of filtration. These differ chiefly in respect to the

* "Pretiosa Margarita Novella" of Petrus Bonus (1330), Venetia, 1546.

† "Comment. Alchymia," Part I., lib. iii., edition 1606.

‡ The origin of this curious term we have been unable to discover, nor is it of common occurrence in early writings on chemistry and pharmacy. The only explanation of the expression which we have as yet found occurs in the "Lexicon novum Medicum Græco-Latinum" of Stephen Blancardus, published in 1702. This author writes as follows:—"Manica Hippocratis est sacculus laneus figura pyramidalis, quo vina aromatica et medicamentosa, aliique liquores percolantur; ex ὑπο sub et κεραυνῶν misceo."

§ Boyle's Works, London, 1772, vol. i., p. 79.

|| Idem., vol. iii., p. 223.

* Plato's works, Burges's translation, vol. iii., p. 480 (Bohn).

† "Collect. ex Rhasi in Margarita Pretiosa Novella" of Petrus Bonus (1330), Venetia, 1546.

materials used: two methods, however, are essentially distinct. The one is styled "*filtratio per chartam bibulam . . . in fundibulo vitreo*" (filtration through bibulous paper in a glass funnel), and the other is described in the words "*per segmentum panni lanei vel laciniam bombycinam vel funiculos gossypii*" (through shreds of woollen cloth, silken threads, or through little strings of cotton).

Our friend Prof. S. A. Lattimore sends us another reference to this process from "The Laboratory or School of Arts, &c., compiled by G. Smith, sixth edition, London, 1799" (vol. i., p. 435), the passage is as follows:—

"To Separate Water from Wine.—Put into the cask a wick of cotton, which should soak in the wine by one end and come out of the cask at the bung-hole by the other, and every drop of water which may happen to be mixed with the wine will still out by that wick or filter."

Thus we see that, so recently as the close of the last century, anethisis was accounted a practical method of filtration.

We have not found this method in Boerhaave (1727), nor in Lemery (1675), nor does Faraday, in his "Chemical Manipulations," make any allusion to it. As a process of purification of solutions it seems to have been lost sight of in modern laboratory practice.

We have made trial of the method rendered noteworthy by more than two thousand years' practice, and find that, while the process is quite slow, it has certain advantages. Chief among these is the fact that, when the capillary syphon is adjusted, it requires no further attention; there is no pouring into a constantly emptying funnel, and there is no possibility of overflow. When properly arranged, the last drop of liquid passes from the upper vessel to the lower, and, except in certain cases of extreme divisibility, the filtration is perfect. The process seems particularly adapted to the purification of weak solutions, as of mineral waters, where the insoluble portion is not to be conserved. There is obviously much choice in the material of which the capillary syphon is made. We have tried cotton, wool, lamp-wick, and asbestos, and we find stout silky wads of the latter most serviceable; it is also useful for filtering very acid and alkaline solutions.

The rapidity of filtration does not seem to be hastened by lengthening the longer arm of the syphon, and is chiefly dependent on the number of threads in the filter, and on their fineness. In one experiment, thirty-two strands of cotton yarn filtered twice as fast as sixteen strands, and only half as fast as sixty-four strands. Oil filters much slower than saline solutions, and the latter much slower than pure water.

We have used the expression "capillary syphoning" in describing anethisis, and perhaps it needs justification. The ascension of the liquid is due to capillarity, and the descent through the longer arm of the syphon is in obedience to gravity. Syphoning is dependent on atmospheric pressure, and cannot be strictly applied to the present case; yet we find by experiment that, if the lower arm of the woollen threads be raised to the level of the liquid to be filtered, the action ceases, and the form of a syphon is at all events essential to the process.

The quotations from ancient authors show that they must have been quite familiar with capillary attraction, yet the first observance of this phenomenon is attributed by some authorities to Franciscus Aggiunti, physician to the Grand Duke of Tuscany, about the beginning of the seventeenth century.† (Aggiunti died 1635.)

In conclusion, the object of this paper is not to propose a return to this ancient method of filtration, the modern rapid processes being more in accordance with present needs; but we have thought it not altogether useless to call attention to a neglected process which can in certain cases be employed with advantage. Where the object of filtration is to collect the insoluble portion, it is obviously of no value; whether the process could be advantageously used on a large scale remains to be tested.

Gehler's "Physikalisches Wörterbuch," article "Capillarität."

SALE OF UNFERMENTED WINE.

ON the 12th ult., at the Salford Borough Police Court, William Pilling, chemist and druggist, was summoned for having sold, "to the prejudice of the purchaser, one bottle of unfermented port wine which was not of the nature, substance, and quality of the article demanded." Another summons charged the defendant with selling similarly a bottle of unfermented sherry.

Mr. J. CARTER BELL, the public analyst for the borough, stated that he received the samples in question on the 23rd November, and subsequently analysed them. With reference to the "unfermented sherry wine" he only found .91 of ash, which was considerably less than one-tenth of what ought to be found in pure grape juice. This ash contained a trace of potash and a trace of phosphoric acid. It consisted chiefly of sulphate of sodium and calcium, with a few other mineral ingredients. 52 per cent of it was only soluble in water. This was no more ash than could be got from ordinary drinking water. In answer to the Bench, the witness said the sherry wine contained about 6.10ths per cent of tartaric acid and about 22 per cent of sugar. He considered that this so-called sherry wine was a solution of sugar and tartaric acid, flavoured and coloured, and containing a little salicylic acid. The port wine was very similar to the sherry.

For the defence it was contended that what was sold to the inspector was of the nature, substance, and quality of the article demanded. There were other materials, it was said, used in the manufacture, but it was manufactured mainly from the juice of the grape.

The Stipendiary remarked that his opinion at present was that, supposing there was a substantial portion of the grape juice in the wine, the label could not be deceptive, but if there were only 1 per cent of grape juice added to it he should consider it a matter of fraud.

Mr. GREEN called attention to the fact, although his defence did not depend upon it, that orange wine had not a particle of orange juice in it.

JOHN HOUSTON, the manager for Messrs. Bell, said it was his business to manufacture the wine in question. The process of manufacture was to preserve the grape juice with salicylic acid. To five parts of water he generally put one part of grape juice. To six gallons of this liquid was added two pounds of sugar.

Mr. LOUIS SIEBOLD, F.C.S., had examined one of the samples of the wine in question. The predominant constituent of it was sugar, precisely the same as was contained in grapes. He also found tartaric acid exactly the same as was found in grapes. He found the constituents of grape juice present, but whether grapes or separate materials had been used he could not say. His personal opinion was that something like 10 per cent of grape juice was used in the wine.

Mr. MAKINSON, in dismissing the summons, said that there was no standard of what grape juice the wine should contain, and it could not be said that the manufacturers had fraudulently made this wine from water, for there was evidence that some proportion of grape juice was used.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Ordinary Meeting, January 24, 1880.

Prof. W. G. ADAMS in the Chair.

NEW member—Mr. W. Ellis.

Mr. GRANT read a paper and exhibited experiments on the "Induction in Telephonic Circuits." He was led to these experiments by a former observation that when an

induction coil primary was placed in a circuit consisting of a telephone, microphone, and battery, the microphonic sounds heard in the telephone were increased on closing the secondary circuit of the coil. Employing a double wound coil, that is having primary and secondary side by side, he found that the latter could act as a condenser and "relay" or translate messages into a second circuit, the microphone and battery being in the circuit of one wire (*i.e.*, the primary), and the other wire (or secondary) containing a telephone. He also inserted a double wound coil in the latter, or secondary circuit, and caused the induced or translated current to flow through both of the wires of this double coil one after another in the same direction. The effect was weak, but on reversing the current in one-half of the double coil by means of a commutator, so as to make it double on itself as it were, the weakening effect of induction was neutralised, and the sounds heard were as loud as if no coil had been inserted in the secondary circuit at all, as was proved by short circuiting the double coil altogether.

Dr. O. J. LODGE read a paper on "*Intermittent Currents and the Theory of the Induction Balance.*" The telephone as a scientific instrument seems destined to play an important part as a detector of minute currents of rapidly changing intensity, and the general theory of intermittent currents is being brought into prominence by its use. The equations to which most attention has been hitherto directed have been those relating to the steady flow of a current after the initial inductive or inertia-like effects have subsided. The galvanometer is essentially an instrument for measuring steady currents, or for giving the algebraically integrated expression for the total quantity of electricity which has passed in the case of transient currents. But the telephone plate has a very small period of swing compared to a needle, and, moreover, the plate is not limited to one mode of vibration like the needle. The induction balance was used experimentally by Done and Felici, but was not appreciated as an instrument of research till Prof. Hughes applied to it the telephone and an intermittent current. The general theory of the establishment of a current in circuits of known resistance was given by Thomson, and is to be found in Maxwell's "Electricity." Dr. Lodge used this theory in order to work out the theory of the induction balance, and one or two other cases of intermittent currents as completely as possible without taking into account the electrostatic capacity of the wires and leakage. The current in either primary of the balance is the same, and the current in either secondary is the same at every instant of time. In fact, the separating of the two halves of the circuits is immaterial to the theory. The current induced in the secondary circuit is a tertiary current induced from the piece of conducting matter inserted between the primary and secondary. An expression being got for the strength of current in the telephone at any instant after a change in the resistance of the primary has occurred, the author deduces among other things the law according to which a small coin by its position and size disturbs the balance. Dr. Lodge remarked that Prof. Hughes, either by inventive intention or great pains, had hit upon the best form of the apparatus for his purpose. The paper, which is very complete, is to be published in the *Philosophical Magazine* for February.

Herr FABER then exhibited his new Speaking Machine, which is designed to imitate mechanically the utterances of the human voice by means of artificial organs of articulation made on the human model, and actuated by an operator who depresses certain keys, as in playing a musical instrument. The organs are a bellows made of wood and india-rubber, which answers to the lungs; a small windmill placed in front of the latter to give the "r" or trilling sounds; a larynx, made of a single membrane of hippopotamus hide and india-rubber, to give the "drone" or basic tone of the voice; a mouth, with two lips, a tongue, and a nose or proboscis made of india-rubber tubing, placed below the mouth, but curving up towards it. Fourteen distinct vocal sounds can be

uttered by the instrument, but in combining these any word in any language can be played by the keys. Thus Herr Faber caused his machine to say such words as "Mariana," "Eliza," "Philadelphia," "Constantinople," and various sentences in French, English, and German, more or less distinctly. Laughing and whispering were also produced, and the voice of the instrument, which was ordinarily loud and clear, and resembling that of a girl, was lowered in pitch and loudness to a more masculine tone.

CORRESPONDENCE.

WATER OF THE CARISBROOKE CASTLE WELL.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 10, Mr. G. W. Wigner refers to my note on the water of the Carisbrooke Well, and I am interested to see his figures for the ammonia, which my sample did not allow me to obtain. I am certainly surprised to find any analysis of the water showing an amount of volatile matter in the water so much lower than the 19.6 grains per gallon, which I found, as 0.18 grain. Is not this a most exceptionally small amount for even a water of great natural purity, which this is certainly not? The point which I wished to emphasise was the necessity, in pronouncing on the quality of a sample of water, of giving due weight to *every* characteristic which it displays, and not sum up merely from the determination of what one might call "the three or four orthodox constituents of a water." The desirability of a more general recognition by chemists of this fact has been very ably dwelt upon by Mr. Wigner himself (*Analyst*, vol. iii., p. 208).—I am, &c.,

H. GRIMSHAW.

Gibbons Street Chemical Works, Bradford,
Manchester, January 19, 1880.

ALCOHOL TABLES.

To the Editor of the Chemical News.

SIR,—It would be a great desideratum to have a table showing the comparative strength of alcohol as indicated by "proof" as practised in England, and percentage by volume according to Tralles and Gay-Lussac's tables, as in use on the Continent. It is strange that neither in Ure, Watts, or Muspratt, are any such tables to be found.

As a full table would occupy too much space, I give merely the formula by which any given strength in proof-spirit may be calculated in percentage by volume or *vice versa*.

Proof-spirit as accepted by the English Custom House = 0.9186 sp. gr., at 60° F., and contains 57.27 per cent by volume.

O = overproof.

T = (Tralles) percentage.

U = underproof.

Formula 1.—To change overproof into percentage.

$$T = O \times 0.5727 + 57.27.$$

Example.—How much is 66.1 overproof in percentage?

$$T = 66.1 \times 0.5727 + 57.27 = 95.12.$$

On reference to the ordinary tables it will be seen that 66.1 overproof = 0.8156 sp. gr., and that 95.12 per cent = 0.8157 sp. gr.

Formula 2.—To change percentage into overproof.

$$O = \frac{T - 57.27}{0.5727}.$$

Example.—95·12 per cent. How much overproof?

$$O = \frac{95 \cdot 12 - 57 \cdot 27}{0 \cdot 5727} = 66 \cdot 1.$$

Formula 3.— $T = 57 \cdot 27 - (U \times 0 \cdot 5727)$.

Example.—20 underproof to change into Tralles.

$$T = 57 \cdot 27 - (20 \times 0 \cdot 5727) = 45 \cdot 55.$$

Formula 4.— $U = \frac{57 \cdot 27 - T}{0 \cdot 5727}$.

Example.—45·55 Tralles to change into underproof.

$$U = \frac{57 \cdot 27 - 45 \cdot 55}{0 \cdot 5727} = 20.$$

—I am, &c.

S. COHNÉ.

65, Gracechurch Street, London, E.C.,
January 22, 1880.

“REDONDA” PHOSPHATE.

To the Editor of the Chemical News.

SIR,—It may not be generally known that there are at the present moment lying idle in the country a good many thousand tons of “Redonda” phosphate (a rich phosphate of alumina and iron), most of which was imported some years ago for various methods of treatment, but all these processes have hitherto proved unremunerative in actual working. As there are besides extensive deposits of phosphate of this class lying unworked in the West Indies, it seems a pity that some practical use should not be made of the mineral.

Now, as the “Redonda” is a tribasic phosphate, exceedingly soluble in acids and of very high percentage (containing phosphoric acid equal to over 80 per cent of phosphate of lime) I cannot help but think that, even without any other treatment than fine grinding it should be as readily available in the soil as precipitated phosphate. This view would seem to be supported by the well-established fact that all soluble phosphates become converted into phosphate of iron in the soil before assimilation by the rootlets of the plant. A further confirmation of the idea is, I think, afforded by the fact that in a certain district manure made from the mineral and containing a large quantity of phosphate of iron, produced such crops that there was the keenest competition for it amongst the farmers the following season. Albeit, because the iron it contained prevented it “analysing” well, they would not pay for it on the basis of soluble phosphate.

Is there any scientific agriculturist or enterprising agricultural society prepared to test the point by careful practical trials after the well-known Rothamsted fashion? At least one if not all the various large holders of the mineral would, I believe, be willing to place a liberal quantity of it at the disposal of any high class investigator for the purpose.

If the result confirmed the view that this phosphate is surely (though, of course, more slowly than soluble phosphate) assimilable by the plant without any other process than that of fine grinding, a new and cheap source of most important fertilising element would be at the service of the farmer.—I am, &c.,

UNPREJUDICED.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 1, January 5, 1880.

On Motion produced by the Diffusion of Gases and Liquids.—H. Sainte-Claire Deville.—The author refers to the fact that if a tube of platinum is filled with

hydrogen, and kept at a constant temperature above 1000°, whilst its outer surface is kept surrounded by a current of nitrogen, the hydrogen leaves the interior of the tube, producing there a vacuum within a few millimetres. The author explains this result and certain analogous phenomena with the aid of an apparatus figured and described in the original. He remarks that the gases are all diffusible in each other, and asks if this diffusion is anything but simple solution? The transition from liquids to solids being effected by a continuous series of intermediate states, what are the properties of gases and liquids which belong at the same time to solids?

Hydride of Copper: a Reply to M. Berthelot.—A. Wurtz.—The author maintains at some length that the phosphorus and oxygen found in this substance are merely accidental impurities.

Formation-Heat of Chloral Hydrate: a Reply to M. Berthelot.—A. Wurtz.—The author endeavours to uphold the accuracy of his experiments, which M. Berthelot had called in question.

An Application of the Pre-existence of Ampère's Currents in Soft Iron.—M. Trève.—In a former communication the author has shown that if currents of equal intensity are caused to circulate in coils of copper and of iron, we obtain in the latter a polarity which—according to the quality of the iron employed—may be four times as great as in the diamagnetic coils. The author has subsequently applied this remarkable property to apparatus for demonstrating the action of currents and the directing action of the earth upon solenoids.

On New Luminous Tubes.—M. Trève.—The author introduced a Fizeau condenser into a large Geissler tube. The two poles of the induced current of a Ruhmkorff coil were placed in contact with this condenser by means of the ordinary electrodes of such tubes. When the induced current is passed into the condenser, the tube still containing air at the atmospheric pressure, the ordinary snorting sound is heard, which gradually becomes fainter as the vacuum is made. When the pressure is reduced to 0·004 m. nothing is heard, but there appears a white and brilliant light, absolutely distinct from the pale, vague, phosphorescent light of Geissler tubes.

Action of Acetic Anhydride upon certain Phenol Aldehyds.—P. Barbier.—This paper does not admit of useful abstraction.

A New Synthesis of Saligenin.—W. H. Greene.—Saligenin, being oxybenzylic alcohol, is formed by the reaction of methylen chloride upon sodic phenate in presence of sodium hydrate. The yield is somewhat larger if the soda is dissolved in alcohol.

Preparation of the Iodine and Bromine Derivatives of Benzin.—W. H. Greene.—Chloride of iodine reacts upon benzin in presence of aluminium chloride, disengaging hydrochloric acid and forming iodised derivatives of benzin. The best method of operating is to let fall the chloride of iodine, drop by drop, into benzin containing a little aluminium chloride. The product is washed with potassa, and the phenyl iodide isolated by fractionated distillation. Higher iodised benzines are obtained in the same manner, and if the mono-compound is required a large excess of benzin must be employed. The bromine derivatives may be easily obtained by heating a mixture of bromine and benzin in presence of aluminium chloride.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 2, January 8, 1880.

“Electricity Tommasi.”—Under this title there is an essay of nine pages in favour of the battery of Dr. Tommasi and its application to the production of the electric light. The paper professes to be a reply to an article by M. Delahaye.

Application of the Tommasi Battery.—M. Bourrières.—The author proposes to arrange a number of

Rhumkorff coils for quantity as in Nollet's magneto-electric machine, substituting for the motive power the Tommasi battery.

Electricity of Cats.—M. Pallas.—The author on stroking a cat experienced formal electric shock. His right hand rested on the left shoulder of the animal whilst he stroked her back with his left. The shocks were felt in the right hand on ceasing to touch her with the left.

MISCELLANEOUS.

Society of Arts.—The Section of the Society of Arts, formed in 1874, for the discussion of subjects connected with Practical Chemistry and its application to the Arts and Manufactures, has been this year enlarged in its scope that it may include Application of Physics as well as of Chemistry. At the meetings of the present year the following papers will be read:—February 12, "Gas Furnaces and Kilns for Burning Pottery," by Herbert Guthrie, C.E.; March 11, "The Noxious Gases Bill," by E. K. Muspratt, F.C.S.; April 8, "On Recent Improvements in Benzine Colours," by F. J. Friswell, F.C.S.; April 22, "On some Recent Advances in the Science of Photography," by Captain Abney, R.E., F.R.S.; May 13, "On some Physiological Applications of Light," by Prof. W. G. Adams, F.R.S. The meetings are on Thursday evenings, at 8 o'clock, and the days have been selected so that they do not clash with those on which the meetings of the Chemical Society are held.

NOTES AND QUERIES.

Schaffner's Glass Reaction Wheel.—Can any reader tell English sulphuric acid makers whether Schaffner's small glass reaction wheel or Barker's mill (mentioned on p. 385 of Lunge's work "Sulphuric Acid and Alkali"), for distributing the acid over the Glover and Gay-Lussac towers, can be purchased in this country? The heavy lead ones in common use are very liable to stop revolving, and thus to seriously derange the working of the towers.—ONE.

TO CORRESPONDENTS.

S. Sankey.—Reimann's "Aniline and its Derivatives," published by Longmans, may be seen at the British Museum Library.
H. C. Standage.—Declined with thanks.
Dr. Kemshead.—Full references will be found in the indexes.
G. Valentine.—Not suitable for "Notes and Queries." Should be an advertisement.

MEETINGS FOR THE WEEK.

MONDAY, 2nd.—London Institution, 5.
— Royal Institution, 5. General Monthly Meeting.
— Society of Arts, 8. "The Manufacture of India-rubber and Gutta-percha," by Thomas Bolas, F.C.S.
TUESDAY, 3rd.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
— Civil Engineers, 8.
— Zoological, 8.30.
— Society of Arts, 8. "Social and Commercial Prospects in the Transvaal," by the Rev. G. Blencowe.
WEDNESDAY, 4th.—Society of Arts, 8. Ordinary Meeting.—"Trade and Commerce with Siberia via the Kara Sea," by Henry Seebohm.
— Geological, 8.
— Pharmaceutical, 8.
THURSDAY 5th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
— Royal, 8.30.
— Chemical, 8. "Contributions from the Laboratory of the University of Tokio, Japan:—On Persulphocyanate of Silver," by R. W. Atkinson. "On Methylated Dioxethylen Amines," by H. F. Morley.
— Royal Society Club, 6.30.
FRIDAY, 6th.—Royal Institution, 8. Dr. Huggins, "Photographic Spectra of Stars," 9.
— Geologists' Association, 8.
SATURDAY, 7th.—Royal Institution, 3. Prof. Payson, "Hædæl."

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THE CHEMICAL NEWS.

VOL. XLI. No. 1054.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Continued from page 50.)

TURNING to lanthanum and didymium, we find that only the latter gives, independently of its ordinary oxide, a brown-chocolate coloured peroxide, decomposed by heat, not answering to any determined formula, and not furnishing any salts. For this reason we cannot settle the atomic weights of lanthanum and didymium, except from observations on the salts of the ordinary oxides, whose equivalents are very nearly alike (about 46). Again, we cannot be sure of the correctness of these last numbers, above all in the case of didymium, because we have no means of distinguishing a mixture of lanthanum, yttrium, and perhaps even thallium and erbium, from the salts of didymium. According to their equivalents, which are very close together, lanthanum and didymium can occupy one of three places in the system. First, in group III., series 8, between Ba=137, at Ce=140? In this case the calculated equivalent is—

$$44.5 = \frac{138}{3}$$

which corresponds with the observed figures. It is very easy, however, to admit an error in the determinations, as the purity of the preparations is only guaranteed by repeated crystallisations, which we well know do not always free us from isomorphous impurities.

The place, "Group III., Series 8," corresponds, according to the atomic analogy, with Cs, Ba, and Ce, to an element which has very basic properties, and which forms very difficultly volatile chlorides; lanthanum and didymium have certainly properties of this kind.

The second place, in which these properties of the oxide would put La and Di, is "Group IV., Series 10," before Ta=182, in atomic analogy with Ce=140 (?) and Th=231. The element which would fill this place should have an atomic weight of about 180, an equivalent 45, corresponding to the oxide RO₂, and nearly identical with the equivalent of La and Di (above all of La).

A third place is free "Group V., Series 12," between Th=231 and Ur=240, for an element which gives an oxide, R₂O₅, and which has an atomic weight of about 235, and an equivalent of about 49. It would be difficult for one of these two metals to occupy this place; the equivalent is too high and the oxide should possess feebly basic properties, less pronounced than in ThO₂, and more so than in UrO₃; the oxides of La and Di present, on the contrary, very strongly marked basic properties, and they are soluble in dilute acids, even after calcination. The oxides and the metals which would suit this place ought, like their atomalogues, to have a great density, whilst the densities of the oxides of La and Di are about 6.5. I therefore think that I am authorised in admitting that La and Di should occupy the two first-named places, that is to say that the oxide of one of these metals possesses the composition R₂O₃, and corresponds to the lower oxide of Ce, and that the oxide of the other is of the form RO₂, and corresponds to the higher oxide of cerium.

The following facts bear witness to the difference of composition of the oxides of La and Di; according to information received personally, Marignac has not noticed one single case of isomorphism between the corresponding salts of La and Di. In one solution Watts obtained dif-

ferent crystals of sulphates of La and of Di. Marignac found that the sulphates had different compositions. If we admit the formula RO for the two oxides, sulphate of didymium is 3DiSO₄.8H₂O, and sulphate of lanthanum is LaSO₄.3H₂O. We have already noticed that the formula R₂O₃ corresponds better to oxide of didymium than it does to oxide of lanthanum, because it changes the unusual formula, 3DiSO₄.8H₂O, of sulphate of didymium into Di₂(SO₄)₃. Further, if we represent oxide of lanthanum by LaO₂, the composition of sulphate of lanthanum will be La(SO₄)₂.6H₂O. If we consider that we often meet in nature with oxides of different compositions together, such as Nb₂O₃ and TiO₂ (Marignac, Hermann), WO₃ and Nb₂O₅ (Wöhler), V₂O₅ and CrO₃, then the simultaneous appearance of Ce₂O₃, Di₂O₃, and LaO₂, should not surprise us, as it is by no means an isolated fact; it might be compared to the concomitance of ZnO, CdO, and In₂O₃.

For these reasons we can admit that the atomic weight of Di=138, that the metal belongs to the third group, and that it forms an oxide, Di₂O₃; and, on the other hand, that lanthanum belongs to the fourth group, and possesses the atomic weight 180*, the formula of the oxide being LaO₂. However, we cannot be certain of the exactitude of these hypotheses except after new researches in this direction. Up to the present we only know a small number of facts, and we have even interpreted these facts, admitting the formula of the oxides to be RO. The study of the compounds of these metals would present a new interest, because of the modifications that would be introduced in the formulæ of the oxides.

To show how these modifications that I propose to introduce will appear, I extract from Marignac's work the formulæ of several salts of didymium, and I put opposite them the formulæ modified by the new atomic weight; we shall see that these latter are more reasonable.

	According to the Ordinary Atomic Weight.	According to the New Atomic Weight.
The Oxide ..	DiO;	Di ₂ O ₃ .
„ Hydrate ..	Di(OH) ₂ ;	Di(OH) ₃ .
„ Oxysulphide	DiS ₂ .2DiO;	Di ₂ O ₂ S.
„ Chloride ..	DiCl ₂ .4H ₂ O;	DiCl ₃ .6H ₂ O (Di ₂ Cl ₆).
„ Oxychloride	DiCl ₂ .2DiO ₃ .3H ₂ O;	Di ₂ Cl ₂ O ₂ .3H ₂ O.
„ Phosphate	Di ₃ (PO ₄) ₂ .2H ₂ O;	DiPO ₄ .H ₂ O.
„ Sulphate	3DiSO ₄ .8H ₂ O;	Di ₂ (SO ₄) ₃ .8H ₂ O.
„ Sulphate	DiSO ₄ .2H ₂ O;	Di ₃ (SO ₄) ₃ .6H ₂ O.
„ Basic salt	DiSO ₄ .2DiO;	Di ₂ O ₂ (SO ₄).
Double salts {	3(DiSO ₄)Am ₂ SO ₄ .8H ₂ O;	DiAm(SO ₄) ₂ .4H ₂ O.
	3(DiSO ₄)Na ₂ SO ₄ ;	DiNa(SO ₄) ₂ .
	3(DiSO ₄)K ₂ SO ₄ .2H ₂ O;	DiK(SO ₄) ₂ .H ₂ O.

The last three salts are of the same composition as alum, except the small quantity of water they contain. Oxide of didymium, as well as alumina, has the property of being precipitated, although slowly, from its solutions by means of carbonate of barium. The analogy between Di₂O₃ (modified formula) and Al₂O₃ was shown in the last vertical column. If the basic properties of Di₂O₃ are more strongly marked than those of Al₂O₃, we can explain it according to the periodic law by this circumstance, that didymium belongs to one of the even and higher series, whilst aluminium belongs to an odd and lower series.

The composition of the salts of LaO₂ is, according to all that we know, more simple than the composition of the salts of didymium, and it corresponds to the proposed modification of the atomic weight; to LaCl₄, for example (or, perhaps, La₂Cl₈, as it is not volatile), corresponds an oxychloride, La₂O₃Cl₂. If the formulæ which we give to CeO₂ and LaO₂ are exact, then the salts which correspond to them will turn out to be isomorphous, and will also be

* It is now possible to obtain lanthanum free from didymium if we guide ourselves by the absorption spectra of the didymium salts, which spectrum was first observed by Gladstone. The numbers found by Zschiesche (45.09), Rammelsberg (44.38), and Erk (45.1) correspond very well to La, because, according to them, La ought to equal 180.36, 177.52, or 180.4.

analogous to the salts of Th and Zr. The study of the double salts of ZrO_2 , CeO_2 , LaO_2 , and ThO_2 would offer a particular interest, as these oxides often form well-crystallised double salts. If the proposed formulæ are correct we might venture to predict the formation of salts as characteristic as the alums. I have already commenced several experiments in this direction.

(To be continued.)

AN EXAMINATION OF SOME COUNTY DUBLIN WATERS.

By J. FLETCHER, F.C.S.,
Membre de la Société Chimique de Paris, Fellow der Deutschen
Chemischen Gesellschaft.

THE following examinations of waters from certain districts in the vicinity of Dublin, made during an autumn vacation, may possess interest for those who make potable waters their study, comprising as they do samples from limestone, granitic, clay, and sea-shore gathering grounds.

For the purpose of comparison I have followed Dr. Tidy's plan of leaving the sample in contact with standard solution of potassium permanganate for three hours, and noting the oxygen consumed: this will enable the reader to compare the results with an average of Tidy's monthly reports of London waters. In following this method I do not adopt it as the best, but for the purpose of comparison only. I believe that a really reliable mode of estimating the organic contamination of drinking water has yet to be discovered: the translation of Tiemann and Preusse's article which is contained in your last issue is a valuable contribution to the literature of the subject, as is also their article upon analysis of gases contained in water, which appears in the same number of the *Berichte der Deutschen Chemischen Gesellschaft*.

Vartry Water.—My experiments with this water having already been given in the *CHEMICAL NEWS* (vol. xl., p. 171) I will not further occupy your space with them.

Rathmines Township.—The City of Dublin, until the Vartry system was established, drew its supplies from the canals. The large and populous township of Rathmines is still supplied from the same source, as are all the great breweries and distilleries: a process of filtration of an imperfect character is gone through, which has only the effect of reducing in a small degree the amount of organic matter, but does not remove it effectually, merely retaining a portion of the matter held in suspension. The mean of several samples taken from different points on the open canal, compared with the average of those from a public fountain in the Rathmines Commissioners Yard, show the results of this partial treatment:—

	Unfiltered.	Filtered.
Total solids.. ..	42'000	26'000
Chlorine	1'280	1'278
Hardness before boiling ..	12°	12°
Hardness after boiling ..	8°	9°
Oxygen consumed	0'120	0'100
In grains per gallon and measures soap solution.		

Howth is a very important fishing station,—a favourite port of the Manx and Cornish herring fleets,—and is also a fashionable summer resort of the Dublin mercantile classes: it is a picturesque peninsular hill of limestone, forming the northern boundary of the bay, and about 3 miles from the city. During the season the harbour contains fully a thousand boats, with crews of five men each; the fish-curer's factories and workmen make altogether a season's population of about 6000, in addition to the regular inhabitants. The water-supply to the harbour is therefore an important matter, and its source is certainly peculiar and suggestive. Overhanging the harbour is a

ruined abbey, and burial-ground of the monks, the local gentry, and townspeople, still used: from out the wall of the abbey grounds, and several feet below the level of the graves, two pipes project from which flow streams, one of which supplies a public fountain, and is then led through pipes along the pier for the use of the boats. I have seen quite a number of water-casks being filled directly from the pipes. I have made a great many enquiries, and the conclusion I have come to is that the two streams are the drainage waters of the hill upon which the abbey and part of the town are built, supplemented perhaps by other surface waters, but that they contain the soluble constituents of a long succession of buried townfolk; both consume a large quantity of oxygen, and in one the proportion of chlorine is considerable. I have added the examination of two other sources of supply to other parts of the place not used by fishermen,—the "Forge Well," being on very high ground and used by poor cottiers, and the well at Lord Howth's baths; the latter, being within 10 feet of high-water mark, should show considerable chlorine contamination, but it is far excelled by the churchyard water.

	Churchyard No 1.	Churchyard No. 2.	Forge Well.	Bath Well.
Total solids.. ..	32'000	51'000	70'000	72'000
Chlorine	3'124	6'993	2'378	3'017
Oxygen used	0'220	0'130	0'100	0'100
Hardness before boiling	20°	22°	17°	17°
" after boiling	16°	17°	16°	16°
In grains per gallon and measures soap solution.				

Malahide is a pretty little town on the shores of the Irish Channel: it is built at the edge of a limestone formation, on sandy knolls, and is surrounded by dunes which put the visitor in mind of Schweningen and the Dutch coast generally. It is a summer resort, and the people in a great measure live by letting furnished lodgings. It boasts of a splendid hotel, built and owned by Lord Talbot de Malahide. There is no public water-supply, the inhabitants depending upon the collection of rain-water and pumps. The hotel pump yields a water of the most sparkling clearness, and the coolest and freshest flavour imaginable; but it will be seen that the solids and chlorine are exceedingly high: the oxygen used, however, is small in amount, differing in this respect from a public pump situated upon the strand, which not only shows a large solid residue and chlorine, but a very large consumption of oxygen. The poorer classes, however, are supplied from a well in what is called the Old Main Street, which is better than either of the others, but still bad enough. There should certainly be something done to improve the supply to this place. The following are the data:—

	Hotel.	Strand Pump.	Street Well.
Total solids	67'000	90'000	40'000
Chlorine	9'445	6'465	3'550
Oxygen	0'010	0'180	0'060
Hardness before boiling ..	14°	18°	19°
" after boiling	9°	18°	12°
In grains per gallon and measures soap solution.			

Dalkey is a bold and rugged granite mountain forming the southern boundary of Dublin Bay, and opposite to the limestone headland of Howth. It contains a town of some 3000 inhabitants, and is thickly studded with handsome and expensively built villas belonging to Dublin merchants: it is abundantly supplied from the Vartry Reservoir, but until late years was dependent upon wells. I examined two only, and the results were disappointing. I expected different and better results, as the geological formation is entirely granitic. It may be explained by one of the wells being under a pump in the town, surrounded by garden patches and dairy yards; the other, called Our Lady's Well, is situated in a cave within a few yards of the sea, amidst rocks, in the grounds of the

Catholic Convent, and has a local reputation for curative properties:—

	Total Solids.	Chlorine.	Oxygen.	Hardness before Boiling.	Hardness after Boiling.
Tobermore Well	50	2'520	0	25°	14°
Lady Well	49	3'727	0	19	19

It will be seen that all the samples examined show very great hardness, a large proportion of total solids, and chlorine. For convenience of comparison I tabulate them:—

	Total Solids.	Chlorine.	Hardness before Boiling.	Hardness after Boiling.	Oxygen consumed.
Average of eight London Co.'s	22	1'257	14'56	4'36	0'079
Unfiltered Canal Water	42	1'280	12	8	0'120
Filtered ditto, Rathmines	26	1'278	12	9	0'100
Howth, Churchyard No. 1	32	3'124	20	16	0'220
Ditto, No. 2	51	6'993	22	17	0'130
Forge Well, H'th Well at Lord	70	2'378	17	16	0'100
Howth's Baths	72	3'017	17	16	0'100
Malahide Hotel	67	9'445	14	9	0'010
Do. Strand Pump	90	6'465	18	18	0'180
Do. Street Well	40	3'550	19	12	0'060
Dalkey, Tobermore Well	50	2'520	25	14	None
Ditto, The Lady Well	49	3'727	19	19	None

NOTES ON THE ALKALOIDS.

By T. TATTERSALL.

1. *Delphinine*.—In addition to the ordinary tests for this alkaloid, the following is exceedingly trustworthy and characteristic:—Take a small quantity, and triturate intimately in a porcelain basin with 1 to 2 times its bulk of malic acid. Now mix with half a dozen drops of concentrated sulphuric acid, and once more stir round with a small agate pestle, to render the liquid perfectly homogeneous. The orange colour first obtained becomes rose-red: this acquires intensity on standing, and after several hours has attained its maximum shade—a beautiful dark rose-colour, with a very slight violet halo round the edge. A gradual transition to a bluish violet takes place: this persists some time, but finally becomes a dirty cobalt. The colour obtained in the first instance is not produced by these reagents on the other alkaloids, and is quite different from that produced by sulphuric acid alone, which is a light Indian red. The change from rose to violet is also peculiar, only care must be taken to add a sufficiency of sulphuric acid, otherwise this may not be observed. The experiment also fails if the alkaloid be first dissolved in the acid, and the malic acid afterwards added; nor should the liquid be heated, or carbonisation of the organic acid will ensue. Citric, tartaric, benzoic, succinic, and tannic acids do not manifest this reaction.

2. *Morphine*.—If mixed with concentrated H_2SO_4 and a crystal of Na_3AsO_4 , gives first a dirty violet colour, becoming on heating dark sea-green, when acid vapours escape; a rather fugitive dark grey colour appears.

Papaverine and *Codeine* also give characteristic colours with these reagents (See CHEMICAL NEWS. vol. xl., p. 126).

Hayfield Printing Co., Hayfield.

Thomson's Galvanometer.—A. Gaiffe.—The author has introduced an improvement in the method of suspension, making use of two cocoon threads very near to each other. He thus reduces the error to less than 1-100th of the value measured.—*Comptes Rendus*.

ON THE TANNIC ACID OF SUMACH LEAVES.

By H. MACAGNO,
Director of Agricultural Stations.

DURING 1879 I undertook some researches on the production of tannic acid in the leaves of sumach (cultivated in Sicily) by the Flek and Simpkin improved process. In decoctions of sumach tannin may be separated by precipitation with an ammoniacal solution of copper. Take 10 c.c. of a decoction of sumach, acidify with sulphuric acid, add some water, and titrate with permanganate of potash. From 100 c.c. of the same tannin extract precipitate the tannic acid with ammoniacal acetate of copper, taking care that there is an excess of ammonia, and filter. The first few drops of filtrate are rejected, and then 10 c.c. of the filtrate are to be acidified with sulphuric acid, diluted, and titrated with permanganate. The number of cubic centimetres of permanganate destroyed by the solution, from which the tannin has been separated, subtracted from that required before removal of the tannin, gives the quantity of permanganate which has been required to oxidise the tannic acid. The analyses were made separately on the leaves of the lower and superior sides of branches, and the results per cent are as follows:—

		Water in Leaves.		Tannin in Leaves.		Mean Results.	
		Superior Side.	Lower Side.	Superior Side.	Lower Side.	Water.	Tannin.
June 10, 1879	..	58'15	60'23	24'93	17'45	59'19	21'19
" 16	"	57'12	63'40	24'92	16'11	60'30	20'51
" 27	"	52'47	63'44	25'82	15'27	57'95	20'54
July 14	"	51'15	62'24	24'75	10'81	56'69	17'78
" 29	"	49'80	60'33	23'80	9'44	55'06	16'62
Aug. 11	"	48'15	61'80	21'91	8'77	54'97	15'34

An unknown and interesting fact shown by these results is the greater quantity of tannin in the leaves of the superior side of branches, and also the total decrease of this element with the growth of sumach. Notwithstanding, for commercial purposes, the cultivator will be always led to retard the crop, as the extra quantity of leaves compensates for the want of tannic acid.

Palermo, Italy, January, 1880.

CUPRIC TEST-PELLETS FOR SUGAR.

At a meeting of the Clinical Society, on the 23rd ult., Dr. Pavy said that he desired to introduce to the notice of the members a new form of test for sugar, which he thought would prove of no inconsiderable service to the medical practitioners. Of the various reagents that have been recommended for the detection of sugar, his experience led him to consider the cupric test by far the most reliable, and this view stood in accord with that which was generally entertained by analytical chemists. It had been hitherto the practice, certainly where delicacy and precision were in question, to employ the test in the form of a solution prepared by mixing an alkaline tartrate with sulphate of copper and either potash or soda. Such a solution contains the oxide of copper in a state ready to be reduced to the condition of the suboxide when in contact at a boiling temperature with glucose. When freshly prepared this solution fulfils all that can be desired, but there is the disadvantage belonging to it that after being kept for some time, especially if exposed to light and air, it is liable, without the presence of sugar, to throw down a certain amount of reduced oxide on boiling, and thus possibly to mislead unless precautions are taken to provide against it. Besides this objection, the liquid, unless frequently used, is very apt to cause the stopper of the bottle in which it is kept to become fixed. In fact it must be spoken of as an inconvenient liquid to keep for only occasional use.

Dr. Pavy had long felt that it would be very desirable if the ingredients of the test could be incorporated and kept in a solid form; and some years ago a fruitless attempt was made to attain this object. Latterly he had given his attention afresh to this subject, and the test-pellets before the Society were the result of the task undertaken. He need not enter into details regarding the ideas which failed on application to yield a satisfactory result. It would suffice for him to say that the test-pellets contained the solid ingredients of the cupric test solution in a dry state, and that they were brought into the condition of a coherent mass by compression. The preparation had been placed in the hands of Mr. Cooper, of 26, Oxford Street, who, it was but justice to say, most successfully met the difficulties that were unexpectedly encountered. An article had been produced which met with Dr. Pavy's entire satisfaction, and which he felt was adapted hereafter to render good service in relation to diabetes. There appeared no reason why the pellets should not keep for an indefinite time, without undergoing change, if preserved in a closed bottle away from moisture. In using them all that was necessary was to place one in a test-tube with about 3 c.c.—or rather under a drachm—of water, and to apply heat until complete solution had occurred, with the accomplishment of which a clear deep blue liquid would be formed, which actually constituted the cupric test solution. At present it was only as a qualitative test that the pellets were introduced, but there was no reason that hereafter the preparations should not be conducted with sufficient attention to accuracy of weighing to render them applicable for quantitative analysis. Dr. Pavy further mentioned that the test-pellets exhibited were ready to be sold by Mr. Cooper, in bottles containing twenty-four, at a cost of one shilling.

ON THE DETERIORATION OF LIBRARY BINDINGS.*

By Prof. WM. RIPLEY NICHOLS,
Of the Massachusetts Institute of Technology.

SOME time since a librarian of my acquaintance brought to me the backs of a number of books, the leather of which had, in some instances, deteriorated to such an extent as to readily crumble to a brown powder when scratched lightly, as with a finger nail. My opinion was asked as to the part that coal-gas or the products of its combustion played in the destruction of the leather.

I confess that I had supposed the matter settled long since, having in memory a discussion in the English journals of some ten years ago. When I found, however, that a chemist as eminent as Dr. Wolcott Gibbs doubted the influence of coal-gas in the matter, I felt that there was room for further investigation. Although I have not done as yet all that I should like to do, or indeed all that I expect to do in the matter, I desire to state the results which I have obtained in the hope that there may be others here who can throw some light upon the subject.

I may first recall what has been done by others. As long ago as 1854, Dr. Letheby made a report to the City authorities of London, in which he took the ground, which he afterwards on other occasions maintained, that the destruction of bindings in libraries and the destruction of textile fabrics in warehouses, where gas was burned continuously and in large quantities, was due to the products of the combustion of imperfectly purified gas. He noticed that the water produced by burning gas is always acid, and will rot leather, paper, cotton, and linen.

Dr. Odling, in a lecture before the British Association of Gas Managers, June, 1868, maintains that the amount† of sulphur in coal-gas is of no great consequence, and shows

by mathematical calculation that the amount of sulphuric acid formed is extremely small compared with the amount of air through which it is diffused. In spite of this, every chemist knows that sulphuric acid is formed when coal-gas is burned, and that articles of galvanised iron or of zinc, when exposed to the lamp-flame, become corroded with formation of sulphate of zinc. This fact is noted with some quantitative statements by Mr. Charles Heisch, F.C.S., Superintending Gas Examiner to the Corporation of the City of London.* From a burner consuming one-half a foot per hour, the products of combustion of which passed into a zinc chimney, he collected in six weeks three-quarters of a pound of sulphate of zinc.

In the CHEMICAL NEWS for 1877, vol. xxvi., p. 179, Prof. A. H. Church states that he found in decayed leather from the backs of books which had been on the upper shelves of an apartment lighted by gas—

Free sulphuric acid	6.21 per cent
Combined sulphuric acid ..	2.21 ..

He accepts without hesitation the theory that the decay is due to the sulphuric acid formed by the combustion of the gas. In the same volume of the CHEMICAL NEWS there is a paper on the subject by Mr. George E. Davis, who examined the leather of some books which had been in daily use in a large office in Manchester from 1855-1858; after that time till August, 1877, they remained uncovered on a shelf near the ceiling of the same room. The books had been strongly bound in rough calf, and had red basil lettering pieces. Upon knocking the books the leather of the backs came off as a mixture of dust and small pieces, which was very acid to test-paper. The leather from the back was treated with water, and the aqueous solution found to contain:—

	Per cent by weight of leather taken.
Combined sulphuric acid	2.847
Free	1.920

The leather underneath the lettering piece contained:—

Combined sulphuric acid ..	0.39 per cent
Free	0.76 ..

The red basil lettering piece contained:—

Ammonia	1.28 per cent
Combined sulphuric acid ..	0.87 ..
Free	1.04 ..

In 1878 Dr. Gibbs examined books in the Boston Public Library, in the Boston Athenæum, in the Harvard College Library, and in the Astor Library in New York City—in some of which gas is used and in others not. He arrived at the conclusion† “that there was no sufficient evidence” that the products of the combustion of coal-gas caused the trouble, and seemed inclined to consider the fault to lie in the tanning of the leather. He says, “I analysed a number of samples of the leather in my own laboratory and find no free acid whatever.”

My own experience is as follows:—I have had a large number of samples of leather in all stages of decay. I found, as others have done, that *morocco* is but little affected, while Russia and calf are badly acted upon, and ordinary sheep is also attacked. Qualitative and quantitative examinations showed that, in a general way, the more the leather was decayed the more marked was the acid taste and acid reaction on test-papers, and the larger was the amount of sulphuric acid to be found in the aqueous extract. Further, I found that the aqueous extract always contained ammonia, and although the solution had an acid reaction and required a certain quantity of alkali to neutralise it, in no instance, I think, was the acid in greater quantity than that which would be required for the acid sulphate of ammonium. I examined a number of samples of fresh leather; the aqueous extracts were only slightly acid, not sufficiently so to affect the taste, and

* Read at the Saratoga meeting of the American Association for the Advancement of Science, August, 1879.

† CHEMICAL NEWS, vol. xxiii. (1868), p. 65.

* London Journal of Gas Lighting, 1874, p. 856.

† In a letter to W. W. Greenough, Esq., dated Aug. 5, 1878, and Published in the Lib. Jour., vol. 3, p. 229.

contained only a minute amount of sulphuric acid in combination.

I will now give some of the results of quantitative analyses which have been made in my laboratory. The method employed was to soak the leather with successive portions of water until chloride of barium ceased to produce a precipitate, using at first, at any rate, a temperature less than that at which the leather balls together. A portion of the extract was acidulated with chlorhydric acid, and the sulphuric acid precipitated as sulphate of barium. Another portion was distilled with carbonate of sodium and the ammonia determined by the Nessler reagent. The results were calculated into percentages of the original leather. Under this treatment samples of new leather of good quality gave the following results:—

	Sulphuric acid (SO ₃).	Ammonia (NH ₃).
Uncoloured Russia ..	0.25 per cent	0.14 per cent
Coloured Russia ..	0.42 "	0.21 "
Sheep, oak-tanned ..	0.21 "	0.08 "

A sample of *well-worn* but not *decayed* sheep was taken from the side of a family bible, printed in 1814, and presumably in the original binding. The book had never been exposed to gas. The leather was found to contain:—

Total sulphuric acid 1.42 per cent.

A sample of very rotten Russia, which was very strongly acid to taste and to test-paper, contained:—

Total sulphuric acid 8.4 per cent.

Another lot, scraped from a number of books, contained:

Total sulphuric acid 10.6 per cent
Ammonia 3.1 "

In this case the "acidity" of the extract was determined. The greater part of the acidity was due to the presence of sulphuric acid, either "free" or as an "acid salt." Reckoned as sulphuric acid it amounted to 4.2 per cent.

Another sample contained:—

Total sulphuric acid .. 6.4 per cent
Free or an "acid salt" .. 2.4 "
Ammonia 2.4 "
Lime 0.1 "
Alumina No more than a trace.

These determinations indicated to me that the sulphuric acid was in considerable measure present as sulphate or acid sulphate of ammonium. I then performed the following experiment:—

A quantity—about 20 grms.—of the rotten leather was carefully extracted with water, and after dialysing the extract several times and allowing the dialysed liquid to crystallise, I obtained about a grm. of white crystals which were but slightly acid to test-liquids, and were found to contain:—

Sulphuric acid (SO₃) 56.43 per cent
Ammonia 23.20 "
Non-volatile matter 10.23 "

The non-volatile matter contained something insoluble in chlorhydric acid, also some lime and an amount of sulphuric acid equivalent to 4.01 per cent of the original crystals. Leaving out this sulphuric acid which remained in the "ash," the composition of the portion driven off by heat would be:—

Sulphuric acid (SO₃) 58.39 per cent
Ammonia 25.84 "
Water and loss 15.77 "

100.00 "

The theory for the normal sulphate of ammonium is:—

Sulphuric acid (SO₃) 60.60 per cent
Ammonia 25.76 "
Water 13.64 "

100.00 "

It would seem, therefore, that in this case the crystalline salt obtained was mainly the normal sulphate of ammonium. In other cases, however, the impure crystals obtained evidently contained some of the acid salt.

In view of these facts, it would certainly seem that bindings of Russia, calf, or sheep, when exposed to the products of the combustion of illuminating gas, do absorb sulphuric acid. It is difficult, otherwise, to account for the large amount, 8 and 10 per cent, which is found in the rotten leather. A small amount might come from sulphate of lime in the leather, from sulphate of iron used in staining the backs, and from other sources, but the amount in the samples of new leather which I have examined is very small, and the largest amount that I have ever found, except where I knew the leather had been exposed to gas, was in the sides of a dilapidated copy of Athanasius Kircher's *Magneticum Naturæ Regnum*. The history of the binding was unknown as well as its age; it was very "greasy," and from it water took out 4.9 per cent sulphuric acid. It was somewhat acid to taste, but it is not unlikely that the book had, in the course of its long history, been exposed to sulphurous acid from the combustion of soft coal, or indeed to gas itself. The same leather contained 2.2 per cent of ammonia, but this could easily be accounted for. As in other cases, more or less ammonia may come from the air, from the decay of the paste, from the leather itself, and some, no doubt, from the gas in certain cases.

With reference to the source of the sulphuric acid, it has been objected that the sulphur compounds burn mainly to sulphurous acid and not to sulphuric. I analysed some of the deposit which formed on a galvanised iron plate which received the water condensed on the under surface of a "water-bath" when the gas was first lighted. I found the salt present to be a sulphate, and could obtain no evidence of a sulphite.

As it has been suggested that the alum in the paste might explain the sulphuric acid, I took some of the scraped backs and examined a portion containing the paste and paper of the back as well as some leather. The results were:—

Total sulphuric acid 3.55 per cent
Alumina (with trace of iron) only 0.42 "

It has further been suggested that it is the fault of the leather. I cannot prove that this is not so, and as a part of the investigation I desire to examine some decayed backs which have never been exposed to gas. This I have not been able to do. Although I have been promised such backs I have never received them. I may say in this connection that some of the books which I have examined were bound by a man now engaged in the same library, and he claims to know that the leather was good when put on.

I think the evidence collected throws such suspicion on the gas burned that one would be justified in insisting upon better ventilation, and in recommending that the burners should be arranged, when practicable, so that the products of combustion should be drawn by a ventilating chimney or pipe away from each burner without mixing with the air of the room. In one of the London libraries, referred to by Dr. Letheby, better ventilation was determined upon, and, as I understand it, with gratifying results.

To settle finally the vexed question, I have laid out the following plan:—I propose to have a set of books bound at the same time by the same person, using the same leather and paste. These books are, some of them, to be put in the most exposed situation, one of them to be carefully examined now, one after a year's interval, another after two years, and so on. Meanwhile two of the same set are to be put where they will not be exposed to gas, but where they will quietly grow old. I believe this, coupled with an examination of the air, which I hope to make, will settle the question.

It should be said that the books which are most decayed have been upon the upper shelves of the library, where

they are subjected to a high temperature, and it is of course possible that the disintegration of the leather has given an opportunity for the absorption of the vapour of sulphuric acid and ammonia rather than that the absorption is the cause of the disintegration.

[The writer of the above paper would be pleased to receive for further research any samples of leather binding whose history can be well ascertained.]

ACTION OF BONE-BLACK ON SOLUTIONS OF PURE SUGAR.

By P. CASAMAJOR.

In a paper published in the *American Chemist*, for November, 1871, "On the Purification of Sugar Solutions for the Optical Saccharometer," I gave an account of experiments made with dry bone-black on sugar solutions. These experiments led me to differ from the conclusions to which Dr. Schiebler had arrived, that bone-black absorbs sugar from its solutions, and that, therefore, the use of bone-black, even when thoroughly dried, tends to give results with the saccharometer that are lower than they would be without the use of bone-black.

My attention was called to this subject again during the latter part of 1878 by several communications of our regretted fellow member, Professor J. M. Merrick, of Boston, to the *CHEMICAL NEWS*. In these communications Professor Merrick cites many authorities who agree in this: that the use of bone-black, in tests by the optical saccharometer, tends to give results that are too low. In addition to citing these authorities, he gave experiments, made by himself, which confirm the opinion of the authorities cited by him. He found in several cases that when using bone-black the results were from 0.5 to 0.8 per cent lower than they were previous to using bone-black. Prof. Merrick wrote to me at the time, calling my attention to these results, and I answered that, being very busy with other things just then, I had not time to go over the subject.

Since that time, during February of this year, I made some experiments to discover whether bone-black really absorbed pure sugar from its solutions. In these experiments I made use in one case of a solution of pure sugar, and in another case of a solution of good loaf-sugar. The pure sugar was obtained by soaking good loaf-sugar in alcohol for over twenty-four hours, washing the crystal with more alcohol, and finally drying the sugar over a water-bath. The object of using pure sugar, or nearly pure, was to eliminate errors which might result from the unequal absorption of bone-black of the impurities that exist in commercial sugars. These impurities are of many kinds, but it generally happens, with sugar of high grade, that the impurities are in such a condition that they exert no action on polarised light, so that, on making the correction after inversion, the corrected result is almost always the same as the direct test, when we operate with sugars of 90 per cent or over.

There being a possibility that the effect of bone-black in lowering the saccharometric degree was due principally to its power of removing dextro-rotate impurities in greater quantity than the lævo-rotate, the use of pure sugar would eliminate any error from this cause.

The experiments were conducted in such a way as to leave no doubt on the question whether bone-black took up sugar from its solutions. A weight of bone-black was used which was many times greater than the weight of sugar that filtered over it.

First Experiment.—This was made with 125 c.c. of recently and thoroughly dried new bone-black. These 125 c.c. weighed 79 grms. The bone-black was not washed to remove the salts.

The solution of pure sugar that was filtered over these 79 grms. of bone-black had a sp. gr. of $1.052 = 12.75^\circ$

Balling. In the optical saccharometer it stood at 51.5 per cent. These numbers give a coefficient of purity = 100. A volume equal to 80 c.c. was allowed to run out, the operation taking one hour. This portion that came through had a specific gravity of $1.057 = 13.05^\circ$ Balling. In the optical saccharometer it stood at 49 per cent. These numbers give a coefficient of purity equal to 92.8 per cent.

This fall in the coefficient of purity from 100 to 92.8 cannot be due to absorption of sugar, although the fall in the saccharometric indication from 51.5 to 49 per cent shows that sugar has been removed from the solution. The decrease in the coefficient of purity can only come from the impurities present in new bone-black, and points to the necessity of washing new bone-black before passing sugar solutions over it.

As the quantity of bone-black was very large in proportion to the weight of sugar that filtered over it, the quantity of sugar removed from the solution may be taken as being very nearly the quantity which 79 grms. of new bone-black can absorb. To ascertain this quantity of sugar let us take 80 c.c. of the solution, such as it went on the bone-black, its specific gravity being 1.052 ; the weight of the solution was $80 \times 1.052 = 84.16$ grms.

The degree Balling being 12.75 , and the coefficient of purity being 100, the whole weight of sugar in the 84.16 grms. of solution was $0.8416 \times 12.75 = 10.73$ grms. of pure sugar.

The 80 c.c. of solution which filtered through the 79 grms. of bone-black in the space of an hour had a specific gravity of 1.057 , which gives for the 80 c.c. of solution a weight of $80 \times 1.057 = 84.56$ grms. The degree Balling of these 84.56 grms. being 13.05 , and the coefficient of purity 92.8, the quantity of pure sugar in the filtered 80 c.c. is $= 0.8456 \times 13.05 \times 0.928 = 10.25$ grms.

Subtracting this from the quantity of sugar previously obtained, we have $10.73 - 10.25 = 0.48$ gm. as the weight of sugar absorbed by 79 grms. of new bone-black, which is equivalent to 0.606 gm. as the quantity of sugar absorbed by 100 grms. of new bone-black.

Second Experiment.—This experiment closely resembles the first. The bone-black used was the ordinary bone-black of the refinery. The quantity taken was 125 c.c., weighing 100 grms. The bone-black was thoroughly dried, as in the first instance.

The solution of loaf-sugar filtered over these 100 grms. of old bone-black had a specific gravity of $1.051 = 11.5^\circ$ Balling. In the optical saccharometer it stood at 46 per cent. These numbers give a coefficient of purity equal to 99.5.

As in the previous case, a volume equal to 80 c.c. was allowed to run through the filter in the course of one hour.

The portion that came through had a specific gravity of $1.044 = 10.95^\circ$ Balling. In the optical saccharometer it stood at 43.3 per cent, which gives a coefficient of purity equal to 98.6.

We may notice here at once that the fall in the coefficient of purity is insignificant when compared to what happened with new bone-black: still it points to impurities absorbed by the old bone-black.

If we go over the same calculations as in the first experiment we find that the weight of the 80 c.c. of solution was $80 \times 1.051 = 84.08$ grms. The total number of grms. of sugar in 80 c.c. = $11.5 \times 0.8408 \times 0.996 = 9.63$ grms.

The 80 c.c., after filtering over 100 grms. of old bone-black, weighed $80 \times 1.044 = 83.52$ grms., and the total weight of pure sugar in the filtered portion was $10.95 \times 0.8352 \times 0.986 = 9.01$ grms.

If we subtract this quantity from 9.63, belonging to the 80 c.c., before going on the bone-black, we have $9.63 - 9.01 = 0.62$ gm. as the weight of sugar absorbed by 100 grms. of old refinery black.

Conclusions.

These numbers, 0.606 gm. for the quantity of pure sugar absorbed by 100 grms. of new black and 0.63 gm. for the

quantity of pure sugar absorbed by 100 grms. of old black, are remarkably close, particularly if we consider the degree of approximation compatible with sugar analysis.

The quantity of sugar absorbed by 100 grms. of bone-black may be put down as 0.6 gm. It is possible that new bone-black may generally absorb slightly more or less sugar than the old, but this point can only be settled by a great number of tests.

To return to the action of bone-black in sugar tests, there seems little doubt that in many cases the addition of bone-black seems to lower the saccharometric test. This action must in part be due to the comparatively greater absorption of the dextro-gyrate impurities that are found in commercial sugar.

As to the absorbing action of bone-black on sugar, if, as the above experiments show, this absorption is about 0.006 of the weight of the black employed, there must be no perceptible error in shaking a small quantity of fine bone-black with the solution previous to filtration. A quantity of black equal to 4.5 grms., shaken with 100 c.c. of sugar solution, will almost always produce a marked improvement in the colour of the solution. If this quantity of bone-black absorbs 0.006 of its weight of sugar, this absorption will be equal to $0.006 \times 4.5 = 0.027$ gm. Now this quantity divided by 26.048 grms. (the quantity dissolved in 100 c.c. of solution) will give $\frac{0.027}{26.048}$, which is very approximately = 0.001, or one-tenth of 1 per cent, and this is the error which $4\frac{1}{2}$ grms. of bone-black may produce from its property of absorbing pure sugar. By absorbing dextro-gyrate impurities a greater error may arise, but I regret to say that I have not tried to solve this problem, which is well worthy the attention of chemists.*
—*Journal of the American Chemical Society*, vol. i.

ON THE ESTIMATION OF THE NITROUS COMPOUNDS IN THE VARIOUS STAGES OF THE MANUFACTURE OF SULPHURIC ACID.†

By JAMES MACTEAR, F.C.S., F.I.C.

(Concluded from p. 54.)

SUMMARY.‡

Found in 100 c.c.	a	0.00209	N ₂ O ₃	and =	1.6278	N ₂ O ₅
"	"	b	0.03664	"	=	4.71067 "
"	"	c	0.01957	"	=	0.47457 "
"	"	d	0.8859	"	=	0.0797 "

The amount of N₂O₅, formed in the above experiment, will show how it is that a single day's carelessness may utterly ruin a vitriol chamber by the destruction of the lead from the action of the nitric acid. The amount of acid run down the Gay-Lussac was not altered during the experiment, which accounts for the low percentage of nitrous compounds, 0.9416 N₂O₃, by ammonia process.

If we calculate this nitrous vitriol to the strength of Dr. Lunge's specimen, viz., 0.413 gm. N₂O₃ per 100 c.c., the N₂O₅ would amount to 0.37 gm. per 100 c.c.; or if the N₂O₃ be called 100, the N₂O₅ is as near as may be 9 per cent.

The method of working chambers has a great deal to do,

* Dr. Scheibler has called attention to the error committed in saccharometric tests by using basic acetate of lead. As the precipitate obtained occupies a certain volume, this volume must be deducted from the 100 c.c. measured by the graduated flask. By using a volume of liquid smaller than 100 c.c. the result obtained is necessarily too high. This being the case, a sufficient quantity of bone-black, shaken up with the solution, may act as a corrective by lowering the saccharometric degree. If there was any constancy in the effect of acetate of lead in raising the saccharometric degree, and of bone-black in lowering the test, it would be possible to neutralise a certain volume of basic acetate by a corresponding weight of bone-black. Unfortunately, if I am not mistaken, experiments are wanting to elucidate this subject.

† A Paper read before the Newcastle-upon-Tyne Chemical Society.

‡ See also table at end of the paper.

no doubt, with the character of the last gases passing into the Gay-Lussac, and it is evident that where there is only a short distance between the chambers and Gay-Lussac column, the condensation of the N₂O₅ formed will be imperfect, and it will be found in the nitrous vitriol. On the other hand, if the distance is considerable (in the above experiments it was about 200 feet), and the drainage to the chambers, the amount found in the nitrous vitriol will be the less, and in any case the amount will be irregular.

It appears to me that while we may say that in any special case we have found such-and-such results as to composition of the gases or of the acid, we can not apply such results as a general principle applicable to all cases; the variations are very great, and require to be studied almost for each short period of time, as well as for each set of plant, and it may well be asked what is the use of our generalising as we are too prone to do on such a subject.

12. The subjoined tests from plant working under normal conditions and giving good results, will be interesting:—

(a.) Glover tower acid, 149° T. = sp. gr. 1.745, having a reddish colour; this disappeared on heating, but returned on cooling again. It disappeared altogether on the addition of a piece of zinc.

It gave in 100 c.c.:

	Grm.
1. By permanganate process	0.04235 N
2. " ammonia	0.02940 N = as N ₂ O ₃ 0.0798

Excess from 1 0.01295 N = SO₂ 0.059*

(b.) Glover tower acid, smelling *strongly* of nitrous compounds; strength of acid, 149° T. = 1.745 sp. gr.

It gave in 100 c.c.:—

1. By permanganate process	0.2018 gm. N
2. " ammonia	0.1969 " N

Excess N from 1 0.0049 " N = SO₂ 0.0224*

(c.) Glover tower acid, smelling *strongly* of sulphurous acid; strength of acid 149° = 1.745 sp. gr.

It gave in 100 c.c.:—

1. By permanganate process	= 0.05712 gm. N
2. " ammonia	= 0.04872 " N

Excess N from 1 = 0.00840 " N SO₂ = 0.0384*

(d.) Gay-Lussac acid, dirty-looking in appearance; strength, 148° T. = 1.740 sp. gr.

It gave in 100 c.c.:—

	Grm.
1. By ammonia process	= 0.34160 N
2. " permanganate	= 0.33425 N

Excess N from 1 = 0.00735 N as N₂O₅ = 0.02835
2.15 per cent of the N being in the state of N₂O₅.

(e.) Last drip of last chamber in No. 8 series, under ordinary conditions of working. The acid very clear; gas given off on standing, which turns red in air.

The chamber was very high in colour at time of sampling; strength of acid, 80° T. = 1.400 sp. gr.

100 c.c. gave:—

	Grm.
1. By ammonia process	= 1.463000 N
2. " permanganate	= 0.004375 N

Excess N from 1 = 1.458625 N = as N₂O₅ 0.56261
99.7 per cent of the N present being in the state of N₂O₅.

(f.) The last drip from the fifth set of chambers, under ordinary conditions of working; strength, 81° T. = 1.405 sp. gr.

* Stated as SO₂ for simplicity; actually it is oxidisable matter present, As₂O₃, in considerable quantity

It gave in 100 c.c. :—

1. By ammonia process 0.3332 grm. N
2. „ permanganate „ 0.00525 „ N

Excess N from 1 0.32795 „ N=as N_2O_5 1.265
98.4 per cent of the N present being in the state of N_2O_5 .

(g.) Acid from a Glover tower became reddish on cooling; strength of acid, $150^\circ T.$ = 1.750 sp. gr.

It gave in 100 c.c. :—

- | | |
|----------------------------|-----------|
| | Grm. |
| 1. By permanganate process | 0.06111 N |
| 2. „ ammonia „ | 0.05152 N |

Excess N from 1 0.00959 N= SO_2 0.0438*

(h.) Average of last drips of six series of chambers, working under ordinary conditions; strength of acid, $73.5^\circ T.$ = 1.3675 sp. gr.

It gave in 100 c.c. :—

1. By ammonia process 1.2838 grm. N
2. „ permanganate „ 0.0063 „ N

Excess N from 1 1.2775 „ N=as N_2O_5 4.9275
99.5 per cent of the N present exists as N_2O_5 .

It would, I think, be clearly absurd to assume from such results that a sample of Glover tower acid which *smells very strongly* of sulphurous acid should not contain a greater proportion than one which smells as strongly of nitrous compounds.

And from results such as that of sample A, as well as its rather stable characteristics and its absorption by ferrous sulphate, I am inclined to believe that there is present in Glover tower acid, under certain conditions as yet undetermined, appreciable quantities of N_2O_2 , and that there is reason to believe that this holds good also with the Gay-Lussac acid or nitrous vitriol.

In fact, if we calculate the effect of the arsenic, under the assumption that in the Glover tower acid, containing sulphurous acid, at least that it exists as As_2O_3 , we will see that, on the test by permanganate, there will not be oxygen enough to oxidise the N compounds fully to even N_2O_3 .

The sulphate of nitrosyl is a well enough known body, and has been examined by many eminent workers, and it would require more proof than mere assertion on the part of modern investigators to convince us that the work of the older and oft-times more patient worker was untrustworthy and incorrect.

[We have had only recently a grand corroboration of the views of one of these older men, in the victory over the gases, of MM. Cailletet and Pictet, whose patient labour and sound scientific reasoning has been crowned with the success it merits. I think, Sir, with all deference, that this is just one of these occasions on which we as a Society ought to express our congratulations to both these gentlemen, on their having (quite independently of each other) so successfully reached their goal.]

The complete investigation of this subject for *any* given set of conditions alone is one which would prove of the most laborious and complicated description, and is quite beyond the province of one to make whose time, like my

own, is fully occupied with the multifarious details of technical work. I trust, however, that the results of the investigation which I have to-night submitted to you will be of use in two directions :—

1st. In giving some indication of the methods of testing the loss of nitrous compounds.

2nd. In opening up a discussion on the subject generally, which may—nay, must—add to our knowledge of the subject, and enable us to obtain more clearly an insight into the reactions and conditions under which we direct our labour in the manufacture of sulphuric acid.

I have to apologise for the great length to which this paper has extended, but I trust you will excuse this in consideration of the great interest the subject possesses.

I am sorry also to have to differ so completely from Dr. Lunge, and to have to criticise so adversely his recent paper in the way I have done: but as his object, like mine, is to get at the truth, I am certain he will take all I have said in good part.

ON THE ARTIFICIAL PRODUCTION OF PRECIOUS STONES.

By ALFRED H. ALLEN, F.C.S.

ON Wednesday evening Mr. Alfred H. Allen, the borough analyst for Sheffield, read a paper at the School of Art, before the members of the Literary and Philosophical Society, upon "Artificial Precious Stones." Dr. Hime presided.

Mr. ALLEN said that synthetical mineralogy, or the artificial production of natural minerals, was a subject which had always caused great interest, and was likely to be of very considerable commercial importance in the future. A study of the chemistry and mode of occurrence of minerals had resulted in attempts, many of which had been successful, to obtain those minerals by artificial means. Of the substances used for ornamental purposes and usually classed as precious stones, a very large number consisted of silica in a state of greater or less purity. As instances of these he mentioned rock-crystal, amethyst, cairngorm, aventurine, agate, cornelian, onyx, jasper, calcedony, chrysoprase, and opal. With the exception of the last of these, the varieties of silica were not of very great value for ornamental purposes, and many of them were capable of close imitation by glass. Microscopic crystals of quartz or silica were obtainable by various methods, and very fair imitations of artificial opal had also been prepared, though such products never rivalled the natural opal in their play of colours. Several silicates used as precious stones had been intentionally prepared or had been noticed among mineralogical products. Garnet, zircon, hyacinth, and topaz were instances of these. The beryl or emerald had also been obtained by synthetical means. The stone known to mineralogists as corundum, and to jewellers as sapphire, oriental ruby, or oriental emerald, according to its colour, consisted chemically of pure crystallised alumina, and had been obtained by various artificial means. Most of the processes, however, were only capable of giving it in very small quantity or very minute crystals, but Messrs. Frey and Feil had recently conducted operations on a very large scale, and their success had completely eclipsed that of all previous

SUMMARY OF TESTINGS.

	Degrees Tw.	Grammes. In 100 c.c.		Weight in Grms. of 100 c.c.	On Total Weight. Per cent.		Per cent of SO_3 in Acid.	SO_3 in 100 c.c. of Acid.	Per 100 of SO_3 .		Per 100 of N. N existing as	
		N_2O_3 .	N_2O_5 .		N_2O_3 .	N_2O_5 .			N_2O_3 .	N_2O_5 .	N_2O_3 .	N_2O_5 .
A.	38	0.0022	1.628	119	0.0018	1.3681	21.22	25.2518	0.0087	6.4471	0.18	99.82
B.	73	0.0366	4.7107	136.5	0.0268	3.4511	37.91	51.7471	0.0707	9.1033	1.09	98.91
C.	57	0.0195	4.7458	128.5	0.0152	3.6932	30.61	39.33385	0.0496	12.0634	0.58	99.42
D.	149	0.8860	0.0770	174.5	0.5077	0.0441	66.12	115.3794	0.7679	0.0667	94.08	5.92
E.	60	0.01188	5.6261	140	0.0085	4.0186	40.81	57.1340	0.0208	9.8472	0.30	99.70
F.	81	0.01425	1.2650	140.5	0.0101	0.9003	41	57.6050	0.0247	2.1960	1.60	98.40
H.	73.5	0.0171	4.9275	137	0.0125	3.5967	38.36	52.5532	0.0325	9.3762	0.50	99.50

investigators. They had very recently operated on 50 or 60 lbs. of material which they kept heated uninterruptedly for 20 days. In this manner they obtained crystals of artificial corundum, either blue, red, or green, as desired, capable of scratching quartz and topaz, and indistinguishable from the natural precious stone. Messrs. Frey and Feil had not patented their processes, and there was every reason to believe that these would be shortly extensively applied. Another natural product often used for ornamental purposes, and now manufactured artificially on an enormous scale, was the *lapis lazuli*, better known in its artificial state as ultramarine. The natural substance was extremely costly. In 1829 the artificial product, equal in every respect to the natural, was worth £12 a lb., but it was now less than 6d. a lb. The total production in 1872 was over eight million kilograms. The most valuable of all precious stones, valued on account of its lustre, rarity, and extreme hardness, was the diamond. The diamond was well known to chemists to consist of pure carbon in a crystallised state, and very many attempts had been made to prepare it artificially. Great obstacles stood in the way of success, the chief being the complete insolubility of carbon in all known liquids, its non-volatility, ignorance of the conditions under which the diamond was produced in nature, its great rarity, and hence the presumed rare existence of the conditions for its formation, its crystalline form, and hence the probable very slow formation. The artificial production of the diamond, although presenting inherent difficulties of the highest order, was not a mere philosopher's day-dream, like the discovery of perpetual motion, transmutation of metals, or the squaring of the circle. The production of adamantane carbon—the same thing as diamond—was so far within the reasonable bounds of possibility that the wonder was that it had not already been definitely effected, and no chemist would be greatly surprised to hear that the problem had been fully solved. Quite recently several new claimants had arisen for the honour of having solved the problem of obtaining artificial diamond. The most notable of these was Mr. James Maclear, managing partner or director of Messrs. Tennant's celebrated chemical works at Glasgow. Mr. Maclear was well known as an ingenious and able practical chemist, and when he announced that he had solved the problem chemists were quite prepared to congratulate him on his success. Mr. Maclear recently went so far as to submit some specimens of what he believed to be diamond to the experts of the British Museum, and much to the surprise of the scientific world it was now definitely admitted, even by Mr. Maclear himself, that these specimens were not real diamonds. How Mr. Maclear could have been deceived originally as to their real nature was a matter of mystery. In the opinion of Mr. Allen, by far the most hopeful direction in which we had to look for the artificial production of diamond was based on the curious fact very recently discovered that gases under high pressure had a very marked power of dissolving solids. This extraordinary discovery had a bearing on the recently reported spontaneous rupture of a diamond. It was assumed, with great probability, that the explosion was due to the pressure of a cavity containing highly compressed gas, which was supposed to have been carbonic acid. If this supposition were proved to be true it would throw enormous light on the natural mode of forming diamond, and might prove the key to its artificial production. It was impossible to omit mention of the investigations of a native of Sheffield, Dr. R. S. Marsden, an old pupil of his, who had recently been experimenting on the production of crystallised elements. He had got a crystalline form of something, which he (Mr. Allen) thought might possibly be diamond. By an entirely different process from anything issued before he had obtained a crystalline body, which was very probable to be crystallised carbon,—in other words, diamond. The economical production of artificial diamond, even of minute size, would be of immense importance in the arts, and diamond wheels would

soon replace emery wheels. The production of diamonds of sufficient size for ornamental use would probably soon follow the other discovery, and for a time at least such stones would command a very high price. The economical production of diamonds of fair size would lead to their extensive and habitual employment for rock-boring, stone-cutting, and all similar purposes. He was so sanguine that the artificial formation of diamond was practicable that he should regard with the greatest interest the formation of any company or similar association which might be established to ascertain the possibility of making such a production.

A cordial vote of thanks was given to Mr. Allen for his able lecture.

CORRESPONDENCE.

TESTING OF VITRIOL CHAMBER EXITS, &c.

To the Editor of the Chemical News.

SIR,—Allow me to state that the method for the examination of chamber exits given by Mr. Vasey in the *CHEMICAL NEWS* (vol. xli., p. 47) was fully described by me in a paper read before the members of the Faraday Club, on February 19th, 1879, having been in use in my laboratory for nearly twelve months previously. It is strange Mr. Vasey has forgotten this, for on September 20th, 1878, he collected for me the gases passing an exit which I examined by this very method, being the results of a 22½ hours' aspiration. These results were given in the paper alluded to, and I am glad to have the pleasure of thanking him for stating the convenience and accuracy of the process. I pointed out, however, to the members present that peroxide of hydrogen was more easily deprived of its oxygen when alkalies were present, and that is why I advised the exit gases to be drawn through diluted peroxide, and the acid solution titrated with normal soda, which, after acidifying and evaporating, was to be transferred to the nitrometer. By this means a minimum of sulphate of soda is introduced into the tube.

Mr. Whewell seems rather in a fog as to the action of chloride of magnesium in causing mildew on cotton goods. Your reviewer was quite correct in stating that it acts by virtue of its property of absorbing moisture, and I fail to see that your correspondent has established any other proposition.

Mr. Menzies's letter (*CHEMICAL NEWS*, vol. xli., p. 22), extolling the virtues of chlorate of potash as against those of bichromate, calls for a few remarks as a guide to your practical readers. Bichromate of potash is a splendid oxidiser, and quite safe even when mixed with strong sulphuric acid; chlorate of potash, on the contrary, explodes violently when heated with strong sulphuric acid—in fact accidents have happened by mixing these substances in large quantities *in the cold*. Again, when oxidising organic compounds with chlorate of potash, chloro-compounds of quite a different nature to those compounds obtained by the use of bichrome and sulphuric acid are often formed, and, last and not least, there is danger in storing and using chlorate, which there is not with bichromate of potash. Commercial chlorate and commercial bichrome are sold in a state of great purity. When chemist to the firm of J. C. Gamble and Sons, of St. Helens, one of the largest producers of chlorate in this country, I usually examined the chlorate once a week for impurity: there is a most minute quantity of CaCl_2 in all, but I could only estimate the amount of impurity by taking such large quantities as 100 grms. The chloride has often been *below* 0.001 per cent, or 1 part in 150,000 actual chlorate.

In the *CHEMICAL NEWS* (vol. xli., p. 11), under the heading of "Notes from Foreign Sources," it appears that the *Transactions of the Society for the Promotion of Industry* contains a paper by Jul. Post on the Composition of Wel-

don Mud, the reproduction of which is strictly prohibited. Can any of your German correspondents give a reason for this, and state how Industry is to be promoted by such measures?—I am, &c.,

GEORGE E. DAVIS.

Heaton Chapel, January 27, 1880.

TESTING OF VITRIOL CHAMBER EXITS.

To the Editor of the Chemical News.

SIR,—I notice that Mr. T. E. Vasey (CHEMICAL NEWS, vol. xli., p. 47) lays claim to the application of the mercury nitrometer to the testing of vitriol exits so long ago as "the beginning of May last year." He should be aware that this test was in use long before this in Mr. Davis's laboratory, and he (Mr. Davis) described it in a communication to the Faraday Club read on January 22nd, 1879. I know that in the fall of 1878 Mr. Davis persuaded several chemists to use it, myself amongst the number, but several of them saw difficulties in the transference of the residue of evaporation to the nitrometer, and preferred their own processes. When I succeeded Mr. Vasey as Manager of these Works I found that the old process of estimating the nitre *by difference* was in use: this is, as a scientific method, so exceedingly unsatisfactory—as all errors are put on to the nitre—that I caused Mr. Davis's process to be used instead. It has been used ever since, and I find it both quick and accurate.—I am, &c.,

W. JESSE LOVETT.

Victoria Chemical Works, Bradford, Manchester.

ALCOHOL TABLES.

To the Editor of the Chemical News.

SIR,—Your correspondent Mr. S. Cohné (CHEMICAL NEWS, vol. xli., p. 57) mentions the fact that several of our standard works contain no tables for the calculation of proof-spirit to the percentage of alcohol by volume, as used on the Continent. Mr. Cohné gives several formulæ for effecting such calculations, but as they are somewhat clumsy I hope to be excused for giving the following methods, which I quote from a recently-published work* in which I take a special interest:—

1. The percentage by volume of absolute alcohol is obtained by multiplying the percentage of proof-spirit by the factor 0.5706.
2. By multiplying the percentage of alcohol by weight by the observed specific gravity, and dividing the product by 0.7938, the percentage of alcohol by volume is obtained.
3. By dividing the percentage of alcohol by volume by 0.0706, the percentage volume of proof-spirit is obtained.
4. The percentage of alcohol by weight, multiplied by the specific gravity, and the product by 2.208, gives the percentage by volume of proof-spirit.

If we express the percentage of alcohol by weight as W, the density by D, the percentage of alcohol by volume as V, and the percentage of proof-spirit by P, the above instructions can be expressed by the following formulæ:—

1. $V = P \times 0.5706.$
2. $V = \frac{WD}{0.7938}.$
3. $P = \frac{V}{0.5706} = V \times 1.7525.$
4. $P = WD \times 2.208.$

I note that Mr. Cohné takes the specific gravity of proof-spirit at 0.9186 instead of 0.9198, and this makes his contained percentage of alcohol by volume 57.27 instead of 57.06; but it is not necessary to enter into the cause of these differences.—I am, &c.,

ALFRED H. ALLEN.

Sheffield, January 31, 1880.

* "Commercial Organic Analysis," vol. i., p. 90. J. and A. Churchill.

THE UNFERMENTED WINE CASE.

To the Editor of the Chemical News.

SIR,—In your report of the unfermented wine case (CHEMICAL NEWS, vol. xli., p. 56) there is a slight error in regard to the amount of ash: it should be 0.019 (not 0.91) in 100 c.c. This will equal 13.3 grains to the gallon; the chemist for the defence made it 0.020 per cent, or 14 grains to the gallon. If there had been even one-sixth part of grape-juice in the so-called wine, the ash would have amounted to nearly 50 grains in the gallon.—I am, &c.,

J. CARTER BELL.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 2, January 12, 1880.

Observations on a Note by M. Wurtz (*Comptes Rendus*, Session of December 22, 1879, lxxxix., p. 1062).—The discussion on the question of chloral hydrate appears in danger of degenerating into an exchange of personalities.

Influence of the Nature of the Charcoal upon the Electric Light.—Th. du Moncel.—The author calls attention to the experiments which he made as far back as 1855, in order to show the advantages resulting from the use of charcoal of vegetable for increasing the lustre of the electric light.

The Photographic Spectra of the Stars.—W. Huggins.

Glycogenesis in Infusoria.—A. Certes.—As far as the researches of the author have extended they seem to support the law of Claude Bernard.

Thermic Laws of Electric Sparks, produced by the Ordinary Incompletes and Partial Discharges of Condensers.—E. Villari.—*First Law*:—The galvanometric deviations produced by incomplete discharges are proportional to the quantities of electricity constituting the discharges themselves. *Second Law*:—The heat occasioned by the spark produced by the incomplete discharge is directly proportional to the quantity of electricity forming the spark.

Variations of Magnetic Declination Deduced from the Regular Observations made at Moncalieri, 1871 to 1878.—F. Denza.—Not susceptible of abstraction.

Potash contained in the Clay of Arable Soils.—A. Perrey.—Clays constantly contain potassa in quantities ranging from 1.8 to 7.3 (?) per cent.

Dissociation-Tension of Chloral Hydrate, and the Tension of the Vapour of Anhydrous Chloral.—MM. Moitessier and R. Engel.—The authors conclude that unless their experiments are inaccurate the vapour of chloral hydrate is merely a mixture of the vapour of water and of anhydrous chloral, and the hydrate is not decomposed and volatilised in the vapour of anhydrous chloral at a sufficient tension.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 2, January 8, 1880.

Changes in the Specific Gravity of Steel in Consequence of Tempering and Annealing.—C. Fromme.—The conclusions are drawn that hardening decreases the specific gravity of the steel, the increase of volume being slighter as the ingot is thicker. (This phenomenon has long been noticed; see "Useful Metals and their Alloys," published in 1857.) Tempering has the opposite effect.

THE CHEMICAL NEWS.

VOL. XLI. No. 1055.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Continued from page 62.)

It still remains for me to mention a possible modification in the atomic weights of *yttrium* and of *erbium* (perhaps also of *terbium*, if terbium is really an element existing in nature). However, the history of these interesting elements is still enveloped in darkness, according to the contradictory debates between Mosander and Delafontaine on the one hand, and Bunsen and Bahr on the other hand; they therefore require fresh researches. If we keep to the results obtained by these two last investigators, and if we admit with them that the equivalent of Yt = 30.85 (Berzelius found 32.1 and 35.0, Bopp found 34, and Delafontaine found 32), and that the equivalent of Er = 56.3,* except from any error resulting from them having been insufficiently studied, these elements can be placed in Group III., which receives the highest forms of oxides, such as R₂O₃. Yttrium, with the atomic weight 88, would come in the sixth series, immediately after Rb = 85 and Sr = 87, and consequently before Zr = 90 and Nb = 94; therefore the equivalent of Yt would be $= 29.3 = \frac{88}{3}$.

As it is very doubtful whether anyone has yet succeeded in separating Yt from Ce, La, Di, Er (perhaps also from Tb and Th), we can admit that the determinations of Bunsen and Bahr coincide sufficiently with the theoretical number.

This place for yttrium is justified, as the results of applying the periodic law to the preceding elements show, viz., by the strongly basic properties of the oxide, by the non-volatility of YtCl₃, by the composition of the sulphate, (Yt₂SO₄)₃.8H₂O, similar to the composition of the salt which didymium forms analogous to Yt, by the insolubility of the fluoride, &c. The erbium of Bunsen and Bahr, if we take their results as bordering on the truth, apparently belongs to Group III., Series 10, before La = 180, Ta = 182, and W = 184, and it should possess the atomic weight 178; the form of the oxide is therefore R₂O₃, and its equivalent = 59.3. We can explain the low number (56.3) found by Bunsen by supposing that his erbium was not free from yttrium.

To convince us of the exactness of these modifications of the atomic weights of Yt and of Er, we do not know any fact, as we do in the case of the metals of cerite; nobody has determined either the density of their oxides,† their behaviour in the presence of oxidising and reducing agents, or the composition and the form of their double salts. Here, then, more than in any other part of the system, are researches, guided by the periodic law, necessary.

To give another example of the direction in which researches on elements (based on the periodic law) should be made, I propose to pass now to the determination of the properties of elements which are still undiscovered. Without the periodic law, it would be absolutely impossible to foresee any of the properties of unknown elements; and, further, we could not form any idea of what gaps

were existing in the series of elements. The discovery of new elements was uniquely the results of observations, and was made either by chance or by the extra powers of thought and perception of the investigator. These discoveries did not offer any special philosophical interest; this is the reason we have seen such a small number of investigators venture on the study of the elements, like an exploring party in a new and unknown land, not knowing which way to turn; but now they will find that this vast and important domain of chemistry is not entirely devoid of landmarks, but that the periodic law will serve as a guide, and will facilitate future discoveries.

IV. THE USE OF THE PERIODIC LAW FOR DETERMINING THE PROPERTIES OF AS YET UNDISCOVERED ELEMENTS.

The preceding developments show us that the periodic law renders it possible for us to bring to light the unknown properties of elements whose atomalogues are known to us. Further, we can see, by referring to Tables I. and II., in which the periodic relations are shown, that many elements are missing, and we can confidently predict their discovery. I am therefore going to describe the properties of several as yet undiscovered elements: by this means I hope to show, in a new and perfectly clear manner, the exactitude of the law, although the confirmation of these proofs is reserved for the future. Let us add that the previous determination of the properties of unknown elements will facilitate the discovery of these elements, because knowing them we can foretell the reactions of their compounds.

So as to avoid introducing new denominations for the unknown elements into science I shall designate them by the name of the nearest lower analogue of the odd or even elements in the same group, and placing in front of this word one of the Sanscrit numbers (eka, dui, tri, tschatour, &c.). The unknown elements of the first group will be called ekacæsium (Ec = 175), duicæsium (Dc = 220), &c. If niobium, for example, were not known we could call it ekavanadium. The denominations will show the analogies very clearly; the names, however, of the fourth series have not this advantage, because they ought to be derived from those of the elements of the second series, and we know from Chapter I. that this typical series is not in complete atomic analogy with the fourth.

Besides, in this series there is only one missing element; it is in the third group, and is called ekaboron, Eb. As it follows K = 39, and Ca = 40, whilst it precedes Ti = 48, and V = 51, its atomic weight should be about Eb = 44; its oxide should be Eb₂O₃, but it should not have very characteristic properties; it will form in all respects the transition from CaO to TiO₂. In its maximum salts,

EbX₃, the equivalent of the metal will be about $15 = \frac{44}{3}$;

it will not, therefore, be lower than the equivalents of bases which are already known; it will be intermediate between the equivalent of Mg = 12, and that of Ca = 20.

We have seen in the foregoing chapter that Yt = 88 (?), Di = 138 (?), and Er = 178 (?) belong to this same group. However, the position of this latter is still very uncertain, and the elements have not been sufficiently studied; therefore the properties of Eb can only be determined by means of its atomalogy with the elements Ca and Ti of the fourth series. The case of this element is therefore more complicated than those of other unknown elements. In consideration of Ca and Ti only giving one stable oxide in the air, we may admit that Eb will only give one stable basic oxide, Eb₂O₃. The oxide in its properties ought to be to Al₂O₃ what CaO is to MgO, or what TiO₂ is to SiO₂; consequently it should be a more energetic base than aluminium, and at the same time it ought to agree with Al not only in its forms of corresponding compounds, but also, in many cases, in its properties. Thus, the sulphate Eb₂(SO₄)₃ will not be such an easily soluble body as Al₂(SO₄)₃, because sulphate of calcium is more difficultly soluble than sulphate of magnesium. The base

* Erbium, according to Delafontaine, has an equivalent lower than 39.68. Bunsen thinks that Delafontaine was working with an impure sample.

† The number 48.4 given by Ekeberg for the oxide of yttrium might be thought to relate to an impure substance. However, it corresponds to the periodic law, for the volume of 2SrO = Sr₂O₂ = 49, the volume of Yt₂O₃ = 47, the volume of Zr₂O₄ = 45.

formed by Ca being more feeble than that formed by Na, and, at the same time, TiO_2 being a less energetic base than Al_2O_3 , Eb_2O_3 will be weaker than MgO . Eb_2O_3 will therefore occupy, in many respects, the place between Al_2O_3 and MgO : numbers of its reactions will be explained by this circumstance.

Duiboron, or yttrium, gives an oxide, in every respect more energetic, as Sr gives a more marked base than Ca; however, even as there exists a great concordance between Ca and Sr, Ti and Zr, so shall we find many analogies between ekaboron and duiboron (yttrium?). If, then, yttrium is veritably duiboron, and ought to be accompanied by ekaboron, the separation of ekaboron from yttrium will be very difficult; in fact, it will only be possible by means of excessively delicate differences; for example, in the solubility of the salts, or in the energy of the basic oxides, &c.

Oxide of ekaboron will naturally be insoluble in alkalis, but we are uncertain as to whether it will drive off the ammonia from chlorhydrate of ammonia. The carbonate of ekaboron will be insoluble in water, and it will be precipitated as a basic salt, if we can judge from what we know of the salts of MgO and Al_2O_3 . Its salts will be colourless, and will give gelatinous precipitates with KHO , K_2CO_3 , HNa_2PO_4 , &c. Sulphate of potassium will give a double salt with $\text{Eb}_2(\text{SO}_4)_3$, a sort of alum, which will probably not be isomorphous with alum proper. A few salts only of ekaboron will crystallise well, and they will belong to the double salts. The degree of volatility of EbCl_3 will depend on the molecular formula of this compound; EbCl_3 will be volatile, but Eb_2Cl_6 and the higher molecules will not volatilise. Chloride of ekaboron will, in all probability, be more difficultly volatilised than chloride of aluminium, because TiCl_4 boils at a higher temperature than SiCl_4 ; because also CaCl_2 is more difficultly volatilised than MgCl_2 . But we may be permitted to presume that the salts of ekaboron will not be sufficiently volatile to be discovered by means of spectrum analysis. The chloride of ekaboron will naturally be a solid body: its anhydride will be decomposed by water more easily than MgCl_2 with the evolution of HCl .

As the volume of $\text{CaCl}_2=49$, and the volume of $\text{TiCl}_4=109$, the volume of EbCl_3 will be about 78, and its density will be 2.

Oxide of ekaboron will be an infusible powder, which will, after calcination, dissolve in acids, although with difficulty; it will easily give to water its alkaline reactions, but it will not completely saturate acid solutions of litmus so as to make the colour disappear. The density of the oxide will be about 3.5; the volume about 30, because the volume of $\text{K}_2\text{O}=35$, of $\text{Cu}_2\text{O}_2=36$, of $\text{Ti}_2\text{O}_4=40$, of $\text{Cr}_2\text{O}_6=72$.*

Ekaboron, the simple body, will be a light, non-volatile, difficultly fusible metal. It will only decompose water under the influence of heat, and even then incompletely; it will dissolve in acids, with the evolution of hydrogen. Its density will be about 3.0 (apparently higher), because its volume is about 15; for the volumes of metals in the even series diminish constantly from the first group. Thus the volume of $\text{K}=50$, of $\text{Ca}=25$, of Ti and V = about 9, and of Cr , Mn , and Fe = about 7.

(To be continued.)

Variations of the Electromotive Force of the Batteries of Grove, Bunsen, and Daniell, with the Concentration of the Liquids.—C. Fromme.—Grove's battery is most efficacious when the nitric acid contains 40 percent of water. In the Bunsen battery the strength of the current diminishes with the concentration of the acid. In Daniell's battery the electromotive force increases with the concentration of the solution of sulphate of copper, but with a less rapidity.—*Les Mondes*.

* Oxide of duiboron (yttria?) should, from an analogical point of view, have for its density the number 4.8 (see above.)

REMARKS ON THE NEW METALS FROM GADOLINITE AND SAMARSKITE.

By M. M. DELAFONTAINE.

SINCE M. Marignac and I published our memoirs on Terbia, in March, 1878, the real or supposed discovery of ten other rare earths has been announced by different chemists,—namely, mosandria, philippia, ytterbia, decipia, scandia, holmia, thulia, samaria, and two others not yet named. As may be imagined, such a multiplication of bodies difficult to isolate and characterise has provoked scepticism, and it has been suggested that some of them may be duplicates. If the Academy will permit me I will communicate my views on this subject as the result of my own researches.*

In my researches on gadolinite and samarskite I have always attached great importance to the atomic weights. When the equivalent of a base is found to be higher than that of the other members of the same group (decipia and ytterbia, for instance) I have not had the least doubt of the specific existence of this base. But if this equivalent, on the contrary, should be intermediate between two others (philippia as compared with yttria and terbia), I naturally have multiplied experiments to make certain that I am not working with a mixture of known bodies. In this case the physical characteristics have sometimes been of great help. For these reasons, therefore, ytterbia, decipia, and philippia appear to me definitely acquired for science.

Scandium is unknown to me; I can say nothing about it.

Nothing has taken place to make me change my opinion upon mosandrium since I proposed to erase it from the list of elements.

Samarium.—M. Lecoq published in February and August, 1879, two notes, one on a new earth obtained from samarskite and the other on samarium. The properties which he attributes to his unnamed earth do not appear to me to differ essentially from those of decipia, or rather to a mixture of decipia and terbia: samarium is only characterised by absorption rays which appear to embrace a portion of those of decipium. I do not know any facts which would lead me to imagine that decipium is not homogeneous; its absorption rays increase and diminish together in all the treatments to which I have submitted it. The discovery of samarium therefore demands verification by further experiments.

MM. Marignac and Soret have shown that Bunsen's erbia is a mixture of several bodies. With the assistance of M. Thalén, M. Clève has repeated their experiments with the same results. But the *savant* of Upsala has gone further than those of Geneva; he does not hesitate to give names to the supposed new metals which he believes to be the cause of certain spectroscopic differences. According to him the ultra-red ray discovered by M. Soret characterises thulium; the red and green rays ($\lambda=640$ and 536) belong to holmium,† whose oxide is yellow and the nitrate less easily decomposed by heat than that of erbium; beyond this he has neither obtained the first nor the second in a state of even approximate purity.

Samarskite contains very little of the old erbia of Bunsen: the absorption spectrum of this latter is very feeble with the solutions obtained from it, and in particular the green and blue bands ($\lambda=523$ and 488) of erbia (in a restricted sense) are feeble; on the contrary, the indigo band ($\lambda=462$) shows much better. After having separated the didymia, decipia, and almost all of the terbia, I submitted the formate of the earths to fractional crystallisation; the first products gave from 49 to 47 per cent of

* Not having been for some time in a position to see the *Comptes Rendus* I only know some of the notes on these new metals from what has appeared in the *CHEMICAL NEWS* of London. If, therefore, there is some misunderstanding on my part it must be attributed to a want of sufficient information.

† M. Soret likewise adds the indigo band, $\lambda=452$.

base. They were re-united and re-crystallised several times. In this way was separated a salt rich in terbia and mother-liquors richer in yttria: these were rejected. The rest, calcined and combined with nitric acid, showed the indigo band very strong, and the others more feeble. This nitrate, dissolved or in crystals, was perfectly colourless; its base, of a beautiful clear yellow, had the equivalent 98 ($\gamma_0 = 74$, $\pi_0 =$ about 115). This was submitted to a series of partial decompositions by heat, which divided it into six products (A to F). The earth from the first, slightly clearer, had the equivalent 102; the crystals of its formate were slightly rose-coloured; its absorption spectrum showed the absorption rays of erbia (in a restricted sense), but deeper. The four products following (B to E) contained a more coloured base; its nitrate, which was colourless, gave a spectrum consisting only of the red (640) and the green (536) rays, narrow, but much stronger than before, and of the large indigo band (448 to 455). The green ray of true erbia was reduced to a narrow trace; the blue had disappeared. These earths, B to E, could not be split up into yttria and terbia by any known means. Finally, the product F gave a feeble spectrum; its colouration and its atomic weight were less than those of the original mixture.

I have called philippia the yellow oxide, distinct from terbia, which forms the bulk of the earths of which I have just spoken. Its properties are those of the earth X of M. Soret and of M. Clève's holmia: this last named, being a duplicate, should not be retained.

In strictness it may be supposed that philippia is a mixture of two oxides, one of which gives the indigo band (448 to 455), and the other the rays 640 and 536; but I know of no fact in favour of such a conclusion.

The so-called euxenite from North Carolina contains still less of the earths giving absorption spectra than samarskite—a trace only. Besides a notable proportion of terbia I have obtained from it a straw-yellow earth, with an equivalent ranging about 90, without absorption spectrum, and which does not appear to be a mixture of terbia and yttria. I am not yet sure whether samarskite contains it, and in particular whether my philippia may be soiled by it: this is a point which I hope to clear up very soon. I am also examining another base from samarskite which appears to have a great resemblance to ytterbia.—*Comptes Rendus*.

PRODUCTS OF THE SLOW OXIDATION OF PHOSPHORUS.

By R. H. RIDOUT.

READING Professor McLeod's paper in the *Journal of the Chemical Society* for this month recalls some experiments I instituted as far back as 1874 or 1875, which point to the same conclusions, though the examination was undertaken with a different object. A country medical practitioner had under his care a very obstinate case of fistula, and conferred with a London specialist, who recommended an injection of "ozonised water." In vain did the medical man apply to successive wholesale chemists for this article. He then came to me, asking if I could prepare some. I told him that it was the general opinion that ozone was insoluble in water, but to make sure of the matter I instituted some experiments, and among others the preparation of ozone by moist phosphorus. A glass cylinder was fitted with a cork, having suspended from its centre a bracket containing lumps of phosphorus (about $\frac{1}{2}$ lb.), and was perforated with four holes. Through one passed a short tube bent at a right angle, and leading to the aspirator; through the second a tube terminating in a fine jet, and delivering water at a temperature of 20° to 22° C. on the phosphorus; through the third passed a Wirttemberg syphon, to remove the

superfluous water and prevent the cylinder from filling; and through the fourth a tube leading nearly to the surface of the water. On aspirating a current of air (20 litres per hour) an abundant evolution of an oxidising body ensued. The air was passed in its way to the aspirator through a wash-bottle containing water, but as this did not suffice to remove the phosphorus fumes it was led through two wash-bottles containing caustic soda, and then through recently distilled water. The oxidising body passed without loss through the caustic soda, and not a trace of any oxidising agent could be detected in the distilled water, after the action had been going on for six hours. It thus appeared that the body was insoluble in water, and not attacked by the caustic alkali. It could not therefore be peroxide of hydrogen, which is soluble in water, and would have been destroyed by the carbonate contained in the commercial soda used. Further, it appears to confirm what has so often been said before, that ozone is insoluble in water. Upon my client sending to the specialist for a sample of the "ozonised water," it turned out to be potash permanganate. Doubtless by this time the label bearing this name has become quite familiar.

THE RESULTS OF SEWAGE TREATMENT.

IN a recent issue we laid before our readers the able report on the efficacy of different methods of sewage-treatment as presented to the Local Government Board by Dr. Angus Smith, F.R.S. Both to municipal authorities and to the public at large it must be very satisfactory that this vexed question should have been finally dealt with by an authority whose competence and freedom from bias are alike beyond suspicion. There are in the report certain points of interest to which we beg to call especial attention. Five processes have here been specially selected: irrigation, pure and simple, as carried on at Aldershot; precipitation by lime, as conducted at Burnley (Scott's process), and again at Birmingham; precipitation by aluminous salts in conjunction with clay and charcoal (A B C process), as in action at Aylesbury; and finally, treatment with sulphate of alumina and lime, followed up by irrigation, as at Coventry. We think it may reasonably be inferred that these five processes have been selected not at random, but as the most successful. Had any other method now in operation in England been found superior, or even equal, as a means of purifying polluted waters, Dr. Smith would assuredly not have concealed a fact of such importance. The difficulty which municipal authorities have hitherto experienced in making a selection will in future be greatly reduced.

Aldershot may, we suppose, be fairly taken as a type of the irrigation-system, where carefully worked upon a suitable soil. That under such circumstances it is highly successful, as far as the purification of the sewage is concerned, cannot be denied. But this efficiency is greatly modified by the season, since in dry weather the Aldershot effluent takes the first rank with a proportion of only 0.0550 part of albuminoid ammonia for 100,000, whilst in wet weather this impurity rises to 0.207.

As regards the precipitation-processes the results of the lime-methods are much less striking. The Burnley effluent retains 0.305 part of albuminoid ammonia per 100,000, or 25.4 per cent of the proportion originally present. At Birmingham the albuminoid ammonia left in the purified water rises to 0.66, or 62.9 per cent of the original quantity. Sulphate of alumina is more efficacious.

The Aylesbury effluent (A B C process) retains only 0.070 per 100,000 of albuminoid ammonia (=0.0490 per gallon), or 10.6 per cent of this impurity as existing in the sewage. It will be noticed how decidedly this falls within the limit (0.121 part per gallon) proposed by the late Rivers' Pollution Commission. This process is therefore not merely

superior to the lime treatments, but in wet weather to irrigation. These results strongly confirm the opinion expressed some years ago by the Editor of this Journal, that the purification of sewage must turn to a great degree upon the price of the soluble salts of alumina.

It will readily be admitted that if, *e.g.*, the Aylesbury effluent were subsequently passed through land, it would be still further purified. We need therefore feel no surprise that the water at Coventry, where a double system prevails, should be of a very high quality. The sewage of that town, which is first treated with sulphate of alumina and lime and then run upon the soil, retains, as it finally issues into the river, only 0.061 of albuminoid ammonia per 100,000, or 3.60 per cent of its original proportion. Irrigation after the use of lime, however, gives less striking results. Sewage thus dealt with at Birmingham still retains 0.210 of albuminoid ammonia, or 20 per cent of the initial quantity.

It must not be forgotten that four other samples of effluents taken at Aylesbury—on July 23rd, 26th, and 29th, and September 4th—were all superior in purity to the one above-mentioned: that taken on September 4th containing merely 0.035 of albuminoid ammonia for 100,000, or 0.024 per gallon. We fear that many waters more heavily charged with organic pollution are used for household consumption.

Dr. Angus Smith's results certainly prove that sewage is the most completely purified by irrigation if all the circumstances are favourable, *i.e.*, if the soil is of a suitable nature and the weather is dry. Where these conditions are wanting it is excelled by a judicious application of aluminous or aluminous-ferrous salts. These conclusions agree substantially with the results of our own experience. We have always contended that no one process could at all places and under all circumstances claim the preference. Where suitable land can be procured near the point where the sewage has to be discharged, and especially where the climate is dry, we have fully recognised the value of irrigation. But in the numerous cases where the conditions are the reverse, we hold that a judicious system of precipitation is the only practical expedient.

RESEARCHES ON COBALT AND NICKEL, AND A METHOD FOR THEIR DISTINCTION IN MIXED SOLUTION.

By Dr. G. PAPASOGLI.

If a strip of zinc is plunged in a solution of the double cyanide of nickel and potassium, we observe suddenly, or in a short time, that it is covered with small bubbles which gradually increase and form a continuous gaseous envelope, whilst upon the zinc nickel is deposited in the state of a black powder, and around it there are produced dense clouds formed by an intensely red liquid.

This liquid is the solution of a new salt of nickel in potassium cyanide, having a greater density than the medium in which it is found, and descending to the bottom and slowly increasing it colours the whole mass of the liquid a blood-red.

On attentively observing the progress of the reaction it was seen that the colouration reached a maximum limit at which the double cyanide must have undergone a complete modification, but the compound which is formed becomes modified in its turn, since the colouration slowly decreases in intensity and finally disappears altogether. The author considers that in this experiment the red colour which is developed depends on a new combination of cyanogen and nickel produced by the reduction which the cyanogen undergoes from the action of the nascent hydrogen evolved when a small particle of the metal is deposited upon the slip of zinc. This opinion is confirmed by the following experiment:—On causing a solu-

tion of the double cyanide of nickel and zinc to be traversed by an electric current, and having care to keep the two platinum rheophores separate, so as to observe separately the action of the oxygen and the hydrogen, there was observed at the negative pole where hydrogen was evolved a reddish colouration, more or less intense according to the quantity of nickel present, whilst at the positive pole there was no colouration. This experiment shows that the zinc takes no part in the reaction, but is merely a means of evolving hydrogen. The same result is obtained if aluminium is applied in place of zinc.

If the experiment which has been made with the double cyanide of nickel and potassium is repeated with a double cobaltic cyanide, it is seen easily that the reaction above mentioned is peculiar to the former metal.

If a slip of zinc is immersed in a solution of the double cyanide of cobalt and potassium the metal becomes covered with a black powder and a gaseous envelope, but there is not the slightest colouration. If such a solution is traversed by an electric current it remains colourless at the negative (Zn) pole, but assumes a maroon colouration at the positive (Cu), due probably to para-cyanogen mixed with an oxide of cobalt.

The author next sought to ascertain if the presence or cobalt injures the reaction, preventing it altogether or rendering it less sensitive. He finds that it is totally unaffected, and that on experimenting either with the slip of zinc or with the electric current it is possible to detect 0.0005 of nickel mixed with an excess of cobalt. In such delicate researches it is necessary to operate upon very concentrated solutions.

There is no want of characteristic reactions for the detection of cobalt and nickel when they are separate from each other, and which permit to recognise cobalt when mixed in all proportions with nickel. But it is not so when a small quantity of nickel is mixed with cobalt, in which case it has been difficult to establish its presence.

Although nickel when alone displays very salient characteristics, they are all masked by the presence of cobalt.

In a note on the alkaline polysulphides as reagents for cobalt, the author made known that on treating a solution of the double cyanide of cobalt and potassium with any of these polysulphides there is produced an intense red colouration, peculiar to cobalt, upon which the simultaneous presence of nickel has no appreciable influence.

The author proposes the following method, based upon these two reactions, for the recognition of cobalt and nickel:—

To an acid solution of the two metals is added a slight excess of a solution of the hydrate of potassium, to precipitate the metals in the state of basic salts. The precipitate is allowed to settle, the alkaline liquid decanted off, and the deposit repeatedly washed to remove the excess of potassa. A few drops of ammonium chloride and of ammonia are then added, and it is heated in a slight excess of potassium cyanide.

Care must be taken, as far as possible, not to stir the solution of the double cyanide, to prevent the absorption of atmospheric oxygen. If the double cobalt solution is converted into cobalti-cyanide, the addition of an alkaline polysulphide no longer produces the red colouration mentioned above.

The solution of the cyanides is then divided into two portions. To the first are added a few drops of the ammonium polysulphide, so that it may form a stratum above the cyanide solution. If in the plane of contact of the two liquids there is observed a red colouration, this indicates the presence of cobalt. The reaction is independent of the presence of nickel, and is sensitive to the fourth and even the fifth decimal place if only the solution is highly concentrated.

In the other portion of the solution is immersed a small slip of zinc: if the liquid contains nickel there will be observed, besides the gaseous envelope, an intense red colouration which shows itself over the whole surface of

the slip, but especially at the lower part. The presence of cobalt does not interfere with this reaction, which is still produced even if the solution containing the cobalt has absorbed oxygen.

If the solution is concentrated the sensitiveness of the reaction extends to the fourth decimal place. The presence of ammoniacal salts in excess retards, or even hinders, the reaction.

The solution of potassium cyanide used should be concentrated, but an excess in quantity should be avoided, especially if the quantity of nickel is small. If an excess of the cyanide is used, then, on immersing the slip of zinc, there occurs suddenly a strong evolution of gas, which disperses the red colouration and makes it difficult to perceive the reaction.

To avoid this inconvenience the author places in the solution to be examined a slip of platinum, joined at one extremity to a slip of zinc. The greater portion of the gas then envelopes the platinum, and as the red colour forms only around the zinc it is less readily dispersed, and the reaction remains more sensitive.—*Gazzetta Chimica Italiana*.

ON THE PHYSICAL CONSTANTS OF LIQUID HYDROCHLORIC ACID.*

By GERRARD ANSDELL, F.C.S.
Chemical Assistant at the Royal Institution.

In a paper read before the Royal Society, on June 18th, 1879, I described the results of some experiments made with liquid acetylene, obtained by compressing the gas in one of M. Cailletet's ingenious pumps for the liquefaction of gases. The tensions of the saturated vapour then obtained were so entirely different from M. Cailletet's results, that it appeared to me to be possibly due to a slight impurity in the gas, probably a trace of mono-bromethylene which always occurs in the red copper compound of acetylene, and is very difficult to completely eliminate.

I therefore prepared a quantity of the silver compound (which can be obtained entirely free from mono-bromethylene) by passing acetylene prepared in the same way as formerly, viz., by the action of alcoholic potash on dibromethylene, into an ammoniacal solution of nitrate of silver.

On filling several tubes with acetylene generated by the action of hydrochloric acid on this compound, the tensions were found to be exactly the same as those obtained with the acetylene made from the copper compound, thus proving the correctness of the former experiments.

As the Cailletet pump is so well adapted to the examination of gases in their liquid state, and is at the same time so easily managed, it appeared interesting to determine the physical constants of some other gases, and Prof. Dewar suggested an examination of the halogen acids, with regard to their physical relations to each other, more especially as we have absolutely no reliable knowledge as to these numbers.

Faraday was the first to liquefy hydrochloric acid gas in 1823, and he subsequently, in 1848, made a series of determinations of the vapour tensions of the liquid at different temperatures. He also liquefied hydrobromic and hydriodic acids, but gives no account of their properties in a liquid state. Gore (*Proc. Roy. Soc.*, vol. xiv.) examined very carefully the chemical properties of liquid hydrochloric acid, its solvent action, &c., but made no determinations of its physical constants.

Hydrochloric acid is the only one I have as yet had time to examine, with the following results:—

The gas was made by the action of strong sulphuric acid on dry chloride of ammonium, being afterwards freed from sulphuric acid and dried before entering the tubes. It was found of no advantage to continue the stream of

gas for more than six hours, as a constant minimum of air always remained in the tube at the end of this time (about 1-400th of the total volume of the tube), which appeared utterly impossible to remove.

The pump was used in the same way as described in my former paper, two iron reservoirs being used, one containing the air manometer, and the other the tube with the gas to be liquefied.

Apart from the mere determination of the vapour-tensions, densities, &c., the ratios of the volume of saturated vapour to that of the liquid was considered of the highest importance, as from these numbers the latent heat of transformation and other important data can be easily calculated. For these reasons the gas was examined in rather a different way to the acetylene, the volume to which it had been compressed at the point of liquefaction (or the volume of the saturated vapour) at any given temperature being first accurately determined, and then the pressure increased until the condensed liquid entirely filled the upper part of the tube. The volume of this liquid column was then measured, so that a comparison between the volume of the saturated vapour and the volume of the total condensed liquid was obtained at each temperature.

This fractional volume of the gas at the point of liquefaction was determined by observing when the air manometer ceased to show an increase of pressure on compressing the gas; the volume of the gas at this point being then measured, and the pressure being then gradually released until the air manometer showed a slight decrease of pressure—the volume of the gas was then again measured at this point, and the mean taken as the point of liquefaction.

The results of the whole series of experiments are recorded in a condensed form in the following table:—

A.	B.	C.	D.	E.	F.
4	137.31	$\frac{1}{38.89}$	7.55	18.18	29.8
9.25	118.96	$\frac{1}{45.75}$	7.90	15.05	33.9
13.8	103.50	$\frac{1}{53.19}$	8.35	12.39	37.75
18.1	91.77	$\frac{1}{61.17}$	8.74	10.50	41.80
22.0	81.19	$\frac{1}{70.06}$	9.10	8.92	45.75
26.75	69.69	$\frac{1}{82.94}$	9.50	7.33	51.00
33.4	55.75	$\frac{1}{105.98}$	10.12	5.50	58.85
39.4	44.85	$\frac{1}{134.33}$	10.68	4.19	66.95
44.8	36.34	$\frac{1}{168.67}$	11.96	3.03	75.20
48.0	31.33	$\frac{1}{197.60}$	12.00	2.61	80.80
49.4	27.64	$\frac{1}{224.96}$	12.92	2.13	84.75
50.56	25.70	—	14.30	1.79	85.33
51.00	23.96				

In this table—

A = temperature of gas.

B = volume of the saturated vapour at point of liquefaction.

C = fractional volume of the gas at point of liquefaction in relation to the initial volume under one atmosphere of pressure.

*. Read before the Royal Society, January, 1880.

D = volume of the condensed liquid.

E = ratio of volume of liquid to that of the gas.

F = pressure in atmosphere.

The critical point was found to be 51.25°C .

It will be seen from this table that the volumes of the saturated vapours and liquid gradually approach each other as the temperature nears the critical point, and would undoubtedly become identical, if the experiments could be carried on up to the critical point. Curves accompany the paper, the first of which represents the change of volume of the saturated vapour and liquid, in relation to the temperature; and the second the ratio between these volumes in relation to the temperature (column E in the table) show these results in a graphical form.

Curves.

The ratio between the volume of the saturated vapour and the volume of the liquid at different temperatures decreases very regularly until within about three degrees of the critical point, where a singular point in the curve occurs, and the ratio approaches unity with great rapidity. The volume of the liquid increases very regularly up to a temperature of about 48°C ., and at 51°C ., or within 0.25 of a degree of the critical point, the distinction between the saturated vapour and the liquid vanishes, as although liquid is plainly seen to condense on the surface of the mercury, on increasing the pressure the line of demarcation immediately disappears, and it is impossible to say whether the tube is filled with the saturated vapour or the liquid itself; therefore no results could be obtained nearer the critical point than about a fourth of a degree.

Avenarius, in a paper entitled "The Causes which Determine the Critical Point" (Acad. Sci., St. Petersburg, 1876-77), made a number of experiments on ether, and came to the conclusion that the volumes of the saturated vapour and of the condensed liquid at the critical point were not identical. He was under the disadvantage of working at a very high temperature, the critical point of ether being 192.6°C ., which must have been very difficult to keep fixed within half a degree.

My own experiments, which were carried out at a very much lower temperature, and were consequently capable of greater accuracy, on account of the facility with which the temperature could be kept constant to within $1/20$ th of a degree, appear to confirm his results, in so far as it is evidently impossible to measure the relative volume of fluid and gas within less than a fourth of a degree of the critical point, and at this place the volumes are certainly unequal. This, however, does not disprove their identity as the critical point.

The density of the liquid at different temperatures was determined in the same way as described in my former paper, and gave the following numbers:—

Temperature.	Density.
0 $^{\circ}\text{C}$.	0.908
7.5	0.873
11.67	0.854
15.85	0.835
22.7	0.808
33.0	0.748
41.6	0.678
47.8	0.619

It has therefore not quite such a high density as liquid carbonic acid, which is 0.95 at 0°C ., and is about twice as high as acetylene, which is 0.450 at the same temperature. It is interesting to note that acetylene is the lightest known fluid substance. On referring to the density of liquid hydrochloric acid given by other authors, Professors Roscoe and Schorlemmer, in their recent treatise on chemistry, describe it as a colourless liquid having a specific gravity of about 1.27. As this value is so much higher than that obtained by myself, and as I feel confident of the accuracy of my results, it seems evident that a mistake has in some way arisen, and that the value given by them refers to the density of the gaseous acid

with reference to air, and not to the liquefied gas. Unfortunately Faraday does not seem to have determined its density.

Experiments made to determine the coefficients of compressibility of the fluid at different temperatures were carried on in the same way as formerly detailed. The following are the chief results:—

Mean coefficients of compression at different temperatures.

Range of pressure from 52.8 to 208.19 atmospheres.

Temperature of HCl.		Coefficients.
47.0 $^{\circ}\text{C}$.	=	0.00166
41.6	=	0.00123
33.0	=	0.00096
22.7	=	0.000635
15.85	=	0.00062
10.5	=	0.00054
5.7	=	0.000397

It is rather more compressible than liquid acetylene, whose mean coefficient at 22.3°C is 0.00058.

The latent heat of hydrochloric acid and other values depending on thermo-dynamical considerations, will be given in a separate communication.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 5, 1880.

Mr. WARREN DE LA RUE, President, in the Chair.

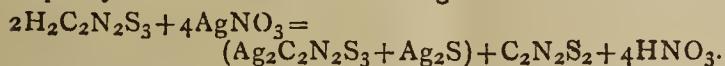
The following certificates were read for the first time:—
M. Bechler, W. J. Dibdin, A. C. Fryer, C. C. Graham, C. P. Sheibner, J. R. Skelton.

The following communications were read:—

"Note on the Assumed Formation of Ozone by the Atmospheric Oxidation of Phosphorus," by C. T. KINGZETT. The author criticised a short paper recently read before the Society by Prof. McLeod on the above subject. The quantitative experiments in Prof. McLeod's paper consisted in aspirating air over phosphorus partly submerged in water, and then heating the gas to a temperature at which peroxide of hydrogen is assumed to be decomposed into water and oxygen. Any water formed was absorbed in a weighed sulphuric acid tube. In one experiment 2760 c.c. of air thus treated increased the weight of the sulphuric acid tube 0.0006 gm., while the gas which passed through liberated iodine in a potassic iodide mixture equal to 1.8 c.c. of decinormal sodium thiosulphate. Prof. McLeod then proceeds:—"Now as 1 c.c. of the thiosulphate corresponds to 0.017 gm. of hydroxyl, which on decomposition by heat would form 0.009 gm. water, and as we may reasonably assume that at 200° at least the half of any hydroxyl that might be present would be decomposed, we should in the last experiment expect an increase of about 0.016 gm. in the sulphuric acid tube instead of only 0.0006 gm." Prof. McLeod therefore concludes that the active agent cannot be hydroxyl, and is consequently ozone. The author of the present paper contends that the above argument falls to the ground because 1 c.c. of decinormal thiosulphate is not equivalent to 0.017 gm. of hydroxyl, but only to 0.0017 gm.; so the gain in the sulphuric acid should be 0.0016 and not 0.016 gm. Passing exposure to 200° would probably not decompose a very small quantity of peroxide of hydrogen diffused through nearly 3 litres of air. The author, therefore, does not accept McLeod's experiments as in any sense conclusive, and hopes to present shortly a further communication on the subject.

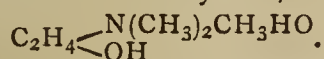
The SECRETARY then read a paper, entitled, "Contributions from the Laboratory of the University of Tokio,

Japan (II. On Persulpho-cyanate of Silver)," by R. W. ATKINSON. In a previous paper (*Chem. Soc. Journ.*, ii., 1877, 254) the author proved the above salt to have the formula $\text{Ag}_2\text{C}_2\text{N}_2\text{S}_3$; also, that when boiled with water, this yellow salt was converted into a black precipitate, in which the ratio of silver to sulphur varied, the variation being at that time attributed to the presence of some silver sulphocyanate in the silver sulphide, which formed the main bulk of the substance. From further experiments the author concludes in the present paper that the body mixed with the sulphide is some undecomposed persulphocyanate. The reaction being—



The actual proportions of the two insoluble silver salts formed depends on the influence of time, temperature, and the quantity of free acid present. The author has not been able to isolate the cyanogen disulphide, but is at present examining the orange-red sublimate formed when dry silver persulphocyanate is heated, in the hope that it may contain the missing body. The paper contains several tables, giving the results of many experiments as to the influence of variations of time, temperature, and the quantity of the free acid present on the above decomposition.

On Methylated Dioxethylen-amines," by H. F. MORLEY. Wurtz showed that ethylene oxide unites directly with ammonia, forming mono-, di-, and tri-oxethylen-amine, and that by heating glycollic chlorhydrin, or ethylenic oxide, with a solution of trimethyl-amine, neurine chloride is obtained; Baeyer having proved neurine to be trimethyl-oxethylen-ammonium hydrate,—



Various similar compounds were subsequently prepared, but no one had investigated the action of mono- and dimethyl-amine on glycollic chlorhydrin. These reactions the author has studied in the present paper. He has prepared and analysed the platinum salts of mono- and dimethyl-di-oxethylen-amine. The first crystallises in orange-red prisms; the second separates in small yellow crystals.

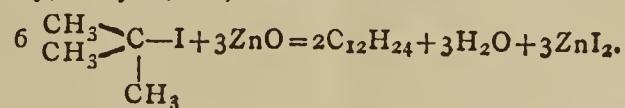
"Note on Igasurine," by W. A. SHENSTONE. The author has prepared some so-called igasurine of Desnoix. Four gallons of an aqueous decoction of *nux vomica* beans, from which the alkaloids had been precipitated by boiling with lime, were evaporated; on examination, the residue contained notable quantities of strychnine and brucine, the quantity of strychnine present being sufficient to explain the superior activity of the so-called igasurine over that of brucine. The other property which was supposed to distinguish it from brucine, viz., its superior solubility, being of little importance, as the solubility of brucine varies with its purity. Igasurine is therefore a mixture of brucine and strychnine.

Mr. HOWARD and Dr. WRIGHT commented on the utter uselessness of the solubility of an alkaloid as a test of its nature, unless the alkaloid was perfectly pure.

Prof. TIDY remarked that there was a marked difference between the physiological action of igasurine and strychnine.

"On some Reactions of Tertiary Isobutylic Iodide," by L. DOBBIN. Primary isobutylic iodide, prepared in the usual way from the corresponding alcohol, was allowed to flow from a dropping funnel into a boiling alcoholic solution of potassic hydrate; isobutylene was formed and collected in a gas-holder. The isobutylene was then shaken vigorously with fuming solution of hydriodic acid, the mixture being iced. The product thus obtained was separated from the aqueous hydriodic acid solution, treated with dilute potassic hydrate, dried with calcic chloride, and filtered. On prolonged shaking with a 12 per cent solution of hydrocyanic acid tertiary isobutylic iodide dissolves. After treatment with zincic oxide and distillation some quantity of trimethyl carbinol was

obtained in crystalline masses melting at 25.5° , boiling at 82°C . A combustion was made, and numbers obtained closely agreeing with those calculated. It was afterwards found that the hydrocyanic acid takes no part in the reaction, which occurs equally well when water alone is used. This furnishes the first example of an alcohol radical being decomposed by water at ordinary temperatures. On mixing tertiary isobutylic iodide with the quantity of zinc oxide necessary to combine with the iodine present, the temperature being kept at about 15°C ., isobutylene was formed, and identified by its vapour-density, analysis, &c.,—



Lermontoff also prepared the substance (*Liebig's Annalen*, 196, 116) by the action of calcic oxide, at 100°C . in sealed tubes, on tertiary isobutylic iodide saturated with isobutylene; she found it, however, to be mixed with isobutylene, and gives no quantitative results. The author of the present paper obtained no sensible quantity of isobutylene, and the reaction was effected at the ordinary temperature. Wislicenus observed that when tertiary isobutylic iodide was acted on by sodium a considerable quantity of gas was formed. The author has studied this reaction, and analysed the gas which was obtained mixed with atmospheric air. 630.6 c.c. were obtained by the action of 1 grm. of sodium. The gas contained:— C_4H_8 , 16.5 per cent; C_4H_{10} , 1.14 per cent; H_2 , 18.07 per cent. The formation of hydrogen is explained by the polymerisation of some of the isobutylene. Thus, by decomposing tertiary isobutylic iodide by sodium, the bodies formed are isobutylene, isobutylene, and hydrogen, with small quantities of a hydrocarbon not absorbed by fuming sulphuric acid. No isodibutylene was found.

Mr. M. W. WILLIAMS suggested that the action of dry hydriodic acid on the dry gas should be investigated: as the author had shown that water decomposed the iodide, the hydrogen might be formed by the action of sodium on the products of decomposition of the iodide by water.

The Society then adjourned to February 19, when a ballot for the election of Fellows will be held.

PHYSICAL SOCIETY.

THE annual Conversazione of this Society was held on Saturday evening, February 7, in King's College. The Museum of King George III., the Wheatstone Laboratory, and other halls of the College were occupied by a fine display of physical apparatus and artistic furniture, including numerous relics of Sir Charles Wheatstone. There was a large number of ladies and gentlemen present, and during the evening selections of music were played by the Musical Association of the Royal School of Mines. The apparatus was peculiar to the whole range of physical science, and were furnished in part by the College and in part by the various instrument makers and electric engineers of the metropolis. Mr. Crookes exhibited his experiments, and delivered a short lecture on *Radiant Matter*. The Telephone Company and the British Electric Light Company contributed telephones and electric lamps, and Herr Faber exhibited his ingenious speaking machine.

Detection of Oleo-Margarin in Butter.—The author remarks that since it has been found practicable, by the use of lard oil to lower the melting-point of margarin, the determination of the melting-point of butter, or of the fatty acids which result from its saponification, is no longer sufficient for the detection of sophisticated butters. It is only by determining the fatty acids, soluble or insoluble, in water that the fraud can be detected.—*Les Mondes*.

CORRESPONDENCE.

NITROUS COMPOUNDS IN SULPHURIC ACID.

To the Editor of the Chemical News.

SIR,—In a paper which Mr. Maclear read some time ago to the Newcastle Chemical Society, and which has been republished in your last few numbers, that gentleman attacks some of my previous statements on the estimation of nitrous and nitric acids. As since that time a good deal more has been published on this subject, and as the same is fully discussed in various parts of my "Treatise on the Manufacture of Sulphuric Acid and Alkali," I refrain from requesting you to publish my reply to Mr. Maclear, which was read to the Newcastle Chemical Society on November 28, 1878, and is printed in its *Proceedings*, vol. iv., pp. 177—184. I shall only solicit your permission to mention, as briefly as possible, the more important heads of my reply.

First. I pointed out that some of the objections made against my statements arose from mere misunderstandings. My analytical processes having been worked out to assist me in the theoretical study of the action of SO_2 on nitrosulphonic acid, they are not affected by the impurities existing in factory acids.

Secondly. I never thought of condemning the estimation of nitric acid by reduction to ammonia *on principle*, but I pointed out that this process sometimes yielded bad results, even when all the prescriptions given for it up to that time were strictly observed, and that, therefore, other processes of undoubted accuracy should be preferred to it. Possibly Mr. Maclear's own modification might be reliable, but of that I could know nothing then. Curiously enough, Mr. Maclear quotes Dr. Eder as confirming the accuracy of the reduction process, whilst that chemist most decidedly proves that this process, in *all* the shapes published up to that time, cannot be depended upon. Only for his modification he claims perfect accuracy, and this I had been able to confirm some time before I knew of Mr. Maclear's paper; but that does not touch my assertion respecting the *former* processes. Still I maintained (and to this I adhere now as well) that the reduction processes for estimating acids are not to be recommended for nitrous vitriol and similar compounds, because they require a good deal of apparatus and very much time; whilst the permanganate process, combined with the ferrous sulphate process, or Crum's mercury method, fulfils all requirements for pure acids, and the latter even for impure ones at a mere fraction of the time.

Mr. Maclear condemns the analytical method I employed for estimating both nitrous and nitric acids at the same time as "quite absurd" because the blank experiment required 1 per cent of permanganate in excess. That expression was really not called for: the "1 per cent" excess over the theoretical quantity means 0.15 c.c., which I stated would be required for staining the large bulk of liquid, and which anyhow would only amount to 0.0013 m.grm. of N_2O_3 on a total of about 50 grms. of vitriol! No other analytical process could have estimated such a small quantity with certainty.

Touching arsenious acid, I merely claimed that its interference is of very slight amount, even in ordinary nitrous vitriol, and this is proved by many experiments I made on purpose.

Mr. Maclear calls it "quite incomprehensible that I should have forgotten the fact that N_2O_5 is formed from the decomposition of N_2O_4 in the presence of water." That would have been very careless indeed on my part, but I cannot comprehend that he did not recognise N_2O_4 to be a misprint for N_2O_3 . This is so fully proved, both by the context and by my *previous* publications on the subject, that I need not stop to argue the point. After Winkler, who first discovered the facts, nobody has shown more conclusively than myself that under the circum-

stances in question only N_2O_3 (*not* N_2O_4) is formed, viz., by the action of an excess of oxygen on NO in the presence of sulphuric acid.

Mr. Maclear disagrees from my opinion that in *normal* nitrous vitriol nitric acid does not occur, but merely N_2O_3 . Apart from my other reasons, all founded upon experimental evidence, I showed in my reply that even in a case quoted by Mr. Maclear, where he had purposely produced quite *anomalous* conditions in an acid chamber, conducive to the formation of N_2O_4 , and consequently of NO_3H in the Gay-Lussac acid, only 5.92 per cent of the nitrogen in the latter was found to exist as nitric acid.

As I have no wish to be drawn into a fresh controversy on a subject which may now be called well worn, I have purposely remained entirely on the defensive, and merely shown that I did not say the things attributed to me. My positive arguments can be found in my pretty numerous publications on that subject, both English and German, and in a condensed form in my "Treatise."—I am, &c.,

GEORGE LUNGE.

Polytechnical School, Zurich,
January 29, 1880.

COMPOSITION OF WELDON MUD.

To the Editor of the Chemical News.

SIR,—Mr. J. E. Davis wonders why the reproduction of a paper on this subject by T. Post, in the *Transactions of the Society for the Promotion of Industry* (of Berlin), should be prohibited. I really do not know myself why this very unusual prohibition was laid down in that special case; but in any case it only refers to a *literal* reproduction, not to abstracts nor to translations; and the whole substance of the paper was given by Post, in June and July last, in the *Berliner Berichte*, of course without any reservation as to reproduction. A refutation of some important points in that paper, which has been read before the Newcastle Chemical Society, will no doubt be noticed shortly in the *CHEMICAL NEWS*.—I am, &c.,

GEORGE LUNGE.

Zurich, February 9, 1880.

CUPRIC TEST-PELLETS FOR SUGAR.

To the Editor of the Chemical News.

SIR,—The idea of giving to the chemist a reliable sugar-test that would keep any length of time is not a new one, and has been some years ago realised by Schiff (*Annalen der Chemie und Pharmacie*, Band 112, p. 369), viz., 250.0 of sulphate of copper in crystals are dissolved in a little warm water, and mixed with a solution of 280.0 tartrated soda. The light blue salt which separates is washed and dried at ordinary temperature; its formula is $\text{C}_4\text{H}_2\text{O}_5, \text{CuO} + 3\text{HO}$, and it contains 29.964 per cent of oxide of copper. If well washed until the wash-water ceases to give a reaction with chloride of barium, 3.686 of it are reduced by 0.5 glucose: this quantity, after having been verified by titration, is filled into small glass tubes, stoppered, and kept standing in a box protected from daylight. When required for use the contents of one tube are dissolved in sufficient soda-lye of 1.14 sp. gr.—I am, &c.,

H. W. LANGBECK.

London, February 9, 1880.

ALCOHOL TABLES.

To the Editor of the Chemical News.

SIR,—In the last number of the *CHEMICAL NEWS* (vol. xli., p. 70) Mr. Allen has given a formula for obtaining percentage by volume of alcohol for a given percentage of proof spirit, as preferable to mine, designating it as

"clumsy." But his formula, which I notice for the first time, is, as it stands, certainly not correct. Say, for instance, How much is 95.1 percentage by volume, percentage of proof spirit? By his formula $P = V \times 1.7525$ would give 166.6, which is wrong. Again, How much is 20 percentage of proof, percentage by volume? By his formula $V = P \times 0.5706$ would give 11.46, which figure is, I suppose, intended to be deducted from the factor 57.06 = 45.6, which in this case brings it to my formula; therefore is not my formula at any rate the more concise? Surely it needs not the ability of a Cambridge Wrangler to decide.

As for the standard figure for proof spirit, 57.27, sp. gr. 0.9186, I quote—"By parliamentary enactment the strength of proof spirit has been fixed at such a density that 13 volumes at 51° F. should equal in weight 12 vols. of water at the same temperature. According to this standard proof spirit has a gravity of 0.9186 at 60° F., and contains 57.27 per cent by volume, or 49.5 by weight of absolute alcohol."

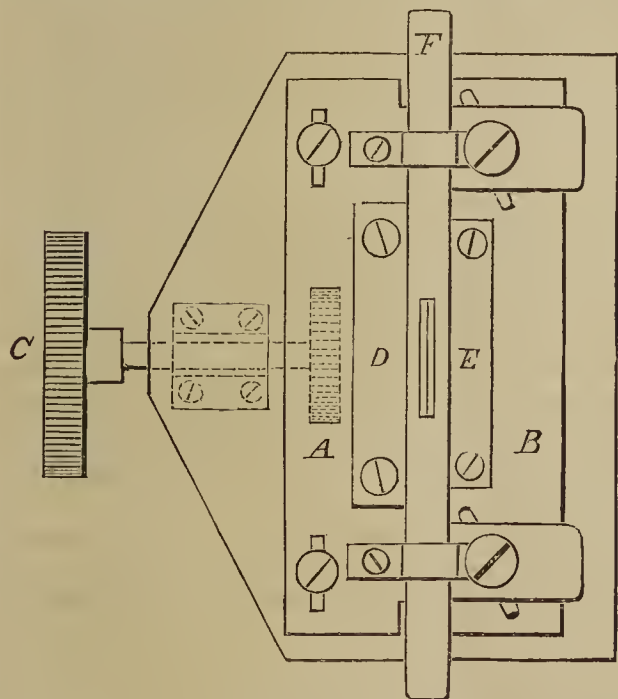
The object of my communication (CHEMICAL NEWS, vol. xli., p. 57) was merely intended to call attention to the utility of a set of tables for practical use, without any pretension of setting out the best or any particular formula.—I am, &c.,

S. CONNÉ.

SLIT FOR SPECTROSCOPE.

To the Editor of the Chemical News.

SIR,—I send a sketch of a spectroscope slit which, I think, has advantages over those ordinarily used. There is no fine screw which suffers generally from acid fumes; it occupies little room; and is, I think, easier to construct.



A is moved vertically by a rack; this displaces B sideways, both being guided by slots. D and E are movable iridium jaws; F a shutter for taking coincident spectra.—I am, &c.,

ALEX. E. TUCKER, Fell. Inst. Chem.

Rhymney Iron Works, Rhymney,
February 5, 1880.

Quinine Salts.—We are informed that Messrs. Domeier and Co. have been appointed agents of Messrs. C. F. Boehringer and Soehne, Mannheim, for the sale of their sulphate of quinine, quinidine, cinchonidine, cinchonin, and all other salts of quinine and santonine.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 3, January 19, 1880.

Heat of Formation of Chloral Hydrate.—M. Berthelot.—The author gives a detailed and illustrated description of the apparatus used in his experiments, and considers it proved that gaseous chloral and gaseous water combine with the disengagement of heat, forming gaseous chloral hydrate.

Chloral Hydrate.—A. Wurtz.—The author, after hearing the description of M. Berthelot's experiments, declares that the apparatus realises the same conditions which were observed in his own experiments. He points out that chloral contains always a small quantity of hydrochloric acid, which requires special precautions. He has never observed in his experiments the smallest elevation of temperature.

Plants which Serve as the Basis of the Different Curares.—G. Planchon.—All the plants which serve as the basis of the American poison belong to the genus *Strychnos*. There are four distinct regions in which the poison is prepared, and in each, distinct species of plants are employed. In British Guyana the chief species is *S. toxifera*, accompanied by *S. Schomburgkii* and *S. cogens*; on the upper Amazon the main material is *S. Castelnœana*, along with a menispermaceous plant. On the Rio Negro the chief ingredient is *S. Gubleri*, and in upper French Guyana the poison is chiefly composed of *S. Crevauxii*.

Remarks on the Use of Smithson's Battery for the Detection of Mercury, especially in Mineral Waters.

—J. Lefort.—The gold-tin element proposed by Smithson for the detection of traces of mercury has been subjected by Orfila to a criticism, which is only in part well founded. If the apparatus is left for a considerable time in a liquid supposed to be mercurial, a small quantity of tin may dissolve and be deposited upon the leaf of gold, so that it is turned white even in the absence of mercury. So far, therefore, the experiment may lead to a doubtful or erroneous result, but as tin is not volatile it is merely requisite to heat the slip of gold in a narrow tube, in order to drive away the adhering deposit, and then to act upon the metallic sublimate with vapour of iodine, thus forming mercuric iodide of a more or less decided red colour. If the mixture operated upon contains merely mercury, the operation thus described leaves no doubt as to the sensibility and the exactness of the process. But here the trustworthiness of the Smithson battery ceases, as it does not confine its action to the reduction of mercurial salts. The search for mercury in mineral waters has permitted the author to observe that arsenious and arsenic acids are easily reduced by the Smithson battery. As metallic arsenic is volatile, like mercury, and forms with iodine a compound of a red colour, more or less comparable to that of mercuric iodide, it follows that these two elements may be respectively confounded, especially when the red sublimate is obtained only in microscopic quantities. Hence, it appears that the oxides of arsenic are readily decomposed by metals under the influence even of a very feeble electric current. Thus a slip of copper immersed in a solution containing 1-100th of arsenic acid does not change its appearance, but if hydrochloric acid is added, and especially a little common salt, metallic arsenic is deposited upon the copper. This fact ought not be forgotten when the very general presence of mercury in mineral waters is admitted on the faith of imperfect analyses.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 15, 1879.

A Synthesis of Anthracen.—C. Loring Jackson and J. Fleming White.—The authors consider it as proved by their synthesis that the two atoms of carbon in anthracen are connected with both benzol nuclei in the ortho position.

Preparation and Derivatives of Xylylic Acid.—E. Ador and F. Meier.—A description of the barium, calcium, ammonium, and silver salts, of xylylic acid chloride, amide, and anilide.

A Peculiar Formation of Tolan-tetra-chloride.—C. Liebermann and J. Homeyer.—The authors obtained this compound on passing chlorine gas into boiling toluol on a large scale. They have not yet been able to determine the conditions under which this formation takes place.

Determination of the Solubility of Certain Constituents of Tar.—G. v. Bechi.—The solvents employed are absolute alcohol and pure toluol. The solution apparatus employed is that of V. Meyer. The results are given in the form of a table.

Formula of Quinhydrone.—R. Nietzki.—The author upholds the accuracy of the formula $C_{12}H_{10}O_4$ in opposition to Wichelhaus.

Nitro-benzoic Acids of Fittica.—C. Bodewig.—A series of crystallographic determinations, for the details of which the reader is referred to Groth's *Zeitschrift für Crystallographie*, iv., part 1.

Synthesis of Substituted Guanidins.—E. Erlenmeyer. The author points out that he has for some years been engaged with researches similar to those lately taken in hand by F. Berger (*Berichte*, xii., p. 1859).

On Skatol.—L. Brieger.—The author assigns to skatol, which he has obtained from human excrement, the formula $C_{10}H_{10}N$, or $C_{10}H_{11}N$. He has determined its melting-point and vapour density, and studied its physiological action. It appears to be less active as a poison than phenol.

Studies on Compounds from Animal Tar.—H. Weidel.—This long and important memoir does not admit of abstraction.

No. 16.

Allotropic Conditions of Hydrogen.—Julius Thomsen.—Referring to the statement of Dr. D. Tommasi (*Les Mondes*, xlviii., No. 7), that "the formation-heat of potassium chlorate is 9760 cal.; that of potassium chloride, 104476 cal.; during the conversion of chlorate into chloride there is consequently an absorption of heat corresponding to the difference of the formation-heat of these two compounds, $104476 - 9760 = 94716$, i.e., this quantity of heat must be supplied in order to reduce the chlorate to chloride." These statements contain a three-fold error. In the first place, according to the above data, the reduction of potassium chlorate to potassium chloride and oxygen would not absorb, but liberate, 94716 cal. Secondly, the figure 9760 is not the formation-heat of potassium chlorate, which amounts to 95840 cal., but the heat liberated on resolving this salt into potassium chloride and oxygen in the dry way. Thirdly, the reductions which Dr. Tommasi treats of refer to potassium chlorate dissolved in water, the liberation of heat on the decomposition of this salt in the moist way being 15370 cal. The theoretical considerations which Dr. Tommasi founds upon his figures lose, consequently, their value.

Formation of Glycosides of Several Members.—Hugo Schiff.—A notice of certain derivatives of helicin.

Thermo-chemical Researches on the Carbonates.—Julius Thomsen.—The author gives a table of the formation-heat of the anhydrous carbonates of potassium, sodium, barium, strontium, calcium, manganese, cadmium, lead, and silver as formed from metal, oxygen, and carbonic oxide. The formation-heats of the carbonates of barium, strontium, calcium, lead, and silver are also given when these compounds are generated by the union of car-

bonic acid with the metallic oxide. The quantity of heat absorbed during the decomposition of limestone in the kiln is 425 cal.

On some Derivatives of Resorcin and Orcin.—V. Merz and G. Zetter.—The authors describe the formation and properties of styphnic acid, trinitro-orcin, triamido-resorcin, amido-diimido-resorcin, trioxiquinon, triacetyl-oxyquinon, tribenzoyl-trioxiquinon, trioxytoluquinon, and various other compounds closely connected with the above.

Synthesis of Phenyl-naphthalin.—Watson Smith.—The author has somewhat modified his process, described in *Berichte* (xii., p. 1396), using naphthalin in excess, and has thus obtained phenyl-naphthalin in larger quantities. He gives the formula of the pure compound as $C_{16}H_{12}$. Its vapour-density as experimentally determined is 7.10, calculation giving 7.05. The corrected melting-point is 95° to 96° .

On Cyanallyl and the Products of its Saponification.—A. Pinner.—The author contends that in the formation of cyanallyl no transposition of the allyl-radicle ensues. On its saponification by hydrochloric and by potassa lye the respective products are β -chlorbutyric acid and β -oxybutyric acid, both of which on further treatment with potassa are transformed into ordinary crotonic acid.

Preparation of Acetic-ester.—A. P. N. Franchimont.—The author finds that all the hydrates of carbon yield perfectly pure acetyl-derivatives if heated with 4 parts of acetic anhydride and a small fragment of fused zinc chloride.

A New Method of Preparing Hydrobromic and Hydriodic Acids.—G. Bruylants.—The author introduces into a tubulated retort, containing 500 c.c. and fitted with a reflux-condenser, about 60 grms. of the essential oil of copaiba. At the other end of the condenser is a bent glass tube which effects a connection between the retort and a drying-tower for gases, the lower end of which contains a light asbestos plug, whilst an escape-tube opens into its upper end. The oil is slightly heated, 20 grms. of iodine are gradually dissolved, and the temperature is raised. In a few moments there begins a copious and regular discharge of gas. When it abates the retort is allowed to cool a little, and a fresh dose of iodine is introduced. As the reaction is now less energetic, the iodine can be introduced in larger portions. Heat is again applied, and the operation is continued till 150 grms. of iodine have been consumed, which should yield 145 to 150 grms. of hydriodic acid. In the preparation of hydrobromic acid the same process is followed, but the gas should be allowed to pass through two or three drying-towers. It is also necessary to place the bromine in a dropping-funnel, and allow about 20 grms. to enter drop by drop: 150 grms. of bromine and 60 grms. of oil yielded 142 grms. of hydrobromic acid.

Thermo-chemical Researches on the Oxides and Acids of Nitrogen.—Julius Thomsen.—The author gives tables showing the heat liberated or absorbed in the formation of the oxygen compounds of nitrogen, of the anhydrous nitrates, of the nitrates of the formula $RO_2N_2O_4$, and of their solution-heat. He points out the serious discrepancies between his own results and those of M. Berthelot (i.e., N_2O_5 .Aq. — 14800 Berthelot, and +180 Thomsen), and subjects the methods of the French chemist to a sharp criticism as far as NO is concerned.

Action of Nitroso-dimethyl-anilin upon Phenols not containing the Methyl Group.—Raphael Meldola.—A preliminary communication. The author dissolves 1 mol. β -naphthol in glacial acetic acid at about 110° , and adds gradually to the solution 1 mol. nitroso-dimethyl-anilin hydrochloride, when the mixture takes a fine blue colour and a bronze lustre. The melt is washed with water, dissolved in alcohol, and the solution is mixed with hydrochloric acid. The hydrochlorate of the base forms in long, splendid crystals, which dissolve in water and

alcohol with a fine violet-blue colour. Corresponding compounds have also been prepared from resorcin and α -naphthol. The former yields a violet-blue, and the latter a red colour.

Volatility of Platinum in Chlorine Gas.—F. Seelheim.—The author, on heating platinum-foil to bright redness in a current of chlorine for twenty-four hours, found a number of small crystals of platinum deposited in the tube beyond the foil. Platinous chloride was heated in a small, narrow-necked porcelain flask, to bright redness. The chloride disappeared, and the sides and bottom of the flask were found covered with minute but well-defined crystals of platinum, proving that platinum is decidedly volatile in an atmosphere of chlorine. As the assumption of the dissociation of chlorine in the experiments of V. and C. Meyer is based upon the supposition that platinous chloride, on heating, yields an absolutely fixed residue, its sole basis is thus removed, and there is no occasion for the hypothetical explanations of Lieben (*Comptes Rendus*, lxxxix., No. 6). The increase of one-third in the volume of the chlorine may be explained by the equation $\text{Pt}_2\text{Cl}_4 = 2\text{Cl}_2 + \text{Pt}_2 = 6$ vols.

On a Sulphuretted Colouring-matter from Paraphenylen-diamin.—A. Koch.—The base of this colour has the composition $\text{C}_{24}\text{H}_{20}\text{N}_6\text{S}_2$. As hydrochlorate it dissolves in water with a fine violet colour. Reducing agents decolourise the solutions, but the colour is restored by the oxygen of the air or by feeble oxidising agents. Powerful oxidisers completely destroy the compound. The precipitate given by acids dissolves in an excess of the precipitant with a pure blue colour. Several of its salts are here described.

Colouring-matters produced by the Action of Oxidisers upon Di- and Tetra-para-phenylen-diamin.—C. Wurster.—The red colour from the di-compound may be isolated in combination with bromine, and proves tolerably permanent. It is still more readily obtained as a ferro-cyanide.

Tetra-brom-ethans.—R. Anschütz.—An attempt to show the identity of Bourgoin's tetra-brom-ethan with perbrom-ethylen.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 3, January 15, 1880.

The Tay Bridge catastrophe is described under the heading "Barbarism of Exaggerated Civilisation."

Cast-iron Magnets.—M. Carré.—The author patents a process for making good magnets of cast-iron.

Utilisation of Sewage.—M. Ladureau.—The author contends that the sewage of Lille cannot be used in irrigation on account of the nature of the soil of the district, and of the humid and rainy climate, which would render irrigation in most cases, not merely injurious, but positively hurtful. The sewage of Roubaix Tourcoing he treats with lime and clay.

Various papers on chemical subjects are taken from the *Berichte der Deutschen Chemischen Gesellschaft* and from the *Chemiker Zeitung*.

No. 4, January 22, 1880.

Unexplained Electric Experiment.—If a hollow, open, metallic cylinder, capable of rotation around its axis, is placed above a magnet of double length, in such a manner that one of its extremities may coincide with the centre of the magnet, and the other with one of the poles, and if a strong electric current is passed from one end of the cylinder to the other it assumes a movement of rotation whose speed is independent of the resistance of the metal of which the cylinder is formed and of its thickness. Longitudinal grooves and slopes on the surface of the cylinder do not hinder the rotation. There is here, therefore, a conversion of electro-motive into pondero-motive force. Weber is of opinion that the resistance of the

metal to the passage of the current is the cause of this transmutation of energy, which, nevertheless, has been supposed independent of such resistance. He says that the first tendency is that of a rotatory movement of the current in the conductor, but as such movement is only possible by a passage of the electricity through the matter of the conductor, overcoming its resistance, the principle of the minimum of heat requires that the energy should be transported in an indefinitely short time to the conductor itself.

Electricity and Steel.—Signor Guidi proposes to convert iron into steel by electric action. To produce 2 tons daily the continuous action of an electro-dynamic machine of 120 horse-power is required.

Apparatus for the Industrial Analysis of Gases.—H. Orsat.—From the *Bul. de la Soc. d'Encouragement*.

MISCELLANEOUS.

Balmain's Luminous Paint.—*Engineering* says:—"The Lords of the Admiralty have been making experiments with it in a darkened room at Whitehall, and have expressed themselves in favour of it for lighting up the compartments of ironclads, or for the powder magazines; and two compartments of H.M.S. *Comus* have been ordered to be painted with it. For life-belts and buoys, it will of course be an acquisition in rendering them visible by night. A lantern capable of enabling a person to read or work in the dark can be made by framing a few square feet of painted surface; and the Superintendent of the West India Docks has ordered lanterns for use in their dangerous spirit vaults. The virtues of these lanterns in explosive mines, petroleum stores, and cellars, are too obvious to be dwelt upon. Mr. Towers, who has just supplied the German Navy with his speed indicators, and is now engaged in adapting them also to several English war vessels, notably H.M.S. *Northampton*, has decided to have the dials of his apparatus illuminated in this way, so as to enable seamen on the darkest night to read the index. Mr. Hollingshead, the enterprising manager of the Gaiety Theatre, is in treaty to secure the sole right to apply the paint in the production of theatrical effects; and it is probable that the process will soon come into conspicuous use as a medium for advertisements.

MEETINGS FOR THE WEEK.

MONDAY, 16th.—London Institution, 5.
— Medical, 8.30.
— Society of Arts, 8. "The Manufacture of India-rubber and Gutta-percha," by Thomas Bolas, F.C.S.
TUESDAY, 17th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
— Zoological, 8.30.
— Civil Engineers, 8.
— Society of Arts, 8. Mr. Alexander William Mitchinson, on "The Principal Causes of Disease in Tropical Countries, Scientifically Considered."
WEDNESDAY, 18th.—Society of Arts, 8. Mr. W. P. Andrew, "The Euphrates Valley Railway."
— Meteorological, 7.
THURSDAY, 19th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
— London Institution, 7.
— Royal, 8.30.
— Zoological, 4.
— Chemical, 8.
FRIDAY, 20th.—Royal Institution, 8. Rev. H. R. Haweis "Old Violins," 9.
— Geo.ogical, 1. (Anniversary.)
SATURDAY, 21st.—Royal Institution, 3. Prof. Pauer, "Joseph Haydn," (with Musical Illustrations.)

TO CORRESPONDENTS.

H. Wilson.—For luminous paint and articles painted with it, appl to Messrs. Ihlee and Horne, Aldermanbury.

THE JOURNAL OF SCIENCE

for February (Price 1s. 6d.), includes—

The London Water Supply.

On Water and Air. By Prof. Tyndall, D.C.L., LL.D., F.R.S. (Lecture II.) Illustrated.

Report on Hollway's Process for Reducing Metallic Sulphides without External Fuel. By W. Mattieu Williams, F.R.A.S. (Illustrated.)

London: 3, Horse-Shoe Court, Ludgate Hill.

TO SULPHATE OR MURIATE OF AMMONIA
MAKERS AND OTHERS.The Combustion of Sulphuretted Hydrogen,
and its manufacture into Oil of Vitriol.

This apparatus has been successfully working for a series of years at the Chemical Works, Frizinghall, nr. Bradford (Messrs. Hunt and Illingworth's), the perfecting of which was the result of an incalculable amount of study on the part of the proprietors.

It is necessary, for the thorough and efficient working of the Plant to adopt as well their improved Plant for making Sulphate or Muriate of Ammonia.

The undersigned are open to treat with any person wishful to adopt the process:—

The executors of the late Mr. Wm. Hunt, Messrs. HUNT and GORLE, at the Chemical Works Wednesbury, Staffordshire;

Or Mr. THOS. ILLINGWORTH, Ilkley, Yorkshire.

CORPORATION OF BIRMINGHAM.

GAS DEPARTMENT.

TENDERS FOR TAR.

The Gas Committee of the Corporation of Birmingham are prepared to receive Tenders for the Tar to be produced at their various Works for a period of one, three, or seven years, from the 30th of June next.

The quantities produced in 1879 were as follows:—

Saltley Works.. .. .	8500 tons.
Windsor Street Works	5000 "
Adderley Street "	2500 "
Swan Village "	3000 "

Total 19,000 tons.

The Committee will receive Tenders either at a fixed price per ton, or at a price to be calculated on a sliding scale, varying with the selling price of products.

The Corporation are prepared to let on lease a portion of their surplus land on which Tar Works could be erected.

Forms of tender may be obtained on application to me.

EDWIN SMITH,
Secretary

Old Square Birmingham.

Producers of Alum Cake are requested to furnish us with their prices per cwt. and per ton.—Sutton and Phillips, Works, Stowmarket, Suffolk.

FOR SALE, a small lot (fully 1 ton) Chloride Magnesium.—Address, Jas. W. Gardner, 137, Baronsbury Road, Islington, London, N.

RETORT CARBON.

About 40 Tons on Sale.—Further particulars on application to Mr. Geo. B. Jackson, Gas Dept., Town Hall, Manchester.

Stearine Candle Maker Wanted for Abroad.

One who understands thoroughly the different processes, and is competent to refine and purify glycerine upon the most approved system.—Apply to George Bennet and Co., Oil and Chemical Engineers, Glasgow.

POLYTECHNIC.—MARY QUEEN OF

SCOTS.—The principal events in the life of the beautiful and unfortunate Mary Stuart, illustrated by Tableaux Vivants. The descriptive poem recited by Miss Alice Burnelle. The appropriate vocal music by a Choir of Glee Singers, under the direction of Mr. Stedman, daily at 3.30 and 8.30; Edison's Loud Speaking Telephone, &c., by Mr. Daniel, 10.30; the Zulu War and the Heliograph, by Mr. King, 11; London as it Was and Is, 11.30 and 8; the Electric Light, 12; Instrumental Concert by the Paggi Family, 2.30 and 7.30; Popular Optical Lecture, 3, by Mr. King: Phenomena of Light and the Ghost; by Mr. King, 6.30. Open 10 till 1, 2 till 5, and 6 till 10. Admission 1s. Reserved Seats, 2s., 1s., and 6d.

Water-Glass, or Soluble Silicates of Soda and Potash, in large or small quantities, and either solid or in solution, at ROBERT RUMNEY'S, Ardwick Chemical Works Manchester.

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Manufacturers of every description of Condensing, or High-pressure Blowing, Pumping, Rolling, Winding, and Patent Steam-engines, of any required power, for Irrigating, Draining, Mining, Rolling Mills, or Water Works purposes, and Manufacturers of every description of Chemical, Colliery, Copper Ore, Gold Mining, Glass, and Rock-salt Machinery.

Black-ash Revolving Furnaces of the Most Approved Type.

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Compressing-Engines for Collieries, Iron Works, and Weldon's Patent Bleaching-Powder Process.

Caustic, Chlorate, Decomposing, Nitre, Nitro-Glycerine, Iodine Potash, Sulphur, Acid, and Oxalic Pans.

Gas Producers Patent for Heating and other Purposes.

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THE CHEMICAL NEWS.

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THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Continued from page 72.)

THE two missing elements in the fifth series (third and fourth groups) should have very distinct properties. They will be found in this series between $Zn=65$ and $As=75$, and being atomanalogous with aluminium and silicium, they will be called, the one *eka-aluminium*, and the other *eka-silicium*. As they belong to an odd series they will give volatile metallo-organic compounds, as well as volatile anhydrous chlorides, but they will have much more acid properties than their analogues Eb and Ti of the fourth series have. The metals should be very easily obtained by reduction with carbon or sodium. Their sulphides will be insoluble in water, and Ea_2S_3 will be precipitated by sulphide of ammonium, whilst EsS_2 will be apparently soluble in it. The atomic weight of *eka-aluminium* will be about 68, and that of *eka-silicium* about 72. Their densities will be about 6.0 for Ea , and 5.5 for Es , and their volumes will be about 11.5 for Ea , and 13 for Es , as the volume of $Zn=9$, of $As=14$, and of $Se=18$. We obtain the same numbers when comparing (for Ea) the volumes of Al , In , and Tl ; and (for Es) the volumes of Si , Sn , and Pb , because the first-mentioned elements are the atomic analogues of Ea , and the last those of Es . The volume of $Si=11$, that of $Sn=16$; accordingly that of $Es=13$. In short, in the case now before us, we can find atomanalogues on all sides, and therefore it is possible to determine the properties more exactly than we could when examining Eb . These determinations result from the following atomanalogues:—*Eka-aluminium*, giving the oxide Ea_2O_3 , occupies the place between Ea and As on the one hand, and Al and In on the other hand; *eka-silicium*, giving the oxide EsO_2 , occupies the place between Ea and As on the one hand, and Si and Sn on the other hand. All these circumstances indicating a necessary analogy between Ea and Eb , as well as between Es and Ti , I am going only to submit Es to a comparative examination with Ti , so as to get a clear idea of the properties of these elements.

Eka-silicium will be extracted from EsO_2 , or from EsK_2F_6 , by the action of sodium; it will only decompose steam with great difficulty; it will hardly react on the acids, but will easily attack the alkalis. It will be a dirty grey metal, difficultly fusible, and will be turned by calcination into a finely-divided oxide, EsO_2 , which also will be very difficult to fuse. The density of the oxide will be about 4.7,* corresponding to the volume, which, judging approximately, according to the volumes of SiO_2 and SnO_2 , will be about 22. This oxide in its looks, and apparently in its crystalline forms, in its properties, and in its reactions, will resemble TiO_2 . The acid properties being feeble, although decided, as much in TiO_2 as in SnO_2 , Es will have the same character, and will form a more decided acid than TiO_2 . In this, as in similar cases, we have recourse to proportions. According to the proportion—

$$Es : Ti = Zn : Ca = As : V,$$

the basic properties should be more feebly shown in

EsO_2 than in TiO_2 , but more strongly than in SiO_2 . We can therefore safely predict a hydrate of EsO_2 , soluble in acids, although the solution will probably decompose easily, leaving an insoluble metahydrate. We can see that these properties correspond to those of TiO_2 . However, EsO_2 will be more easily separated from its acid solutions than TiO_2 is, but it will be more difficult to separate from its alkaline solutions. It is certain that EsO_2 will furnish a series of double fluorides, K_2EsF_6 , isomorphous with the corresponding salts of Si , Ti , Zr , and Sn ; the double salt of potassium will be more soluble than the corresponding salt of Si . Like TiF_4 , ZrF_4 , and SnF_4 , the fluoride of *eka-silicium* will not be gaseous, which will apparently result from polymeric changes, the same as for the other fluorides mentioned. The composition and size of the molecule of fluoride of silicium is SiF_4 , that of fluoride of titanium is apparently Ti_2F_8 , chloride of *eka-silicium*, $EsCl_4$, unlike the fluoride, will be a volatile liquid, boiling at 100° (or perhaps a little lower), as $SiCl_4$ boils at 57° , and $SnCl_4$ at 150° . We shall find by means of this property, a practical method of separating *eka-silicium* from Ti , seeing that $TiCl_4$ boils at 136° . In its other properties $EsCl_4$ will coincide with $TiCl_4$. The density of $EsCl_4$ will be about 1.9 (at 0°), its volume will be about 113, as the volume of $SiCl_4=112$, and that of $SnCl_4=115$ (the density of $FeCl_4=1.76$).

A marked difference between Es and Ti will consist in this, that Es , like Si and Sn , will be able to form metallo-organic compounds, such as $EsAe_4$, whilst Ti , belonging to an odd series in the system, will not form compounds of this nature. Judging from the properties of Sn and Si , $EsAe_4$ will boil at 160° , and its density will be about 0.96. This last-mentioned property will serve to separate Es from Ti , Zr , and Nb , if they happen to be present.

In my opinion it is in those numerous but insufficiently studied minerals, minerals with complex compositions, and containing Ti , Zr , and Nb , that we should look for Es , as well as for several other unknown elements. The great resemblance between the properties of Ti and Es allows us even to suppose that Es has escaped observation, although it was present in titaniferous minerals; the difference of the results, relative to the equivalent of titanium, leads to this supposition. To determine the atomic weight of Ti , Rose,* Pierre,† and Demoly‡ decomposed $TiCl_4$ by water, and having weighed the Cl present, admitting $Ag=108$ and $Cl=35.5$, they obtained the following results:—

Rose,	from 48.08 to 48.48 ;	average, 48.26
Pierre,	„ 50.34 „ 49.55 ;	„ 50.0
Demoly	„ 57.3 „ 55.9 ;	„ 56.8

It is difficult to attribute such a want of concordance to differences and errors in the processes of research of the different investigators; we should rather admit that the $TiCl_4$ contained foreign unsuspected matter. We should also believe, because of the periodic law, that the numbers found by Pierre and Demoly are too high; for the atomic weight of Ti should be a little higher than $Ca=40$, but a little lower than $V=51$ and $Cr=52$. The number therefore found by Rose seems to be the correct one, whilst the chloride of titanium used by the other workers must have contained impurities which raised the atomic weight, $EsCl_4$ being a chloride analogous to $TiCl_4$, and as Es should have a higher atomic weight than Ti (viz., 72), it seems to me that Es should be looked for in $TiCl_4$, which has been prepared from rutile coming from various localities. However, a trial that I made did not show me any body in $TiCl_4$ which would boil below 135° .

I would mention that on several occasions new elements have been supposed to have been discovered in the compounds of Ti , Nb , and Zr , and some researches lead us to think that Es might have been present in some cases. We know that Hermann believed that he had discovered

* Kokscharoff has described, under the name of *ilmenorutile*, a body analogous to rutile, but which has the density 4.8, whilst ordinary rutile has the density 4.2; 4.8 corresponds to the density of oxide of *eka-silicium*.

* Poggendorff's *Annalen*, vol. xv., p. 145.

† *Annales de Chimie et de Physique*, 3rd series, vol. xx., p. 257.

‡ *Ibid.*, vol. xxii., p. 213.

ilmenium, although Marignac* showed later on that the colombites, which had been suspected of containing ilmenic acid, in reality only contained a mixture of titanitic, niobic, and tantalic acids. To confirm Marignac's assertion—which was that Hermann's ilmenic acid was only a mixture of titanitic and niobic acids—we can say that the equivalent of ilmenium found by Hermann occupies the place between the equivalents of—

$$\text{Ti} \left(\text{I}_2 = \frac{48}{4} \right) \text{ and of Nb } \left(\text{I}_9 = \frac{94}{5} \right).$$

But Marignac himself has not succeeded in finding an exact method for the separation of titanitic from niobic acid: he himself admits that his process (based on the difference of solubility of the double fluorides of potassium) is insufficiently complete. We have, therefore, a perfect right to be sceptical of the exactness of his results, however reasonable they may seem, and I further hold that a repetition of his work would not be superfluous, more especially as Ti and Nb can be separated in the form of chlorides, compounds whose boiling-points show considerable differences.†

The examples that I have given show sufficiently in what manner we can, by means of the periodic law, foresee the properties of unknown elements; therefore I will not pursue the subject of describing the elements which are wanting in the system. The discoveries which would offer the most interest would be those of the following elements:—Eka-cæsium, Ec=175; dui-cæsium, Dc=220; eka-niobium, En=146; eka-tantalum, Et=235; and the analogues of manganese, for example:—Eka-manganese, Em=100; and tri-manganese, Tm=190.‡ For it is difficult to consider the absence of the whole of a series (the ninth), and nearly all a large period (from Ce=140) as an accidental circumstance: the reason of this gap is very probably caused by the nature of elements.

When we reflect on the problems which form the subject of this memoir another question presents itself:—Is the number of elements limited or unlimited? If we consider that the system of the known elements is limited, and, so to speak, closed; that the aërolites, the sun, and the stars contain only the same elements as the earth; that the acid properties disappear little by little as the atomic weight increases; and that the greater number of the elements with high atomic weights constitute the heavy and difficultly oxidised metals; if, I say, we consider all this to be case, we must admit that the number of accessible elements is very small; and we may presume that if several more heavy metals are found in the interior of the earth, their number and their quantity will be very limited.

A New Voltaic Condenser.—M. d'Arsonval.—The author, seeing the energetic, though brief, effects produced by secondary batteries, and in particular by that of M. Gaston Planté, sought for a condenser capable of storing up a much larger quantity of electricity. He forms a couple of a plate of zinc and a plate of coke, surrounded by lead in a very fine state of granulation immersed in a concentrated solution of sulphate of zinc. He also substitutes for the zinc a layer of mercury.—*Comptes Rendus*, tome xc., No. 4.

* *Archives des Sciences Naturelles*, January, 1866, and August, 1867.

† I would again draw attention to the fact that, judging from its atomic analogy with As and Sn, Es might be met with in the minerals which contain these last elements. Wöhler mentions (*Mineralanalyse*, par. 96) that when tungsten is treated with *aqua regia* and ammonia, an insoluble residue of niobic (up to 2 per cent) and silicic acids is left. The further study of niobic acid would be very interesting, as Wöhler's work on that subject was published at a time when this mineral was very little known.

‡ Perhaps Ru and Os occupy these places, if their highest oxides are like R_2O_7 and not RO_4 . However, if such were the case, the true analogues of Fe would be missing. A comparative study of OsO_4 and Mn_2O_7 is very desirable.

ON A NEW METHOD OF SPECTRUM OBSERVATION.*

By J. N. LOCKYER, F.R.S.

IN anticipation of my report on the Methods of Mapping Spectra, which I have been requested to prepare for the Solar Committee, I beg to present to them the following account of some recent work which has been suggested during the preparation of that report. In the *Phil. Trans.* for 1873† I gave an historical account, showing how, when a light source, such as a spark or an electric arc, is made to throw its image on the slit of a spectroscopic, the lines had been seen of different lengths; and I also showed, by means of photographs, how very definite these phenomena were. It was afterwards demonstrated that chemical combination or mechanical mixture gradually reduced the spectrum by subtracting the shortest lines and leaving only the long ones.

On the hypothesis that the elements were truly elementary, the explanation generally given and accepted was that the short lines were produced by a more complex vibration imparted to the "atom" in the region of greatest electrical excitement, and that these vibrations were obliterated or prevented from arising by cooling or admixture with dissimilar atoms.

Subsequent work, however, has shown‡ that of these short lines some are common to two or more spectra. These lines I have called basic. Among the short lines, then, we have some which are basic and some which are not.

The different behaviour of these basic lines seemed, therefore, to suggest that not all of the short lines of spectra were, in reality, true products of high temperature.

That some would be thus produced, and would therefore be common to two or more spectra, we could understand by appealing to Newton's rule, "*Causas rerum naturalium non plures admitti debere quam quæ et veræ sint et earum phaenomenis explicandis sufficient*," and imagining a higher dissociation. It became, however, necessary to see if the others would also be accounted for.

I have already given to the Royal Society a preliminary account of the extraordinary, because unexpected, phenomena and changes observed in the spectra of vapours of the elementary bodies when volatilised at different temperatures in vacuum tubes. Many of the lines thus seen alone, and of surpassing brilliancy, are those seen as short and faint in ordinary methods of observation, and the circumstances under which they are seen suggest, if we again appeal to the above rule, that many of them are produced by complex molecules.

In this case the appeal lies to the phenomena produced when organic bodies are distilled at varying temperatures, the simplest bodies in homologous series are those volatilised at the lowest temperatures; so that on subjecting a mixture of two or more liquids to distillation, at the beginning a large proportion of the more volatile body comes over, and so on.

At any particular heat-level, then, some of the short lines may be due to the vibrations of molecular groupings produced with difficulty with the temperature employed, while others represent the fading out of the vibrations of other molecular groupings produced on the first application of the heat.

In the line of reasoning which I advanced a year ago§ both these results are anticipated, and are easily explained. Slightly varying Fig. 2 of that paper, we may imagine furnace A to represent the temperature of the jar spark, B that of the Bunsen burner, and C a temperature lower than that of the Bunsen burner.

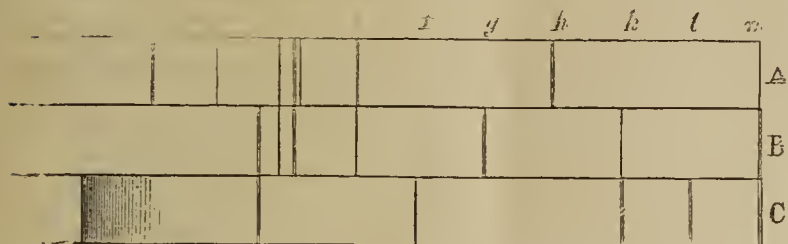
* A Paper communicated to the Royal Society at the request of the Committee on Solar Physics.

† *Phil. Trans.*, 1873, p. 254.

‡ *Proc. Roy. Soc.*, vol. xxviii., p. 159.

§ *Proc. Roy. Soc.*, vol. xxviii., p. 162.

Then in the light of the paper the lines *b* and *c* would be truly produced by the action of the highest temperature, *c* would be short and might be basic, while of the lines *h* and *m*, *m* would be short and could not be basic, because it is a remnant of the spectrum of a lower temperature.



A. Highest temperature. C. Lowest temperature.

To make this reasoning valid we must show then that the spark, or better still the arc, provides us with a summation of the spectra of various molecular groupings into which the solid metal which we use as poles is successively broken up by the action of temperature.

We are not limited to solid metals; we may use their salts. In this case it is shown in the paper before referred to* that in very many cases the spectrum is one much less rich in lines.

I have therefore attempted to gain new evidence in the required direction by adopting a method of work with a spark and a Bunsen flame, which Colonel Donnelly suggested I should use with a spark and an electric arc. This consists in volatilising those substances which give us flame spectra in a Bunsen flame and passing a strong spark through the flame, first during the process of volatilisation, and then after the temperature of the flame has produced all the simplification it is capable of producing.

The results have been very striking: the puzzles which a comparison of flame spectra and the Fraunhofer lines has set us find, I think, a solution, while the genesis of spectra is made much more clear.†

To take an instance, the flame spectrum of sodium gives us, as its brightest, a yellow line, which is also of marked importance in the solar spectrum. The flame spectra of lithium and potassium give us, as their brightest, lines in the red which have not any representatives among the Fraunhofer lines, although other lines seen with higher temperatures are present.

Whence arises this marked difference of behaviour? From the similarity of the flame spectrum to that of the sun in one case, and from the dissimilarity in the other, we may imagine that in the former case—that of sodium—we are dealing with a body easily broken up, while lithium and potassium are more resistant; in other words, in the case of sodium, and dealing only with lines recognised generally as sodium lines, the flame has done the work of dissociation as completely as the sun itself. Now it is easy to test this point, for if this be so then (1) the chief lines and flutings of sodium should be seen in the flame itself, and (2) the spark should pass through the vapour after complete volatilisation has been effected without any visible effect.

Observation and experiment have largely confirmed these predictions. Using two prisms of 60° and a high-power eyepiece to enfeeble the continuous spectrum of the densest vapour produced at a high temperature, the green lines, the flutings recorded by Roscoe and Schuster, and another coarser system of flutings, so far as I know not yet described, are beautifully seen. I say largely, and not completely, because the double red line and the lines in the blue have not yet been seen in the flame, either with one, two, or four prisms of 60°, though the lines are

seen during volatilisation if a spark be passed through the flame. Subsequent inquiry may perhaps show that this is due to the sharp boundary of the heated region, and to the fact that they represent the vibrations of molecular groupings more complex than those which give us the yellow and green lines. The visibility of the green lines, which are short in the flame, taken in connexion with the fact that they have been seen alone in a vacuum tube, is enough for my present purpose.

With regard to the second point, the passage from the heat-horizon of the flame to that of the spark, after volatilisation is complete, produces no visible effect, indicating that in all probability the effects heretofore ascribed to quantity have been due to the presence of the molecular groupings of greater complexity. The more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen.

Let us now turn to lithium.

Seeing that the red line is absent while the violet lithium line is strong among the Fraunhofer lines, we may imagine that the flame has not done the work of dissociation in the case of lithium so completely as the sun does it, so that (1) the other lines of lithium should not be visible, even with the new precautions, in the flame spectrum, and (2) a passage from the heat-level of the flame to that of the spark after volatilisation should produce the other lines which we know to exist in the spectrum of the metal in the orange, blue, and violet.

Experiment and observation have also confirmed this result so far as the yellow and blue lines go; that in the violet is difficult of observation.*

We next come to potassium.

The potassium lines usually recorded as not seen in a flame, but which are observed with a spark, are not very brilliant; nor are they strong among the Fraunhofer lines. Seeing therefore that a high temperature does not greatly develop them, we may expect to find them in the flame. They are almost all there when they are looked for with proper precautions, but those in all probability present in the sun are brightened on passing the spark, showing apparently that the flame volatilises with some difficulty the molecule which gives the line in the red.

The flame spectrum of magnesium perhaps presents us best with the beautiful effects produced by the passage from the lower to the higher heat-level, and shows the important bearing on solar physics of the results obtained by this new method of work.

In the flame the two least refrangible of the components of *b* are seen associated with a line less refrangible, so as to form a triplet. A series of flutings and a line in the blue are also seen.

On passing the spark, all these but the two components of *b* are abolished. We get the wide triplet replaced by a narrow one of the same form, the two lines of *b* being common to both (see Fig. 2).

When the line in the blue disappears on passing the spark two new lines are seen. The spark lines are in the sun, but the less refrangible member of the wide triplet and the blue line seen in the flame are absent.

The following are the details of some of the experiments which have been made on the above points:—

* The way in which the lines in the flame are unaffected by the spark strikingly reminds me of the following remarks of Angstrom and Thalén:—"The Fraunhofer lines can in general be divided, according to their appearance, into two classes; the one sharply defined and tolerably deep black, the other by no means so decidedly marked either as to form or colour. These two different kinds of lines are, as regards their appearance, very happily characterised by the opinion expressed on a former occasion, that the former, especially when the illumination is feeble, look as if they were situated considerably in front of the faint ground on which the latter seem to lie. The most prominent lines of the former class almost all proceed from iron: and those which remain, after the iron-lines are abstracted, belong to the other metals—calcium, manganese, chromium, &c."—"Angstrom and Thalén on the Fraunhofer Lines, together with a Diagram of the Violet Part of the Solar Spectrum." Upsala, 1866, p. 5.)

* Phil. Trans., 1873, p. 258.

† I allude more especially to the production of triplets, their change into quartets, and in all probability into flutings, to the vanishing of flutings into lines, by increasing the rate of dissociation.

Experiment No. I.—Two pieces of platinum wire were supported in a Bunsen flame at a distance from one another of about 3 millims. They were then connected with a Holtz machine, in order that the spark might be passed inside the flame.

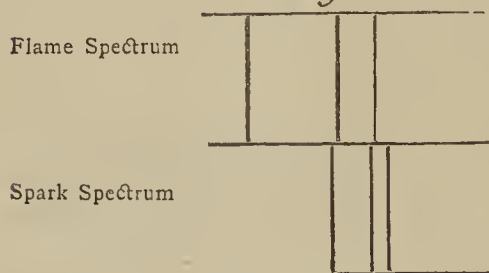
An image of the platinum was then thrown on the slit of the spectroscop by means of a lens. The spectroscop used had two dense flint prisms of 60° .

A piece of charcoal soaked in solution of sodic chloride was put into the base of the flame first, and then just below the platinum, and the spectrum observed; it consisted simply of the yellow line D. The spark was passed and the spectrum again observed; it now consisted of D plus the lines of hydrogen and some air lines, the red and green Na lines and the green flutings being still absent.

Experiment No. II.—Same arrangements, except that a large induction coil was substituted for the Holtz machine. The same results were obtained with the sodic chloride.

Experiment No. III.—Metallic sodium was next tried. It was found that when the metal was put into the flame just below the platinum the green line and the flutings were seen without the spark, that is, at the ordinary temperature of the flame. On introducing the sodium into the lower part of the flame, the green double ($\lambda 5687.2$ and 5681.4) and the flutings were not seen, either with or without the spark.

Fig. 2.



Experiment No. IV.—Same arrangements as No II., with metallic sodium, and with a small blowpipe instead of Bunsen.

In this experiment the flame spectrum showed, besides the yellow line (D), the green double ($\lambda 5687.2$ and 5681.4), and also the flutings in the green, those in the red being absent. As soon as the spark was passed, the green double ($\lambda 5687.2$ and 5681.4) became brighter, while the flutings vanished.

In these observations the sodium was put into the flame just below the platinum. When put into the bottom of the flame the D line was seen alone.

Experiment No. V.—A glass tube $\frac{1}{2}$ inch in diameter was prepared, about 6 inches in length, having two platinum sealed into it at a distance of 4 inches from each other. A bulb was blown at each end, so that the spectrum might be examined with the tube end-on. A piece of sodium was put into the tube, and the latter exhausted with a Sprengel pump. An Argand burner was placed at one end of the tube in order that the absorption of the vapour, as well as its radiation, might be observed. The metal was then very gradually heated by a Bunsen flame.

After the heating had gone on for about twenty minutes the absorption line of D appeared; this gradually increased in intensity.

On passing the spark along the tube, the bright lines of sodium appeared, the green double ($\lambda 5687.2$ and 5681.4), being distinguishable after D had been seen for a little time alone.

The temperature was now increased and the absorption spectrum again examined. The flutings in the green gradually made their appearance, D increasing in intensity, the green line being invisible. Afterwards the flutings in the red came in.

On passing the spark the absorption spectrum, consisting of the red and green flutings, disappeared instantaneously,

and the green double was seen very bright; after the passage of the spark D dark was much increased in breadth.

The quantity of hydrogen given off during the change prevented the passage of the spark, and the observations had to be discontinued. As soon as some of this had been pumped out the same observations were repeated with the same results.

Experiment No. VI.—An experiment was made with lithic chloride in Bunsen flame, with the same arrangement as in Experiment No. I.

The flame spectrum with the dispersion employed showed no Li line except the red one ($\lambda 6705.2$). On passing the spark from the Holtz machine, the yellow line ($\lambda 6102.0$) and the blue line ($\lambda 4602.7$) appeared as bright as the red line. The same results were obtained on repeating the experiment with the large induction coil.

Experiment No. VII.—Potassic nitrate was tried by the method previously described in Experiment No. I.

The flame spectrum consisted as usual of the red lines ($\lambda 7697$ and 7663) and the blue line ($\lambda 4045$), very faint.

The effect of the spark was to bring out the yellow lines (λ about 5800), those in the green (λ about 5340), and the red double ($\lambda 6946$ and 6913) out of the flutings visible in the red, the double at 7697 and 7663 not being affected. The experiment was repeated with the induction coil, and the same observations made, with the additional one that the spark also slightly intensified the blue line.

Experiment No. VIII.—On repeating the experiment with metallic potassium the same phenomena were more markedly observed, the lines about $\lambda 5800$, and other lines more refrangible, were visible as very faint objects in the flame; they were much strengthened, however, by the passage of the spark.

Experiment No. IX.—Some potassium was volatilised by the spark in front of the slit of the sun-spectroscop and comparison of the positions of the lines with the Fraunhofer lines made. It is believed that $\lambda 5829.0$, 5802.0 , 5782.5 are all reversed in the solar spectrum. The less refrangible member of the red double ($\lambda 6946$) was next compared, and was undoubtedly absent from the sun. These observations, however, are rendered extremely difficult on account of the fluted appearance of the yellow lines, and must be repeated with a stronger sun and the electric arc. The spectroscop employed had three prisms, one of 60° and two of 45° .

Experiment No. X.—The flame spectrum of magnesium was examined; a green triplet was observed, which was at first sight taken for b . Measurements, of the lines, however, showed that the less refrangible member was less refrangible than b , and had a wave-length 5209.8 , and that the other two members were b^1 and b^2 respectively. A fresh charge of magnesium was put into the flame and the spark passed; the original triplet was now no longer visible, the line at 5209.8 having vanished, but b^4 was now seen forming with b^1 and b^2 a triplet of similar form on a smaller scale.

According to Thalén, there are three blue lines of magnesium at wave-length 4481.0 , 4586.5 , and 4703.5 . These lines were looked for in the flame with and without the spark. Without the spark only one line was visible in this region; its position was found by comparison with the solar spectrum to be at wave-length 4570.3 , and coincident with no Fraunhofer line. The passage of the spark abolished this line, at the same time bringing in the two lines given by Thalén at wave-length 4481.0 and 4703.5 , both of which are reversed in the solar spectrum.

No line was seen at Thalén's wave-length 4586.5 , the nearest approach to which was the line seen at the temperature of the Bunsen flame at wave-length 4570.3 , a difference of more than sixteen divisions of the scale.

I am now preparing maps showing the phenomena observed at various heat-levels. I think it is not too much to hope that a careful study of such maps, showing the results already obtained, or to be obtained, at varying temperatures, controlled by observation of the condition

under which such changes are brought about, will, if we accept the idea that various *dissociations* of the molecules present in the solid are brought about by different stages of heat, and then reverse the process, enable us to determine the mode of evolution by which the molecules vibrating in the atmospheres of the hottest stars *associate* into those of which the solid metal is composed. I put this suggestion forward with the greater confidence, because I see that help can be got from various converging lines of work. To some of these I may briefly allude here:—

1. We have the lines present in the solar spectrum, and absent from it.

Example.—The red potassium line present in the flame is absent from the sun; some of the other lines are present.

2. We have the varying thicknesses of the lines of any one element in the sun to compare with the thicknesses produced at different temperatures in the laboratory.

Example.—The various lines of magnesium, notably *b*, the most refrangible line given by Thalén, and the other blue line.

3. We have the remarkable behaviour of metals vapourised in a vacuum at the lowest temperatures.

Example.—Sodium gives us D, potassium gives us the triplet in the green-yellow; calcium gives us the line in the blue; thus separating those lines from all the others of those metals.

4. We have the remarkable behaviour of the same vapours under like circumstances, the temperature alone being changed; when this is increased lines visible under ordinary conditions are brought in, and are seen in different parts of the tube, so that each line in turn (and therefore, I presume, each molecule which produces it) is separated from those with which it is generally seen in company.

Example.—By increasing the temperature we get the green line of sodium without D, and some of the magnesium lines have been seen separated from the others.

5. We have the power of determining the lower states by means of absorption phenomena, and then of observing the radiation of the vapours produced by the passage of a feeble current of electricity.

Example.—The fluted spectrum of sodium described by Roscoe and Schuster is instantly abolished by this means, and a brightening of the green and a considerable thickening of the dark yellow line is seen.

6. May we consider the existence of these molecular states as forming a true basis for Dalton's law of multiple proportions? If so, then the metals in different chemical combinations will exist in different molecular groupings, and we shall be able by spectrum observations to determine the particular heat-level to which the molecular complexity of the solid metal induced by chemical affinity corresponds.

Examples.—None of the lines of magnesium special to the flame spectrum are visible in the spectrum of the chloride either when a flame or a spark is employed. The facts recorded in my papers, printed in the *Philosophical Transactions* some years ago, on the spectra of salts and mixtures, seem all explained in this way.

I think, then, that the method of mapping, to be complete, should not only show the metallic lines as produced at various temperatures compared with the Fraunhofer ones, but that for each metal investigations should be made and recorded for as many heat-levels as possible, and for various chemical groupings such as—

CrO	Fe ₂ Cl ₆
CrO ₃	FeCl ₂
Cr ₂ O ₃	Fe
Cr,	

to give examples, with a view of investigating the facts, to see whether we can trace a molecular evolution in each case.

Further, the "basic" lines recorded by Thalén will re-

quire special study with a view to determine whether their existence in different spectra can be explained or not on the supposition that they represent the vibrations of forms, which, at an early stage of the planet's history, entered into combination with other forms, differing in proximate origin, to produce different "elements."

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

Wednesday, February 11, 1880.

GEORGE GODWIN, F.R.S., in the Chair.

"A New Metallic Compound," by GRANVILLE COLE, Ph.D. The paper which I am about to read this evening affords me the privilege of appearing for the first time before a meeting of the members of the Society of Arts, and I hope you will grant me your indulgence for any shortcomings which may attend this my first appearance as a public lecturer in England.

The subject of the paper is the discovery of a metallic compound, which I shall prove to you is new, and I shall endeavour further to set forth a few facts, attested by experiments, in respect of its nature. I am duly impressed with the very technical character of my subject, and the tendency there must be, in dealing with it, towards a certain dryness in my remarks. But, while I trust that many of the *data* will appeal with special interest to engineers and other practical men, I still hope to arrest your attention by showing you to how many artistic and industrial purposes this new metallic compound may be put. In the first place it will be right to give a brief account of the—

I.—Discovery of the Metal.

Nearly a year ago, Mr. J. Berger Spence discovered that the sulphides of the metals, combined with molten sulphur, formed a liquid. This liquid, on cooling, became a solid homogeneous mass, possessing great tenacity, and having a peculiar dark grey—almost black—colour. Nearly every metallic sulphide which is known, combines, as experiments have proved, with an excess of sulphur, and curiously enough, nearly all these combinations have the same properties. The combination, of which I have specimens here for inspection, consists of an ore of iron pyrites containing both lead and zinc sulphides.

II.—Chemical Composition.

Examining the metal from a chemical point of view, I may state, briefly, that it is a chemical compound belonging to that class known as thiates, or sulphur sulphides.

Dr. Hodgkinson, chemical demonstrator at the Science Schools, South Kensington, has kindly sent me the following facts. I cannot do better than quote his letter on the subject:—

"It appears to be the easiest thing in the world to obtain a homogeneous casting with it. Specific gravities of portion sent gave 3.3743 to 3.7036 (reduced to 0° C.).

"When finely powdered, it is acted upon slowly by concentrated HCl and NO₂HO in the cold; in large lumps, little or no action takes place. As yet I have not been able to determine the expansion equivalent accurately; it would appear, however, to be small. The fracture is not conchoidal, as might perhaps have been expected, but somewhat like that of cast-iron.

"I have not had time to try many 'utilisation experiments' on the substance, but I have no doubt it would be exceedingly useful in the laboratory, even, for instance, for making the air-tight connections between glass tubes by means of caoutchouc, and a water or mercury jacket, where rigidity is no disadvantage; the fusing-point is so convenient, about 340°, that it may be run into the outer

tube on to the caoutchouc, which it grips, on cooling, like a vice, and makes perfectly tight.

"I don't know what you may call the material, but should think, as it seems to be more than a mere mixture, 'ferric thiate' would not be a bad or inappropriate name."

III.—*Properties of the Metal.*

I propose, this evening, to illustrate, by experiments such as are possible at a lecture, some of the properties of the metal. I will begin by giving you a short summary of its peculiarities, and its advantages over those of other metals or metallic compounds.

1. It has a comparatively low melting-point, viz., 320° F., or rather more than 100 above the temperature of boiling water. Here, then, we have in its favour the small amount of fuel needful to supply the necessary heat for reducing the metal to a condition for use.

2. It expands on cooling, a property not shared by the majority of other metals or metallic compounds. I believe that type metal and bismuth are two exceptions. Later on, I think I shall convince you that, for an operation like the joining of gas and water pipes, this expanding property is one of great importance.

3. It claims to resist atmospheric or climatic influences, as compared with bronze and marble. I noticed only a few days ago how the statues in bronze, on the Holborn Viaduct, had been affected by our London fogs. And I have evidence to produce in the direction of showing the imperviousness of the metal to such degrading influences.

4. As compared with other metals or metallic compounds, its resistance to acids, used commercially, to alkalies, and to water, is certainly superior.

5. A smooth surface of this metal or metallic compound, now known commercially as Spence's metal, takes a very high polish. I will illustrate this to you by casting some of this molten metal on to a surface of glass.

IV.—*Its Uses to Art.*

It may, perhaps, be interesting to my hearers to learn the manner in which Mr. Spence first thought of utilising this metal for works of art. In order to obtain a perfectly smooth surface, he had been running molten metal on to a piece of glass as I have just done. But before doing so he had chanced to touch the glass, and had left the marks of the pores of the skin of his fingers upon it. On removing the metal, these marks were found to be reproduced, and so indelibly that they did not disappear on polishing the surface metal. This led Mr. Spence to try to cast the metal in a mould; and although at present no artistic work of high order has yet been reproduced, yet I venture to think that enough has been done to justify the expectation that, in a short time, standard works of both ancient and modern art may be successfully and usefully reproduced. Various colours, such as the green patina of bronze, the dark blue hue of steel, and the appearance of silver and gold, have already been obtained. Experiments are now in progress which give promise of enabling those who adopt this metallic compound for such uses to reproduce metallic works in their original colours.

I have here a casting that was made from a nickel-plate engraving; every line, however minute, has been reproduced in Spence's metal. Experiments are now being carried on to test the adaptability of the metal for printing and stereotyping purposes, but they are not complete, so I refrain from giving you any facts about them.

Besides this, experimental castings have been made of various medallions and busts; notably, this large bust of Her Majesty the Queen. The metal in this case has not subsequently been treated in any way beyond being polished with a cloth. The metal can be cast into almost any material used for moulds.

Mr. Spence has succeeded in obtaining casts from metal moulds, plaster moulds, and even from gelatine moulds. These last are probably the first metallic castings produced from gelatine moulds. Spence's metal being

almost a non-conductor of heat, cools so rapidly in the gelatine mould that it yields a perfect impression before the form of the mould is destroyed, and if the gelatine be allowed to remain on the metal till cold, it re-models itself ready for the next casting. It is, therefore, hoped that an additional process has been secured, by which the most undercut objects may be reproduced successfully and easily.

The advantages which Spence's metal possesses over other materials used for artistic productions, may be summarised under three heads, viz.:—1. Cheapness. 2. Facility of working. 3. Resistance to climatic influences.

Cheapness.—As compared with lead, which is one of the cheapest of metals, it is one-third the weight; and, whereas the average cost of lead for the last ten years has been nearly £18 a ton, Spence's metal only costs £15. A ton of Spence's metal being three times the amount in bulk of that of a ton of lead, it is available for three times the amount of work. It may, therefore, be considered to be nearly a quarter of the price of lead, and, consequently, very considerably less than that of bronze.

Facility of Working.—Its melting-point being very low, it can be very easily prepared for pouring into a mould, and its property of expanding, when cooling, causes it to take such a perfect impression, that the cast requires very little chasing after. In respect of a gelatine mould, which can cover a considerable surface of work without joints, such as one has to make in plaster-piece moulding, the metal cast obtained from such a mould would require no chasing whatever.

Resistance to the Atmosphere.—With regard to its resistance to climatic influences, experiments have been conducted in this direction with complete success. A polished surface of the metal has been exposed for six months in all weathers, without showing the least change.

Mr. Wood, the Secretary of this Society, has had a medallion, which I sent him a month ago, exposed to all the recent fogs and frosts, on the outside of this building. You can judge and see for yourselves how well it has stood this test.

Not to confine myself to his test alone, I have here another medallion of this metal, which Mr. Wood tells me has been left for the same period in *aqua regia*, one of the strongest acids known. You see how little effect the acid has had on the surface of the metal. I believe that no work of art in any other substance would bear this test without suffering. I will here endeavour to show you the effect the same acid has on marble or bronze. I venture to think that if Spence's metal has resisted this acid for a month, it ought certainly to be able to resist the climate of London for a very much longer period. I therefore beg to submit that this metal, if skilled labour is brought to bear on it, ought to be of a great value for decorative purposes, both internal and external.

V.—*Industrial Uses.*

I will endeavour now to point out to you the uses to which this metal may be applied for industrial purposes. And I propose to divide these purposes under three heads:—1. Gas and water works. 2. Chemical works. 3. Miscellaneous.

Gas and Water Works.—As practice is better than theory, I will simply relate, as best I can, those experiments which have been tried at the South Metropolitan Gas Works. Experiments, under the direction of Mr. Livesey, were made some weeks back. Two pipes were joined by this metal in much less time than would have been taken had lead been employed. The pipes, after having been joined, were tested under pressure, but no leakage was found.

Method of Joining Pipes with Lead.—In order to show the especial advantages this metal has over lead, it will be as well here, if I endeavour to tell you how gas or water pipes are joined when lead is used. The pipes having been laid together, the joint is packed with yarn; clay is then laid round the exterior of the joint, and the molten

lead is run in. Unfortunately, lead possesses the property of contracting on cooling. The leaden joint has, therefore, to be "caulked," or, as it is called in the north, staved. This caulking, or staving, means wedging the lead into the joint, in order to obtain a perfectly tight joint. This caulking naturally occupies considerable time, and necessitates excavation, in order to allow the men to work all round the pipes.

Excavation and caulking are both rendered unnecessary by the use of Spence's metal. The pipes have only to be laid together, and after the yarn has been forced into the joint and the clay placed, the liquid metal is run into the joint, the clay removed, and the joint is finished. The metal does not splash in running into the mould, thus avoiding a great source of waste of material, and danger to workmen.

Experiments were also tried to test the metal joints in the event of subsidence of the ground. Four lengths of nine feet piping, six inch diameter, were joined with the metal, and supported on tresles. After the metal had set, which it did in a few minutes, the centre supports were knocked away, leaving only the two end ones. The 36 feet length of piping sank 7 inches, without showing, even after pressure, a leakage.

These experiments were so satisfactory, that the South Metropolitan Company have adopted the metal, and are now laying their pipes with it. Mr. Livesey, the chief engineer, in writing on the subject to Mr. Spence, says:—"We have now only the test of time, and that, I think, we may take the risk of." Others of the London gas works, and a very large number of provincial gas works, are adopting it.

In the same way, it will be useful for water works. Mr. Hope, who undertook some experiments in Scotland, reports that at the Edinburgh Water Works two pipes were joined, and subjected to a pressure of 400 feet of water, which was as much as they could get on, without the joint showing any leakage. This is the greatest pressure which we have as yet been able to put it to; so what it will actually bear, I am not at present in a position to say.

From a sanitary point of view, as water has no action on the metal, it would be extremely valuable for cisterns, instead of iron or lead. Being almost a non-conductor of cold, pipes might be lined with it to prevent the water from freezing.

Uses to Chemical Works.—The metal being less acted upon by acids than other metals, it may also be of service to chemical manufacturers. I refer especially to sulphuric acid, which is the most extensively used of all acids in commerce. Lead has, up to the present, been used for sulphuric acid tanks. I have myself tested the metal with sulphuric acid, and its action is almost imperceptible. The one objection to the use of this metal in this case is its low fusing-point, but when acids have only to be used up to a certain temperature, say 200° F., I venture to predict a large field for its use.

VI.—Miscellaneous Applications.

Besides the uses I have thus briefly and, I am afraid, somewhat imperfectly set before you, there are many others to which the metal may be applied, for instance, joining iron to stone or wood, the tensile strain of the metal being from 650 lbs. to the square inch five minutes after setting. For joining railings to stone it would answer equally as well as lead, and cost very much less; also for coating the holds of ships. I have been told that an Act of Parliament has been passed by which builders are compelled, if the district surveyors desire, to cover the walls of houses after they are built two feet out of the ground, with some material to prevent the damp from rising. It seems to me that Spence's metal is peculiarly adapted for this purpose.

I will illustrate to you some of the other uses to which this metal may be put. For hermetically sealing bottles; for covering cloth; for covering parcels that are being sent out to hot climates, thus obviating the use of lined

boxes; for preserving fruit, or other articles of consumption; and I may state that experiments on a large scale are still being carried on at Mr. Spence's works, Belvedere, Kent.

VII.—Conclusion.

I feel sure we have not yet come to the end of all the uses this Spence's metal may be put to, but I trust that I have shown you sufficient to induce you and others who may be interested in this discovery to make further investigations. If my paper is somewhat shorter than is usual, it is to some extent owing to my hesitation to make any statement which either Mr. Spence or I have not verified by actual experiment ourselves. I have already shown you a few of them, viz., casting on glass, casting an engraving, casting in metal moulds, casting in plaster moulds, casting in gelatine, resistance to atmospheric action, resistance to acids, and resistance to acids as compared to bronze or marble, and uses in gas and water works. In conclusion, I express a hope that I have established the proposition with which I started, and I trust you are satisfied that this Spence's metal is a discovery which has a prospect of much utility to the fine and industrial arts, and that I have been justified in bringing it before the attention of the members of the Society.

DISCUSSION.

The CHAIRMAN said that Dr. Cole had certainly proved that there was in one way or other a very large field for the employment of this new metal. It was very curious to see how an invention first made with a particular object sometimes took afterwards an entirely different direction. This material was first brought forward as affording admirable reproductions of works of art, and now they were informed that it could be used to preserve fruit, and possibly meat, and so it might solve the difficulty of bringing over American and Australian meat. This reminded him of an admirable invention for wood-carving, which made no way at first, but when troubles overtook France, and prevented the exportation of wooden heels for ladies' shoes, all the machines in London were immediately set to work profitably. So with Mr. Ransome's patent stone: it did not seem to take until it was turned to making large grindstones, for which it was admirably adapted. The same thing might occur in this case. This metal was already in profitable use for joining gas- and water-pipes, where it possessed considerable advantages, not merely because lead required wedging in order to make a tight joint, but very often from the difficulty of getting at the under side of the joint, it was imperfectly accomplished; whereas this metal, from its fluidity, penetrated all round, and so made a good piece of work of it. It must not be too readily taken for granted that its power of resistance was proved; because, though it had been exposed for seven or eight months, that was really nothing in point of time: they had bronzes thousands of years old which were still perfect, though others, imperfectly made, such as some in that metropolis, which were not twenty years old, were already decaying. The last time he had looked at the paws of the noble lions in Trafalgar Square he noticed they were full of holes; and if some means were not devised for preventing further decay their ruin was certain. However, what they had already heard said a good deal for the durability of this metal. It was quite certain they needed a material for the reproduction of works of art which should be cheap, hard, and durable. Sulphur itself had considerable power of resisting atmospheric influence, and many other reasons might be suggested why this compound might be expected to be durable, and if so, it would be a great boon for artistic purposes; but there were many other directions in which it would be useful, as, for instance, the joining of iron railings to stone copings. Probably owing to galvanic action, lead, which was generally used, had a very prejudicial effect on the stone. After the popular outbreak in Hyde Park, which led to new railings being

put up, he noticed, within a year, that at every ten or twelve feet, where the iron bars were thicker than usual, the piece of stone had been driven forcibly out of the kerb, which entailed a large expense in the repairs. Lead poisoning was another thing which naturally occurred to one, and if this could be used as a coating for pipes, or for the material of pipes, if not too brittle, it would have the double advantage of preventing the decomposition of the lead and also the possibility of freezing.

The SECRETARY narrated the tests to which he had subjected the specimens sent him by Dr. Cole. When Dr. Cole first mentioned the compound to him he said it was not liable to be affected by acids, and he (Mr. Wood) then suggested that if a couple of specimens were sent to him he would himself test them, and relate the result to the meeting. One of these medallions he put on the roof of the Society's house about a month ago, and as they all knew, during the intervening period, they had had every possible description of bad weather. The time was not long, but if the material had been at all subject to atmospheric influences it could hardly have failed to show some signs; at any rate the glassy surface would have been somewhat dimmed, but nothing of the kind was apparent. The other medallion he put into a basin with nitric and hydrochloric acid, about the strongest mixture of acids known to chemists. The surface of this had certainly been affected, and probably there were no substances, except gutta-percha or earthenware, which would have escaped. An iron hook, by which the medallion was intended to be hung, dissolved entirely in a few days. Dr. Cole gave him a piece of the compound, with which he had experimented, and he found it exceedingly easy to work. You could take casts as easily as with wax. The only thing necessary was to get the right temperature. At anything under about 320° it was pasty, and if you went much beyond that temperature it became pasty again, but between 320° and 350° it was perfectly liquid, and could be poured into the very finest mould. He thought it rather a pity that a better name had not been devised for the material, as it was certainly not a metal.

Mr. LIVESEY said he had had some experience in regard to joining gas-pipes, and he would state the result. Lead was most unsuitable for this purpose in some respects, though it was universally used. It was expensive; it required caulking or setting up to make it tight, because it contracted on cooling; and if you wanted to remove the pipes—which constantly happened, except in the very largest mains—from the increased consumption requiring larger pipes, the use of lead prevented your getting out the pipes without cutting them, which occasioned considerable loss. Various means had been tried for drawing pipes, but all had failed, and they always had to be cut. Another drawback was that lead, being valuable and easily disposed of, was constantly being stolen. Now this new metal was free from all these disadvantages. The only thing which occurred to him, while listening to the paper, was that it seemed a shame to put so beautiful a substance to so base a use; for when he looked at those beautiful busts, it seemed a pity to use the same material for joining gas-mains. However, it had many advantages for his purpose, not the least of which was its perfect soundness. The account given by Dr. Cole was quite accurate. The four pipes jointed in the Old Kent Road, as described, and supported at the two ends, were exposed to the most severe test, and when he purposely broke one of the joints at the conclusion of the experiment to see what the effect was, he found the metal had taken a perfect impression of the cast-iron. The South Metropolitan Gas Company were now using it instead of lead: the men had no difficulty in applying it, and the only test required was that of time, of which he had no fear. The only possible difficulty was that workmen, being accustomed to use a fierce fire to melt the lead, might make the metal too hot, and so burn the sulphur; but that could easily be guarded against. If required, it could be easily cut out with a chisel in order to remove the pipes; and another advan-

tage was, that if from any cause it did not run quite round a large pipe and make a perfect joint, you could clean out the part which had not been filled, put a fresh piece of clay there, and fill it up with a little more metal, and thus the joint was made quite sound.

Mr. CRESSWELL wished to ask one or two questions, not with a view to throw a doubt on the utility of the invention, but rather to elicit valuable information. And, first, he wished to compliment Dr. Cole on his paper, which had the rare merit, not only of brevity, but of being written in language so plain that any ordinary person could understand every word. Though brief and simple, it contained matter of considerable importance to the commercial interests of the country, because, if a substitute for lead had been discovered, it would be an immense advantage to everyone, except, perhaps, those interested in lead mines. If, on the other hand, it would serve the purposes of art, he could not conceive a greater boon to the large class of struggling population, who were endeavouring, both male and female, to earn a living by the pursuit of art. He wished to know, in the first place, if this material, if used for jointing pipes, possessed any elasticity; because he was told that it was absolutely essential to the permanence of gas-pipes under the London streets, with heavy traffic passing over it continually, that there should be a certain give and take, or elasticity, in the joints. If, when heavy pressure was brought to bear on these pipes, they gave way at the joint, the value of the joint was reduced to *nil*. In the ordinary way, you first caulked the pipe, then filled it with lead, and thus produced an elastic joint, which would yield to such an extent that you might bend the pipe without destroying the continuity of the joint. If, on the other hand, this metal was so brittle that under any sudden pressure it gave way it would be a formidable difficulty. The next question was this:—He understood there was a project for bringing salt-water to London from the sea, and one of the greatest difficulties, as he was informed, which the promoters would have to deal with was to discover a joint for the pipes which would not be affected by salt-water. If any means could be devised for cementing pipes which would allow of salt-water being conveyed from Worthing to London, and remaining in the pipes for forty-eight hours or more, or even permanently, it would relieve the promoters of this enterprise of a great difficulty, and perhaps confer a boon on the inhabitants of the metropolis.

Captain HEATHORN asked if there were any galvanic action between this metal and copper, and also if it had been left for some time at the bottom of the sea to see if barnacles and such things would adhere to it?

Mr. WALLER asked if it would expand and contract in the same proportion as water-pipes if used for hot water?

Mr. SPENCE said the joints would be considerably affected by hot water if the temperature was very high. Ordinary boiling water would not affect it; but, as it melted at about 300° , if there were a steam pressure of about 40 lbs. there would be such an increase of temperature that the joints would not stand. It did possess a certain degree of elasticity. In one case 30 ft. of pipes were connected with this metal at the joints, and deflected to the extent of 8 inches, and when the weight was removed the pipes recovered their original position, showing that it was elastic to some extent. With regard to expansion and contraction he was hardly yet in a position to speak. He would take this opportunity of stating that if ever this metal, as it was called, did take a place in art or industry, much of its success would be due to Dr. Cole, who had greatly assisted him in his experiments and in elucidating the facts; and also to Mr. Livesey, who was the first to take it up in a practical way.

Mr. LIVESEY said lead was certainly not elastic. If you were to join four lengths of 9 feet together with it, caulking it up in the ordinary way, and then knock out the central supports, the upper part of the joints would squeeze into a narrower compass, and the lower would be left open,

and when the pipes were again brought to a straight line the lead would not return to its original shape. Lead was not elastic in any sense whatever, and it was, therefore, a very bad thing for pipes where there was any settlement or change of position. This metal seemed to have the effect of making the four lengths into one continuous perfect pipe, which had just the same strength in the joints as anywhere else. This was shown by the fact that when supported at the two ends it sagged evenly throughout, making a continuous curve; whereas if the joints had been made with lead, there would have been a number of straight lines, with angles at the joints.

Dr. COLE said he believed that salt-water would have no action on the metal whatever. The same with regard to galvanic action, for which it had been tested. With regard to barnacles at the bottom of the sea, he regretted to say that no experiments had yet been made in that direction. He would conclude by another experiment, to show how quickly the metal set, namely, coating a pat of butter with it.

Whilst this experiment was being performed,

The CHAIRMAN proposed a cordial vote of thanks to Dr. Cole, which was carried unanimously.—*Journal of the Society of Arts.*

PHYSICAL SOCIETY.

Annual General Meeting, February 14, 1880.

Prof. W. G. ADAMS in the Chair.

THE PRESIDENT read the report for the past year, which showed that the position and prospects of the Society are in every way satisfactory, and that more papers were communicated during last year than in any previous year. The following list of Council and Officers was elected for the ensuing year, and votes of thank. were given to the President, the Lords of the Committee of Council on Education, and to the Treasurer, Demonstrator, and Secretaries:—President, Sir W. Thomson, LL.D., F.R.S. Vice-Presidents, Prof. W. G. Adams, M.A., F.R.S. (who has filled the office of President); Prof. R. B. Clifton; Dr. Huggins; Lord Rayleigh; Dr. Spottiswoode. Secretaries, Prof. Reinold and Mr. W. Chandler Roberts, F.R.S. Treasurer, Dr. Atkinson. Demonstrator, Prof. Guthrie. Members of Council, Captain Abney; Walter Bailey, M.A.; J. H. Cotterill, F.R.S.; Dr. Warren de la Rue; Major Festing, R.E.; Prof. G. C. Foster; Prof. Fuller; Dr. J. Hopkinson; Dr. Schuster; G. Johnstone Stoney, F.R.S. Honorary Member, J. E. R. Clausius.

After this business the meeting resolved itself into an ordinary one, and the following new Members were elected:—Senor Roig y Torres, of Barcelona, Mr. Molli-son, Mr. Hare, Mr. J. C. Lewis, Miss Carolina Martineau.

A paper, "On a Quartz and Iceland Spar Spectroscope Corrected for Chromatic Aberration," was then read by Dr. W. H. STONE. The spectroscope consists of two Iceland spar prisms and a quartz train. It differs in no respect from those ordinarily made, except in the fact that the object-glasses of the telescope and collimator are doublets, with a positive lens of quartz and a negative of Iceland spar. The latter has a dispersive power so far greater than that of quartz that an approximation to achromatism may be easily obtained. In a spectrum there is less fear of indistinctness from superposition of images than in a telescope; but a greater amount of focussing is required with unachromatic lenses, insomuch that lines in the field at one time need alteration to obtain distinctness. Moreover, it is an obvious advantage to transmit the whole of the rays coming from the collimator as nearly as possible parallel through the intra-objective space and the prisms. The object-glasses were made by Mr. Ahreas about four years ago, and sent to Prof. McLeod. They were put aside, but have been recently re-mounted owing to M. Cornu having recently published a similar device.

A paper, "On an Automatic Switch for Telephone Circuits," was then read by Dr. WYNNE. The object of the switch was to enable any client of a telephone exchange to communicate with any other through the central office without the need of an assistant at the office.

Mr. VARLEY and Prof. AYRTON criticised the device, and the latter thought that the contacts might not be always reliable.

Profs. AYRTON and PERRY then read a note on their "Theory of Terrestrial Magnetism." Prof. Rowland, of Baltimore, had pointed out an error in their calculation which vitiated their results, and they therefore admitted that the charge of statical electricity on the surface of the earth assumed by them as competent to account for the earth's magnetism was not sufficient to account for the whole but only a portion of that magnetism. Nevertheless, they thought that the changes in the distribution of such a charge, due to changes in the condition of the dielectric medium between the earth and the sun, might account for the observed perturbations in the magnetic elements.

CORRESPONDENCE.

CUPRIC TEST-PELLETS.

To the Editor of the Chemical News.

SIR,—In regard to the account of cupric test-pellets for sugar (CHEMICAL NEWS, vol. xli., p. 63) it seems to me that Dr. Pavy is not aware that the difficulty of preserving Fehling's solution is entirely overcome by keeping the cupric sulphate and alkaline tartrate in solution in separate bottles. Thus, instead of, for instance, dissolving the required quantity of cupric sulphate, then adding the necessary amounts of Rochelle salt and caustic potash or soda, and making up to *one litre*, the same quantity of cupric sulphate is dissolved and made up to half a litre, and the corresponding quantities of Rochelle salt and caustic potash are also dissolved to half a litre, and the two solutions kept in separate bottles. When required, 10 c.c. (say) are withdrawn from each bottle and mixed, when 20 c.c. of Fehling's solution is formed. When so used the strength suffers no deterioration whatever. I have used it in this manner for some time past with accurate results. As to the stopper of the alkaline tartrate becoming a little fixed, this is no more serious than what occurs every day with a bottle containing caustic soda, and can, if desired, be avoided by using caustic potash instead of caustic soda in the preparation of Fehling's solution.

As to Dr. Pavy's pellets, I am of opinion that their application quantitatively would be attended with serious difficulties, such as loss of weight by friction, &c.—I am, &c.,

W. O. GLASSFORD.

The Brewery, St. Helens,
February 10, 1880.

TESTING VITRIOL CHAMBER EXITS.

To the Editor of the Chemical News.

SIR,—I was astonished to see in your issue of Feb. 6 letters from Messrs. G. E. Davis and W. J. Lovett, which imply that I had pirated from Mr. Davis the idea contained in my letter on the "Testing of Vitriol Chamber Exits" (CHEMICAL NEWS, vol. xli., p. 47). I thought that I had given Mr. Davis every credit due to him. He will not have forgotten that when on one of his official visits to these works in May, 1879, I mentioned to him, as a new process, the method for estimating the N compounds by the mercury tube or nitrometer, which I had just begun to use, he invited me to read a paper on it before the Faraday Club. This would of itself have led me to think that the method was new if I had not thought so before. The

twenty-two and a half hours aspiration of exit-gases which I performed for Mr. Davis I understood to be for ascertaining the persistency of H_2O_2 in dilute aqueous solution.

In answer to Mr. Lovett's letter I am obliged to say that the only part of it to which I cannot take emphatic exception is the conclusion, where he is good enough to copy from my letter my opinion, and reasons for that opinion, as to the inaccuracy of the indirect method of determining the N compounds. In the first place, Mr. Lovett was not present at the meeting of the Faraday Club at which the paper of Mr. Davis referred to was read. Then it is extremely surprising that, notwithstanding his connection with Mr. Davis, and therefore presumably long previous acquaintance with the nitrometer method, he should have continued to use, at the Victoria Chemical Works, the old inaccurate process long after he succeeded me there in May, 1879, and that when he first introduced the method I have described the results were so incorrect as I understood at the time they were. I must therefore call upon Mr. Lovett or the works' chemist to say *when* the nitrometer method was introduced by Mr. Lovett to the laboratory of the Victoria Chemical Works; how the process was at first worked so as to give unreliable results; and how it came ultimately to be worked correctly.—I am, &c.,

T. E. VASEY.

Church Street Chemical Works,
Hunslet, Leeds, Feb. 10, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 4, January 26, 1880.

Levulosate of Lime.—E. Peligot.—The products resulting from the action of alkalies upon levulose are those obtained by the action of the same bodies upon the glucose obtained from the saccharification of starch. They are the more complex, the more the air intervenes in the successive transformations.

Notes on the Acids formed on Re-distilling the Crude Fatty Acids in a Current of Superheated Steam.—A. Cahours and E. Demarçay.—In this process there are formed the different terms of the fatty series from the acetic to the caprylic acids inclusively. The authors hope to demonstrate the presence of the higher members, such as the pelargonic and capric. Acids of the succinic series appear also to be produced.

Photography of the Ultra-red Portion of the Solar Spectrum.—M. Abney.—A description of a photograph laid before the Academy.

Density of Chlorine at High Temperatures.—J. M. Crafts.—The author criticises the researches of V. and C. Meyer. The only experiment of theirs which he has repeated under the same conditions has yielded a result analogous to theirs. He agrees with them that at the highest temperature of the Perrot furnace, iodine decreases in density and augments in volume in the proportion of 1 to 1.5 as compared with air.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin. No. 17, 1879.

Amido-Acids with Alcohol Derivatives.—P. Griess.—This paper is devoted to a consideration of trimethylated sulphanilic acid.

Action of Methyl-iodide upon Asparagin.—P. Griess.—The author has obtained a new acid, $C_4H_5NO_3$, and has examined some of its salts.

Action of Cyanogen Compounds upon Diazobenzol.—P. Griess.—Finding that H. Gabriel is engaged with the study of this action, the author desists from the further prosecution of his researches save as regards the compounds formed by diazobenzol with hydroferrieyanic acid and hydro-nitro-prussic acid.

Action of Diastase upon Starch Paste.—A. Herzfeld.—We have here a special description of the final products of this reaction, maltose and achroodextrin. The author has acetylated erythro-, achroo-, and malto-dextrin, as also cane- and milk-sugar, maltose, and dextrose.

Atomic Weight of Antimony.—J. P. Cooke.—A reply to H. Kessler.

The Constitution of Anthraquinon.—H. v. Pechmann.—This paper contains an account of ortho-brom-phthalic acid, of ortho-brom-benzoyl-benzoic acid, ortho-brom-anthraquinon, and erythroxy-anthraquinon.

The Spontaneous Decomposition of Dichlor-ethyl-amin.—J. Tscherniak.—The author adheres to his former assertion that perfectly pure dichlor-ethylamin can be preserved undecomposed for an indefinite time.

The Luminous Nature of Phosphorus.—W. Müller-Erbach.—The author, as against Joubert, claims a priority in the announcement that the luminosity of phosphorus is entirely due to the combustion of its vapour.

The Reduction of Metallic Oxides by Hydrogen.—W. Müller-Erbach.—Referring to a paper by Messrs. Alder, Wright, and P. Luff (*Four. Chem. Soc.*, for 1878, xxxiii., Part 1), the author points out that he published a paper on this subject in 1869 (*Poggendorff*, 136, p. 64), containing results similar to those subsequently obtained by the above-mentioned chemists.

Reduction of Carbonic Acid by Phosphorus at Common Temperatures.—A. R. Leeds.—The author in correction of a former paper, remarks that in his experiments, large quantities of phosphoretted hydrogen gas were formed, the reducing powers of which were overlooked. He has not succeeded in confirming the reduction of carbonic acid to carbonic oxide.

An Exhaustive Chlorination of Iso-dinaphthyl.—Watson Smith.—Not suitable for abstraction.

The Relations between the Physical Properties of Organic Bodies and their Chemical Constitution.—J. W. Brühl.—An extract from the author's paper in *Liebig's Annalen*.

Reimann's Färber Zeitung,
No. 2, 1880.

Storck and Strobel employ the sulphocyanides as resist for aniline-blacks. They destroy the oxides of chlorine, by whose action upon the salts of aniline the black colour is produced. Hence, wherever the sulphocyanides are printed on, the formation of aniline-black becomes impossible.

MISCELLANEOUS.

The Academy of Sciences.—An extraordinary prize of 3000 francs has been awarded by the French *Académie des Sciences* to Mr. Crookes, F.R.S., in recognition of his recent discoveries in Molecular Physics and Radiant Matter.

NOTES AND QUERIES.

Sweep Smelting.—Can any of your correspondents tell me of a good book on this subject.—J. L. CLARKE, Philadelphia.

Solvent Power of Gases.—Kindly tell me where I can find a good account of Hannay's (or other) researches on the solvent power for solids possessed by gases when under pressure and heated.—G. A. KEYWORTH.

Schaffner's Glass Reaction Wheel.—If the enquirer will communicate with Mawson and Swan, Newcastle-on-Tyne, his requirements will be attended to.

THE CHEMICAL NEWS.

VOL. XLI. No. 1057.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Continued from page 84.)

V. ON THE USE OF THE PERIODIC LAW FOR THE CORRECTION OF ATOMIC WEIGHTS.

EVERYBODY knows the fate of Prout's hypothesis. Careful observations having proved that the numbers of atomic weights can contain fractions—Stas having shown that definite fractions cannot be admitted—we can no longer entertain any doubts, in spite of Marignac's brilliant opposition, but that Prout's hypothesis overstepped the facts. I cannot even see how any reasonable argument can support it. Let us admit that the matter of all the elements is completely homogeneous: there is no reason why we should also admit, that n ponderable particles, or n atoms of an element, in changing into one atom of a second element, will give the same n ponderable particles, or that the atom of the second element will be n times as heavy as the atom of the first. We can consider the law of the invariability of weights as a special case of the law of the invariability of force or of motion. The cause of weight is evidently a particular kind of movement of matter, and there is no reason for denying the possibility of transforming these movements into chemical energy, or into some other form of movement. Two phenomena which we can now notice in the elements, viz., the invariability and the indestructibility of the atomic weights, are, up to the present, in intimate and even historical relation. If, then, a known element was to decompose itself, or if it was to change into another one, these phenomena might be accompanied by a diminution or an increase in weight. In this manner can be explained, up to a certain point, the difference of the chemical energy in different elements. In expressing this idea here I wish to say *only* that there is some possibility of the opinions of chemists on the composed nature of elements becoming uniform without adopting Prout's hypothesis.

From a practical point of view, Prout's hypothesis is defective, in so far as it concerns directly, small numbers. Our usual determinations of atomic weights do not often agree to a fiftieth part with the whole number, often even to nearly five or six units, whilst in Prout's hypothesis it is only a question of tenths. Thus, as we have already seen, the different determinations of the atomic weight of titanium vary from 57 to 48.

But from a theoretical as well as from a practical point of view, it is very important to have a principle which will lead to the discovery of the great errors which are sometimes made in the determination of atomic weights, and to have an idea of greater numbers. If in these last we discover a true regularity, it is certain that eventually we shall discover such a regularity that will enable to determine theoretically, and with great precision, the magnitude of atomic weights. The periodic law, even in its present state, enables us to discover moderate errors in the determinations of atomic weights, as we have seen in the case of titanium. We are now going to give examples of a more rigorous nature.

To correct atomic weights exactly it is necessary to submit the individual properties of elements to an exact and comparative examination, for it is these properties which cause the perturbations in the regular variations of the amount of the atomic weights. Already, in the preceding pages, I have noticed the evident existence of such

properties. The absence of an exact regularity in the variations of atomic weights arises from two comparisons.

It arises, first, from the fact that the differences in the magnitudes of atomic weights of corresponding members of different series and different groups are far from agreeing, even if we take into account the admissible errors of determination. Thus, the difference between the atomic weights of Na and Li, or, for the sake of brevity, $\text{Na}-\text{Li}=16$, as also $\text{K}-\text{Na}=16$, but $\text{Mg}-\text{Be}=14.6$; on the other hand, $\text{Ti}-\text{Si}=20$, $\text{V}-\text{P}=20$, $\text{Pt}-\text{Pd}=91$, $\text{Au}-\text{Ag}=89$, $\text{Hg}-\text{Cd}=88$, $\text{Pb}-\text{Sn}=89$, $\text{Bi}-\text{Sb}=86$. It is difficult to admit that the gradual diminution of the differences is only a matter of chance. We ought rather to think that the relation between Pt and Pd is not altogether the same as that between Bi and Sb, and therefore the differences cannot be the same. We might notice that the properties of Bi and of Sb differ by a certain number of individual characters, whose origin arises in the slight individual separations of their atomic weights.

A second proof of the fact that periodic differences are in reality irregular, is found in the precise determinations of the atomic weights of the alkaline metals and the haloids, which were done by Stas.

If H = 1 and O = 15.96		
Li = 7.004	} difference	15.976
Na = 22.980		
K = 39.040		16.06
Cl = 35.368	}	
Br = 79.750		44.382
I = 126.533		46.783

We must see that there is a strict comparison in the differences between the values of the atomic weights of elements that correspond in the system, but we also notice by the side of these comparisons individual separations. Therefore the elements possess general properties, dependent periodically on their atomic weights (for example, the property of given determinate forms of oxidations, which are modified spontaneously and gradually), and individual properties which are determined by the separations mentioned above.

We do not really know anything of the just-mentioned relation, except the periodic property; and even this one is not properly understood. It is therefore impossible to determine exactly the amount of the separations, any more than we can correct positively the magnitude of atomic weights. We can only determine limits, certainly very near together, between which the amount of the atomic weight of a given element should be.

According to the periodic law, the atomic weight of tellurium should be greater than that of $\text{Sb}=122$, and less than that of $\text{I}=127$; that is to say that the atomic weight of tellurium ought to be about 125, because from every point of view, atomic analogies assign it a place between Sb and I. Considering that $\text{Ag}-\text{Cu}=45$, that $\text{Cd}-\text{Zn}=47$, that $\text{Sb}-\text{As}=47$, that $\text{I}-\text{Br}=47$, that $\text{Cs}-\text{Rb}=48$, that $\text{Ba}-\text{Sr}=50$, we are led to presume that in the same manner $\text{Te}-\text{Se}=47$ (nearly), for Te is found between Sb and I, as Se is between As and Br; and as Se, whose atomic weight is 78, has been more completely studied, and can be more easily purified than Te, the number found for Se should be accepted before the atomic weight found for Te; this is why this latter should be about $78+47=125$. This result does not agree with the numbers last given by Berzélius (who determined the quantity of TeO_2 obtained by the oxidation of Te by means of nitric acid).

O = 15.96,	O = 16.0
Te = 128.13,	Te = 128.45
= 127.97,	= 128.28
= 127.96,	= 128.28

Hauer (who estimated the bromine of K_2TeBr_6 as AgBr) obtained corresponding numbers; still, Te is generally taken to be = 128. Further, as the first determinations, made by Berzélius (1812), gave the number 116, as the later experiments have given the number 129, the exacti-

tude of the actually recognised atomic weight gives rise to certain doubts, which can only be eliminated by fresh experiments. It is difficult to purify the compounds of tellurium, and even to be certain when they are pure. This may perhaps explain in a measure the errors of the numbers which have been found. It is difficult to admit that the distinctive individual characteristics of tellurium could determine a gap relatively so great (128 to 125) compared with the number of its atomic weight, as it is deduced from the periodic law. Fresh experiments are therefore necessary to show us to what degree the periodic law can be relied upon in the correction of atomic weights.

Os, Ir, and, perhaps, also, Pt and Au, furnish us with a second example of gaps of this nature. These elements are found in the same series of the system (see Chapter I.); they are preceded by W=184, and followed by Hg=200. Their atomic weights are effectively less than that of Hg, and greater than that of W, but the series does not correspond. For, considering that Os, Ru, and Fe are analogous, but that the atomic weights of Ru and Fe are less than the atomic weights of Pd and Ni, we can foresee that the atomic weight of Os will be less than that of Pt, so that Ir, which is between Pt and Os, will be intermediate. According to Berzélius and Frémy, Os = from 199 to 200, Ir=197, Pt=198. The predicted series is the following:—Pt=198,* Ir = about 197, Os = from 196 to 195, whilst Au will be equal to 199.

Besides, it is allowable to suspect errors in the determinations of the atomic weights of the metals of the platinum group, not only because it is difficult to separate them one from another, but also because the compounds which are used for these determinations are very unstable; IrCl_4 and OsCl_4 , for example, easily lose a part of their chlorine.

(To be continued.)

ON SOME OF THE
EFFECTS PRODUCED BY AN INDUCTION-COIL
WITH A
DE MERITENS MAGNETO-ELECTRIC MACHINE,†
By WILLIAM SPOTTISWOODE, P.R.S.

IN the *Philosophical Magazine* for November of last year I gave an account of a mode of exciting an induction coil by the direct application of one of M. de Meritens' alternating machines, without the intervention of a contact-breaker or the use of a condenser. The experiments of Prof. Dewar on the arc furnished by the machine itself on its spectrum, and on its behaviour in respect of electrolysis, described before the Royal Society (see *Proceedings*, Feb. 13, 1880), have led me to think that an account of some of the peculiarities in the induced discharge, to which the machine gives rise, might be acceptable to the Society.

And, first, as regards the secondary discharge in air. It was mentioned in the paper first quoted that the spark produced by this machine presented an unusually thick yellow flame, and that it was accompanied by a hissing noise different from that commonly heard with a coil excited by a battery. As the machine gives alternate currents, the secondary discharge presents sparks of equal strength in both directions, and the general appearance to the eye is symmetrical in respect of both terminals. The spark was observed in a revolving mirror, first in a vertical and secondly in a horizontal direction. The discharge, although apparently continuous, was immediately seen to be intermittent, with a period in unison with that of the machine. Tongues of flame, leading alternately from one terminal and from the other, crossed the field of view. The length of spark first used (vertically) was about half an inch. When the length was increased to about two

inches, flashes or bands of continuous light were seen to traverse the field of view in diagonals of low slope (*i.e.*, nearly horizontally), showing that there were masses of heated matter passing from time to time at moderate velocity between the terminals. From the known period of the machine, and the number of the discharges crossed by these flashes in their passage from terminal to terminal, it was calculated that the time of passage was about 0.03 of a second. Occasionally there was a still brighter flash or meteor, which similarly traversed the field, but with a velocity apparently of about double that of the others.

When the discharge was set horizontally, similar phenomena were seen; but at a distance about midway between the two terminals the flames appeared to meet, and at their point of meeting they showed continuous luminosity throughout. When the flame was blown aside, there appeared occasionally, and at rather long intervals, the true spark, evinced by an irregular bright line, reaching from one terminal to the other.

On observing the discharge in air attentively, it was noticed that whenever a true spark passed its passage was marked, as usual, by an irregular bright line when its path was outside the aureola or flame, but by a similar dark line when its path was within the aureola. Whether this be a merely subjective effect, due to the fatigue of the nerves of the retina by the bright spark, which always precedes the aureola in point of time; or whether it has any electrical significance, perhaps requires further examination.

The spectrum of the secondary spark was then examined with terminals of various metals.

Aluminium.—The spectrum showed a faint continuous background with the yellow sodium lines, and faint oxide of aluminium lines. This was with a spark of half an inch. But although the spark was subsequently lengthened, no difference in the spectrum was perceived excepting that the continuous background was rendered more bright.

It would seem that these appearances are due to some such process as the following:—The heat due to oxidation, added to that of the discharge, is sufficient to volatilise the oxide of aluminium; but that in its passage across the interval between the terminals, the oxide becomes so cooled that it gives a continuous spectrum. When the spark was lengthened, the oxide, although perhaps at first more heated than with the shorter spark, had more time to cool.

Magnesium.—In this, as in the former case, we have a faint continuous spectrum as a background, on which were seen the *b* group of magnesium lines. One other line in the blue occasionally flashed out, but was not permanently present. There was also a faint trace of the oxide spectrum. The contrast between the cases of aluminium and magnesium, in respect of the prominence of the oxide, or of the true metallic spectrum, is doubtless due to the fact that in the former case the oxide, and in the latter the metal, is the one which is more easily vaporised. On sending a blast of air on the discharge, the blue line always disappeared; the current of air having lowered the temperature so far as to prevent the vaporisation necessary for its production.

When the spark between magnesium terminals was made to pass through hydrogen, the characteristic lines of hydrogen were seen, apparently owing to a rise in temperature. This, as mentioned below, does not occur with carbon poles.

Platinum.—With terminals of this metal the spectrum was mainly continuous, with the addition of the ubiquitous yellow sodium lines. When the spark was short a few bands were faintly visible, some apparently these of nitrogen, and others in the blue and violet belonging to the oxide of platinum. When the spark was lengthened, these bands disappeared, and nothing but the continuous spectrum (with the D lines) was visible.

It appears from these experiments that the application of the De Meritens machine to the induction coil furnishes

* According to Berzélius, 198; according to Andrews, 197.83.

† A Paper read before the Royal Society, February 19, 1880.

us with the means of isolating certain lines of the metallic spectrum from the rest. It has, in fact, enabled us to reduce at pleasure the spectra of aluminium, of magnesium, and of platinum, to their most persistent lines; precisely as had already been noticed as occurring by natural processes in the cases of sodium and of calcium. As a general rule, when the spark is shortened the metallic, or the oxide, lines come out, when it is lengthened they disappear.

From this we may conclude (1) that the discharge which we have been examining is a real flame with metallic particles passing between the terminals in a solid condition; and (2) that in general the temperature is comparatively low, *i.e.*, that it is sufficient to cause any considerable vaporisation. This is notably the case when the arc is long, and when the matter thrown off from the terminals has sufficient time in its passage to cool.

The spark was then tried between carbon terminals in atmospheres of hydrogen and of carbonic acid. In none of them did the spectrum show any gas lines; but with hydrogen there were faint traces of the hydrocarbon group in the green. In this respect the spark differs from the discharge direct from the machine, inasmuch as the latter gives some of the hydrogen lines in hydrogen and carbon lines in carbonic acid.

When the spark was discharged in a magnetic field, known phenomena were reproduced, but owing to the thickness and mass of the flame and the extraordinary strength of the magnetic field, they were exhibited in a state of great splendour.*

When the spark passed in an equatorial direction the whole flame was spread out in an equatorial plane, in which heated masses might be seen revolving in one direction or in the other in the neighbourhood of each of the magnetic poles. To give some idea of the actual appearance, it may be mentioned that a symmetrical spindle-shaped discharge, 15 m.m. in length and 3 m.m. in thickness, was spread out by the magnet into a disk of about 15 m.m. in diameter and 1 m.m. in thickness. When the spark passed in an axial direction, or when the poles themselves were made the terminals, the phenomena described in my paper "On an Experiment in Electro-Magnetic Rotation" (*Proc. Roy. Soc.*, March 30, 1876) were reproduced.

Whatever was the direction of the spark, the resistance due to the magnetic field was such as to extinguish the discharge, provided that the striking distance was near the limit that it could attain when no magnetic field was present. If a plate of glass was interposed between the poles of the magnet (which were still used as terminals) the yellow flame disappeared, and the spark divided itself into numerous ramifications of true sparks which found their way round the edges of the plate. As soon as the magnet was excited the resistance in the field became so great as to exceed that of the glass plate itself, and the plate was pierced.

When magnesium terminals were used in an atmosphere of hydrogen, the yellow sodium lines, the blue and green magnesium lines, and the red line of hydrogen were visible near the terminals, with a continuous background. When the magnet was excited, the only change observed was that the lines became slightly fainter.†

Professor Dewar was good enough to measure the efficiency of the secondary discharge, by taking an inch spark in a glass bulb placed in the centre of a calorimeter, in the same way as he had already measured the efficiency of the intermittent current direct from the machine. The former amounted to about 430 gramme-units per minute, while the latter had been found to be 6000 per minute. The relative efficiency may, therefore, be taken at about

1 : 15. And as the machine was giving about 300 currents per second, this would give for the secondary—

$$430 : 60 \times 300 = 0.023 \text{ unit per discharge,}$$

and for the primary—

$$6000 : 60 \times 300 = 0.3 \text{ unit per discharge.}$$

The results in the case of the secondary discharge may not perhaps possess any great degree of accuracy; but, in the absence of any other information on the question, they may serve to indicate the general scale of magnitude by which the coil discharge is related to that of the machine.

Leaving the subject of the spark from the induction coil, one of the most remarkable effects produced by this machine was the illumination of vacuum tubes by the currents taken simply from the machine. A small sphere of about 2 inches in diameter, with an air-vacuum, and having two parallel straight terminals reaching nearly across the sphere and about half an inch apart, was (after the first attempt, when there was some difficulty in getting the discharge to pass) readily illuminated. Owing to the alternate currents, both terminals were of course surrounded with the usual blue halo. When the speed of the machine was reduced, the discharge through the tube was not maintained, showing that only that part of the current from the machine which possessed the highest electromotive force, and perhaps also the greatest strength, was sufficient, and was therefore actually used for the purpose. As this was apparently only a small fraction of the whole current, we may herein find an explanation of the fact that, compared with the effect from the induction spark, the illumination was moderate and the heating insignificant. It would, perhaps, not be easy to establish an accurate comparison between this and other sources of electricity; but some idea may be conveyed by the fact that, from experiments made with this tube with Mr. De la Rue's chloride of silver battery on a former occasion, and quite independently of the present question, it was estimated that a current having an electromotive force of 400 volts was necessary to effect a discharge.

Other tubes were tried, and were illuminated in the same way.

When the tube was placed in the magnetic field the discharge underwent the modifications described by Plücker, by Hittorff, and by others. But it may be mentioned as a result of the extraordinary strength of the field here employed that, whenever the tube was so placed that the direction of discharge made any considerable angle with the lines of magnetic force passing through it, the discharge was completely extinguished. When the tube was removed to a weaker part of the field the discharge reappeared distorted in form, and altered in colour in a manner known to those who have experimented in this direction.

ON THE USE OF THE SPECTROSCOPE IN DISCRIMINATING ANTHRACENS.

By BENJ. NICKELS, F.C.S. F.I.C.,

(Continued from p. 52.)

In the application of crude anthracen to the production of artificial alizarin and anthrapurpurin—the latter especially—considerable difficulties have been encountered, not only in estimating the actual amount of colour-yielding material present in a given sample of crude anthracen, but in eliminating associated and useless substances which for the time being stand in the position of impurities. Without entering upon the long and complex list of hydrocarbons passing over in the ordinary process of tar distillation, associated more or less with every product derived from it, but more especially with anthracen, sufficient is known to point with some degree of judgment to

* Through the kindness of the Royal Institution, Faraday's great electro-magnet, excited by the Siemens machine, was placed at my disposal.

† There was some reason to think that, at the moment of making the magnet current, the lines became decidedly brighter, and that they lost their brilliancy if that current was maintained. But this, perhaps, requires further examination.

the selection of appropriate samples, and as best adapted to the particular line or process to be followed.

The presence of high fusing-point hydrocarbons is known to offer considerable impediment to the commercial production of high quality anthracens, irrespective of the process followed, whether of washing or sublimation, and such as will ultimately be required under more simplified methods of converting that product into colouring matter. These bodies—classified under the broad and general term of “pyrene” and “chrysen”—are amongst the last in the series that pass over during the usual process of distillation, and do not make their appearance till long after anthracen: the mere term, however, as expressive of these substances is ambiguous, as other hydrocarbons, equally objectionable, are present in the products distilling over, many of them, there is good reason for believing, yet unrecognised. From the fact that these bodies are objectionable, and even prejudicial, in the subsequent processes to which the anthracen has to be submitted, it becomes desirable to possess a simple method of determining their existence, present or otherwise, in a given sample. The fact that most of them yield “quinons,” exhibiting well-marked and characteristic qualities, is to an extent some indication, and would perhaps be all that was necessary were their separation when mixed or associated an easy task. Such, however, is not the case; and the more we approach the examination of these substances in admixture with anthracen the more do we approach problems extremely difficult of solution.

During a somewhat extended experience in the manufacture of artificial alizarin and anthrapurpurin in this country, I have, in common with many others, devoted much time and careful attention to the vexed question of anthracen testing, and have long felt that an expeditious method of examining a given sample submitted for analysis, and without the expenditure of time necessary in wading through the recognised tests, private or otherwise, would be of practical utility—a test that should readily indicate the presence or absence of objectionally high fusing-point hydrocarbons, and in doing so lead up to the question, How came they there? by admixture or otherwise? or from intentionally running a still beyond the limit for good anthracen?

Such a test was originally hinted at by Professor Morton (United States) in a valuable and exhaustive series of papers published in the *CHEMICAL NEWS* as far back as the year 1872 (vol. xxvi., p. 199), following into December, and appearing again in January, 1875 (vol. xxxi., pp. 35, 45). In the later communications it was even suggested that the method followed might be of “some moment in its bearing upon the discrimination of anthracens and associated bodies.” So far as my information serves me, it would not appear that any practical advantage has been taken of this publication, so strongly commending itself to those engaged in the examination and testing of commercial anthracens.

My own experience in the use of the spectroscope in the direction indicated dates some two years back, and at which period I had commenced the examination of the various substances fractionally separated from coal-tar by distillation, and had made considerable progress with the research, when my attention was called to the papers in question. As a matter of fact, I perused them with much interest, the more so inasmuch as my own observations had indicated that the instrument named would, within a certain limit, serve an important purpose in examining anthracens, doubtful or otherwise.

The mode of research, or rather method, followed by Professor Morton was, as will be found on reading over his interesting papers, too complex for ordinary technical purposes, and such as could only be conducted in physical laboratories. The instrument used was a Browning “one prism spectroscope,” and the substance to be examined prepared by “melting with paraffin and pressing between slips of glass.” The special form of instrument employed, however, was not well suited for the detection of delicate

absorption-spectra, its dispersive power being too high: simpler forms of instrument are at our disposal, and admirably fulfil all the conditions that could be desired.

The point of interest in the communication of Professor Morton undoubtedly lays in the fact that attention was called to some remarkable absorption-spectra obtained after much care and seeking, and which he considered to be “chrysen,” employing material specially prepared from tar distillations made at the works of Messrs. Page, Kidder, and Co., of Bull’s Ferry: furthermore, that if not directly attributable to “chrysen itself, it was to an associated substance or impurity” which he had not succeeded in separating. To this question I have since given much attention, and from the fact that my results, following closely in a similar direction, had pointed to associated bodies of still higher fusing-points, as those yielding the observed spectra, I have been induced to view this mode of research as a valuable preliminary test and adjunct to known methods of examining crude or commercial anthracens. Broadly, it may be stated that ordinary yellow chrysen crystallises in minute lemon-yellow laminæ, presenting a magnificent fluorescence, and fusing at 250° C. If we prepare a solution of this hydrocarbon in benzol, and compare it with similar solutions of naphthalin, acridin, carbazol, phenanthren, anthracen, and pyren, employing a suitable instrument for the observation, we find a remarkable departure in the case of the chrysen solution. Each of the preceding substances named present a clear and continuous spectrum, whereas yellow chrysen exhibits superb absorption-bands, broad and deep, two of them placed between the F and G lines, and one immediately to the left of the G line (commencing with D to the right).

In prosecuting these experiments I have found that the substance affording these bands is not chrysen, but (as may already have been observed, although I am unacquainted with any publication) to the presence of an associated body, and to which its intense yellow colour, as already conjectured, is due. Pure white chrysen presenting the same pearly lustre as anthracen, and equally fluorescent, fusing at 248° to 249° C., affords no absorption-spectra, those previously observed in the yellow variety having entirely disappeared.

The associated and colouring substance when isolated presents remarkable features, soluble with difficulty in high boiling-point naphtha; it crystallises in golden-yellow metallic looking plates, with a fusing-point of 280° C.; both as a solid and in solution it presents a superb greenish fluorescence, and contains nitrogen either as an element of constitution, or of a still further associated compound yet undetermined. The additional points of interest resulting from the work carried out lead me to the conclusion of my paper, and to state that during the period named I have examined spectroscopically some hundreds of samples of commercial anthracen, distilled and sublimed, English and foreign, and of all works. Many commercial samples, especially as originally made, and indicating a quality of 40 per cent and upwards by chinon test, exhibit no absorption-spectra or bands: some specimens show them only faintly, and these undoubtedly rank amongst the better qualities; others, and by far the greater number of modern makers, exhibit them broad, heavy, and deep. For comparison of spectra I have been accustomed to use a Browning micro-spectroscope; but for the majority of observations a Browning direct-vision “Miniature,” or pocket instrument, offers all that can be desired, and possesses the advantage of being so simple in its use that the most inexperienced operator may easily familiarise himself with its application.

For the guidance of those interested in these results I append the following particulars:—A few grains of the sample to be examined are placed in a test-tube, a little benzol added, and warmed so as to dissolve the contained anthracen. Finally it is made up to about 6 c.c. with more benzol, and passed through a paper filter to remove water. The spectroscope, previously focussed, is then brought to the eye, and the tube of anthracen solution also into close

proximity with the adjusted slit of the instrument. On looking through the solution, and with a good back light, absorption-bands, if present, are seen as fine lines or bars crossing the spectrum, and in the position of those already indicated, the depth or intensity of which assist the judgment in its conclusion.

Specimens of the pure substances may be had on calling at my laboratory, or of Mr. John Browning, the optician, 63, Strand, London, who has kindly prepared a number of these samples in sealed tubes for spectroscopic examination.

104, Leadedhall Street, E.C.

A SERIES OF ANALYSES OF AIR.

By H. MACAGNO,
Director of the Agricultural Station, Palermo.

For sanitary purposes I have had occasion to undertake a series of examinations of air in this town (Palermo, Italy) at its Astronomical Observatory, 72 metres above sea-level.

The analyses were made by the following methods:—

Oxygen, by pyrogallic acid solution and potash.

Carbonic Acid, by the increase of weight of caustic potash solution, after the passage of a known volume of air.

Ammonia, Nitrates, and *Organic Matters*, by the analysis of water collected from a set of wash-bottles, through which a measured quantity of air was drawn. The ammonia was determined by the Nessler test, the nitrates by the indigo process, and the organic matters by permanganate of potash in alkaline solution at the boiling-point of water, taking 1 of crystallised permanganate equal to 5 of organic matter (Wood, Kubel, &c.).

The results of the following Table are expressed in litres and grammes, and are referred to 100 litres of air in normal condition, namely, at 0° of temp. C. (32° F.), and 760 millimetres of pressure.

Date of Analysis. 1879.	Oxygen. Litres.	Carbonic Acid. Litres.	Nitric Acid. Grms.	Ammonia, NH ₃ . Grms.	Organic Matters. Grms.	For 10 Days.	
						Mean Temps. C.	Rain-fall in m.m.
Feb. 10	—	—	—	—	—	12.4	11.58
„ 20	20.879	0.021	—	0.024	0.154	13.6	17.29
„ 28	20.891	0.048	—	0.028	0.127	12.8	3.57
Mar. 10	20.715	0.025	—	—	0.115	9.8	9.24
„ 20	19.994	0.025	—	—	0.094	13.3	—
„ 31	20.888	0.022	—	—	0.070	14.4	30.61
April 10	20.910	0.021	—	traces	0.076	14.3	32.01
„ 20	20.880	0.064	—	—	0.094	15.8	18.45
„ 30	20.898	0.045	—	—	0.055	16.0	14.75
May 10	20.913	0.005	—	—	0.020	14.6	17.20
„ 20	20.902	0.049	—	—	0.072	14.0	16.65
„ 31	20.017	0.033	—	0.036	0.142	19.8	2.23
June 10	20.894	0.041	—	—	0.107	20.5	—
„ 20	20.918	0.043	—	0.040	0.363	22.0	—
„ 30	20.915	0.043	—	0.009	0.162	23.5	—
July 10	20.977	0.020	traces	0.010	0.111	23.4	—
„ 20	20.984	0.076	—	0.080	0.157	22.6	—
„ 31	20.899	0.039	—	—	0.138	23.0	—
Aug. 10	20.910	0.028	traces	0.005	0.165	25.1	—
„ 20	20.888	0.030	—	0.007	0.112	25.1	—
„ 31	20.895	0.039	—	0.009	0.131	25.0	—

Mean Results.

February, March, April, May (with rainfall)—

20.717 0.033 0.000 0.008 0.102 14.2 173.18

June, July, and August (without rainfall)—

20.920 0.039 traces 0.009 0.160 23.4 0.00

The observations made on the air of Palermo in different seasons, as above described, cannot fail to attract

the attention of the analytical student of nature. And, first, they show, as regard organic impurity and carbonic acid, an increase with augmentation of temperature, which is the principal factor of all putrefactive changes in nitrogenised or ammoniacal matters. Secondly, we cannot but observe the influence of rain as a great air-washer. Rain dissipates the deleterious gases which accumulate and float over towns and cities. It brings down from the higher regions of the atmosphere a more salubrious air, and by the flushing of drains and cleansing of the surface of the country, aids in preventing the contamination of air by the exhalations of animals, and by the decomposition of animal and vegetable matters, which is incessantly proceeding.

A curious and interesting fact, that I happened to discover by these researches on the air, is the want of oxygen during the *siroco* wind, which is so disagreeable to the inhabitants of Sicily. We note in the Table of results two *minima* of oxygen, on March 20 and May 31, and some analyses made at other times, and when also this African wind was blowing, led me to the same conclusion.

March 20, 1879; *siroco* wind; oxygen, 19.994 litres p. c.

„ 21, „ „ „	20.008 „ „
„ 22, „ „ „	20.064 „ „
April 15, „ „ „	19.998 „ „
May 29, „ „ „	20.021 „ „
„ 30, „ „ „	20.032 „ „
„ 31, „ „ „	20.017 „ „

Here we have seven determinations of the most important element of air during that singular wind with its heat and dryness,* rendering so troublesome the medium in which we are always bathed, and in all cases the want of oxygen is very evident.

I do not know how to explain this curious phenomenon; notwithstanding, I cannot but register it, hoping that the attention of scientific men will be drawn to this unknown condition of the air when the *siroco* wind blows.

Palermo, Italy, January 15, 1880.

ARTIFICIAL PRODUCTION OF THE DIAMOND

The following letter appeared in the *Times* of Feb. 20:—

“To the Editor of the *Times*.”

“Sir,—A few weeks since I had to proclaim the failure of one attempt to produce the diamond in a chemical laboratory. To-day I ask a little space in one of your columns in order to announce the entire success of such an attempt by another Glasgow gentleman.

“That gentleman is Mr. J. Ballantine Hannay, of Woodbourne, Helensburgh, and Sword Street, Glasgow, a Fellow of the Chemical Society of London, who has to-day sent me some small crystallised particles presenting exactly the appearance of fragments of a broken diamond.

“In lustre, in a certain lamellar structure on the surfaces of cleavage, in refractive power, they accorded so closely with that mineral that it seemed hardly rash to proclaim them even at first sight to be diamond. And they satisfy the characteristic tests of that substance. Like the diamond, they are nearly inert in polarised light, and their hardness is such that they easily scored deep grooves in a polished surface of sapphire, which the diamond alone can do. I was able to measure the angle between the cleavage faces of one of them, notwithstanding that the image from one face was too incomplete for a very accurate result. But the mean of the angles so measured on the goniometer was 70 deg. 29 min., the correct angle on a crystal of the diamond being 70 deg.

* During the *siroco* wind the relative humidity of air (determined by the pycrometer) is diminished to 30°, 24°, 20°, and even 18°.

31·7 min. Finally, one of the particles, ignited on a foil of platinum, glowed and gradually disappeared exactly as mineral diamond would do.

"There is no doubt whatever that Mr. Hannay has succeeded in solving this problem and removing from the science of chemistry an opprobrium so long adhering to it; for, whereas the larger part of the great volume recording the triumphs of that science is occupied by the chemistry of carbon, this element has never been crystallised by man till Mr. Hannay achieved the triumph which I have the pleasure of recording to-day. His process for effecting this transmutation, hardly less momentous to the arts than to the possessors of a wealth of jewelry, is on the eve of being announced to the Royal Society.

"I am, Sir, your obedient servant,

"N. STORY-MASKELYNE.

"Mineral Department, British Museum, Feb. 19."

[A detailed paper on the above subject was read at the Royal Society last night, and will appear in the next number of the CHEMICAL NEWS.]

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 19, 1880.

Mr. WARREN DE LA RUE, President, in the Chair.

THE list of Officers and Council proposed by the Council for the ensuing year was read from the Chair:—

President—H. E. Roscoe.

Vice-Presidents—F. A. Abel, C.B.; B. C. Brodie; Warren De la Rue; E. Frankland; J. H. Gladstone; A. W. Hofmann; W. Odling; Lyon Playfair; A. W. Williamson; J. Dewar; J. H. Gilbert; N. S. Maskelyne; V. Harcourt; R. Angus Smith; J. Young.

Secretaries—W. H. Perkin, H. E. Armstrong.

Foreign Secretary—Hugo Müller.

Treasurer—W. J. Russell.

Other Members of Council—M. Carteighe, C. Graham, C. W. Heaton, H. McLeod, E. J. Mills, J. M. Thomson, W. C. Roberts, W. A. Tilden, W. Thorp, T. E. Thorpe, J. L. Thudichum, R. Warington.

The following gentlemen were elected Auditors:—A. J. Greenaway, G. H. Makins, and F. J. M. Page; being proposed by Mr. Warington and seconded by Mr. C. J. Neison.

The following certificates were read for the first time:—E. C. Copas, H. H. Slater.

During the evening the following gentlemen were balloted for and declared duly elected Fellows of the Society:—J. C. Evans, W. H. Glazier, J. Hogarth, K. W. Hedges, R. Howell, A. Lloyd, W. Macnab, A. R. Miller, J. McCarthy, G. H. Morris, P. Mathews, W. O. Prosser, H. T. Pasley, W. B. Roberts, G. Salet, A. G. Salamon, T. Terrell, G. Wilcock, W. H. Wood.

During the evening the PRESIDENT mentioned that he has recently seen a crystal prepared by Mr. Hannay, of Glasgow, which had been examined by Mr. Nevil Story Maskelyne, and found to have the angles, lustre, hardness, &c., of the diamond. A similar crystal had been burnt, and found to contain 97 per cent of pure carbon. It was therefore to all intents and purposes a diamond.

The following papers were read:—

"On the Production of Ozone during the Combustion of Coal-gas," by R. H. RIDOUT. Some years ago the author possessed a Bunsen burner which always emitted the smell of ozone. By means of an aspirator and suction-tube it was determined that the greatest production took place at the apex of the flame. The burner, however, was injured, and the author has not been able to get the same action again, either with it or with other burners. He

noticed subsequently, while working with singing flames, the same smell; in consequence he made several experiments. A glass tube, one-eighth inch bore, conveying coal-gas, was placed inside another tube five-eighth inch bore, 15 inches long. A strip of iodised paper was pasted inside the tube throughout its length: the gas burned with a blue flame. After a short interval the paper was discoloured, and on being moistened gave the oxidising reaction, commencing at a point about half-an-inch above the flame, and decreasing towards the top. The author endeavoured to apply this method of producing ozone to technical purposes, but without success. From several experiments the author concludes that the sulphur in the coal-gas is eliminated as sulphuric acid under the above circumstances. Various experiments were now made to determine the nature of the body which gave the blue reaction with starch. It was not absorbed by passing it through caustic potash or water, which would remove all oxides of nitrogen, whilst no oxidising action was observed in a chromate of potash solution, indicating the absence of peroxide of hydrogen. The author concludes, therefore, that the substance formed was ozone. Ether and alcohol burned from wicks of capillary glass tubes gave similar results. Ozone was also produced by the flame of a minute glass Herapath blowpipe, as long as the cone was perfect and of a violet colour; but as soon as the inner bright green cone was exposed the production of ozone ceased. The same violet colour attends the formation, whilst a green tint indicates the destruction, of ozone in the electric discharge.

Mr. WARINGTON rather doubted whether all the oxides of nitrogen would be retained in the absorbing apparatus described.

Dr. ARMSTRONG mentioned that this production of ozone by gas-flames under certain conditions had been patented some time since.

Mr. RILEY had noticed the smell of ozone round the tuyères of blast-furnaces which were not working properly.

Mr. KINGZETT insisted on the great difficulty in removing traces of ozone from a comparatively large quantity of air. A very long tube and a current of low velocity were essential. He did not think that the experiments just related proved the formation of ozone.

Dr. WRIGHT confirmed the statements made by Messrs. Warington and Kingzett. Carbonic acid would pass through caustic potash, and hydrochloric acid through silver nitrate solution, unless special precautions were taken.

The PRESIDENT said that he had used a washing-vessel with spiral divisions with great success, a large amount (20 to 30 feet) of washing surface being thus obtained in a moderate compass.

The author having briefly replied,

Dr. RUSSELL took the chair, and called on Prof. McLEOD to make some remarks on the production of ozone by the passage of moist air over phosphorus.

At the last meeting Mr. Kingzett pointed out an error in some calculations which the author had recently brought before the Society. The correctness of this criticism the author frankly admitted, but did not think that the necessary alteration in the results invalidated his arguments (*Journ. Chem. Soc.*, Feb., 1880) as to the formation of ozone during the slow oxidation of phosphorus. In his opinion the evidence was quite conclusive without the quantitative experiments. He had investigated the reaction simply to satisfy himself as to its nature, and at the commencement thought that hydroxyl was probably formed; but the results of about 100 experiments had convinced him that the substance formed was ozone. He had not observed any evidence of the formation of hydroxyl. Thus, phosphorus was placed in a cylinder with sulphuric acid and bichromate of potash. After some time the liquid and the phosphorus were removed, and the gas shaken up with water. The liquid did not affect potassium iodide and starch, whilst the gas instantly turned the mixture blue. He was glad to hear that Mr. Kingzett was continuing his researches on the subject, and would be

pleased to learn that the substance was peroxide of hydrogen, and still more gratified if it turned out to be ozone.

Mr. KINGZETT, in reply, said he believed Andrews had shown that the active substance in air, in which phosphorus had been allowed to oxidise slowly, was removed by prolonged agitation with water, and that a French chemist had recently observed that the water gave the reactions of ozone, but went on to show that this reaction was due to phosphorous acid. He did not think that the presence of ozone had been thoroughly established.

Mr. R. H. RIDOUT then gave an account of "Some New and Improved Laboratory Appliances," which were exhibited. A test-tube brush, a piece of red or black india-rubber tubing about 3 inches long and $\frac{1}{4}$ -inch bore has seven longitudinal and parallel slits made to within $\frac{1}{4}$ -inch of each end. One end is then inverted and turned back until it is over the other end: the tube is finally secured to a wooden handle with twine. A blowpipe for spirit-lamp or gas. A hydrogen sulphide apparatus. Two Winchester quarts are fitted up like wash-bottles, the long tubes being connected together by a short piece of india-rubber tubing. The short tube of one bottle is open, the other has a piece of india-rubber tubing and a pinchcock: this tube is connected with a sulphuretted hydrogen apparatus, and the pinchcock opened. The gas drives the oil, with which the Winchester quart is filled, over into the other bottle, which is empty. When the first bottle is filled with SH_2 the apparatus is ready for use. The liquid to be treated is placed in a flask having an india-rubber cork and tube; when the liquid is boiling the latter is connected with the above apparatus, and the steam allowed to condense: the S_2H is rapidly absorbed by the liquid. The liquid is again boiled, to drive back the superfluous gas into the bottle: no smell is perceptible during the operation. Filter Funnel.—The author has had funnels made with stem 0.5 m.m. inside, and sides ground to an angle of 60° ; no platinum cone is necessary. He has also contrived an apparatus for delivering the liquid and precipitate into the centre of the filter-paper. Continuous Aspirator.—This consisted of a piece of three-eighths lead tubing bent into a circle, with a hole one-tenth of an inch in the centre of the concave part of the bend. A suction-tube is soldered into this hole: as the velocity of a current of water passing round the bend is greatest on the outside, a relative vacuum is caused on the inside. Filter Pump.—A vacuum is obtained by filling a flask with steam and condensing it. Apparatus for Taking the Gravity of Liquid in Terms of Water at the Same or Other Temperature.—Two graduated U-tubes are connected by a T-tube provided with a pinchcock. The liquids are placed one in each U-tube; air is then blown in by the T-tube, and the pinchcock closed. The liquids are displaced inversely as their gravities. One U-tube, B, is kept filled with pure distilled water; consequently,—

$$\frac{\text{Displacement in B}}{\text{Displacement in A}} = \text{sp. gr. of A.}$$

Messrs. Woolley, of Manchester, make the filter-funnel; Messrs. Townson and Mercer supply the other apparatus.

Dr. ARMSTRONG then made some remarks on "Some Recent Researches on the so-called Unsaturated Compounds." Results recently obtained by several perfectly independent investigators have thrown doubt on the theory usually held as to the constitution of the above substances, and re-open the question of the presence of dyad carbon in these compounds. Menschutkin has pointed out (*Berl. Ber.*, xiii., 163), from his experiments on the etherification of hydro-sorbic and sorbic acids, that the former is a primary and the latter a tertiary acid, *i.e.*, that the former contains the group CH_2COOH , and the latter the group C.COOH . Fittig has shown that hydro-sorbic acid is either $\text{C}_3\text{H}_7\text{CH}=\text{CHCOOH}$ or $\text{CH}_3\text{CH}=\text{CHCH}_2\text{CH}_2\text{COOH}$, the latter appearing to be the more probable. Menschutkin's observations would seem to point in the same direction. But from an acid having this formula it is not possible to derive an acid containing the group C.COOH , unless it be assumed

that two atoms of hydrogen are withdrawn from the carbon atom with which the COOH group is associated, and consequently that sorbic acid contains a dyad carbon atom. Reference was then made to Fittig's suggestion of the formula $\text{CH}_3\text{CH}(\text{COOH}).\text{C.COOH}$, for either citra or mesaconic acid. The determination of the rate of etherification of the three pyrocitric acids by Menschutkin's method would seem to promise interesting results, although as it is to be expected that the phenomena will be complicated by the occurrence of isomeric change, the method is not so likely to furnish any very conclusive evidence in this case. Lastly, Dr. Armstrong drew attention to the recent important paper by Brühl (*Liebig's Annalen*, 200), and especially to the remarkable conclusions arrived at by him with reference to bodies of the acetylene type.

After a few remarks by Mr. WILLIAMS and Dr. WRIGHT, the Society adjourned to March 4, when a lecture will be delivered by Prof. T. E. Thorpe, "On the Relation between the Molecular Weight of a Body and its Specific Gravity when in the Liquid State."

METEOROLOGICAL SOCIETY.

Wednesday, February 18, 1880.

Mr. G. J. SYMONS, F.R.S., President in the Chair.

DR. J. S. CAMERON, Dr. F. E. Carey, J. B. Charlesworth, A. Collenette, S. Forrest, J. G. Gamble, H. J. Marten, J. Nixon, B.A., W. P. Probert, LL.D., S. Rostron, W. P. Swainson, and E. W. Wallis were elected Fellows.

The papers read were:—

"On Typhoons in China, 1877 and 1878," by Lieut A. CARPENTER, R.N.

"Note on the Reports of Wind Force and Velocity during the Tay Bridge Storm, December 28, 1879," by R. H. SCOTT, F.R.S. These reports seemed to show that the velocity of the wind on that occasion was not so high as was generally supposed, and had been frequently exceeded, but that some of the gusts were very violent.

"On the Frost of December, 1879, over the British Isles," by W. MARRIOTT, F.M.S. Exceptionally low temperatures were registered all over the British Isles from the 1st to the 7th of December. On the 1st, the lowest temperature was -2° at Ketton, near Stamford; and the next lowest was 5° at Trent College. The temperature continued low throughout the day, at several places not rising above the freezing-point. On the 2nd the cold was more intense. In the counties of Leicester, Lincoln, and Nottingham, the temperature fell below zero, the lowest being -4.5° , at Coston, near Melton Mowbray. Temperatures between 0° and 10° were registered in the north and south of Scotland, and along the central part of the north of England to the Midland and Eastern counties; while over the whole of England, Scotland, and Ireland, with the exception of the sea-coast stations, the temperature fell below 20° . On the 3rd the temperature was more evenly distributed, and not quite so intense as on the previous day. However, in the North Riding of Yorkshire and the Valley of the Tees, readings at and below zero were registered, the lowest being -2° at Gainford. On the 4th intensely cold weather was experienced over the south of Scotland and the north of England, the lowest reading obtained was -23° , at Blackadder, in Berwickshire. -16° was also registered at Springwood Park, near Kelso, and readings of -5° were reported at Haddington, Melrose, and Corbridge-on-Tyne, and -4° at Alston. Temperatures below 10° were registered over the south and south-east of Scotland, and over the north of England as far as the Valley of the Trent, and also in the Eastern Counties; while over almost the whole of England, Scotland, and Ireland the temperature fell below 20° . In some parts of the south of Scotland and the Border counties the maximum temperature during the day did not rise to 20° . On the 5th the minimum temperature was not so low as on

he previous day, there being a cloudy sky and a general fall of snow. In Ireland, however, this was the coldest day of the month. On the 6th the temperature fell considerably in Derbyshire, Nottinghamshire, and Yorkshire: readings of 3° being recorded at Trent, -10° at Buxton, and 0° at York and Stanley. At many places the maximum temperature during the day was much below the freezing-point. On the 7th very low temperatures were registered over the whole of the north and east of England; the lowest reported was -10° at Ketton, near Stamford. The temperature fell below zero in the counties of Essex, Leicester, Derby, Lincoln, Nottingham, and York, and also in the south of Scotland, while over almost the whole of the north-east and central part of England, as well as a portion of the south-east district, the temperature fell to 10° or below. Readings below 20° prevailed over nearly the whole of England and Scotland and the centre of Ireland. The maximum temperature during the day at a few places was extremely low, the thermometer at Appleby only recording 12.4° , and that at York 18° . During the next few days a little warmer weather prevailed, but on the 9th the temperature fell below 20° over the central part of England, Scotland, and Ireland. Low temperatures were also experienced at most places on the 12th. Milder weather continued for the next few days, but on the 17th the temperature again fell below 20° over the whole of the south of England. Low temperatures also prevailed on the 18th, 21st, 23rd, 24th, and 26th, while the maximum temperatures at many places on the 21st and 26th did not reach 32° . At almost all the inland stations frost occurred on an average of about 25 days during the month, and temperatures below 20° were registered from 8 to 13 days at several places. The only station where frost was not felt was Scilly, the lowest temperature recorded there being 33° on the 2nd. The only comparatively mild districts were the west and south of Ireland and the extreme south-west of England. Even the seaside health resorts which are reputed for their mild climates were not exempt from the cold, the temperature falling below the freezing-point on eleven occasions at Ventnor, fifteen at Torquay, twenty at Sidmouth and Eastbourne, and twenty-four at Ramsgate and Worthing. During the time of the cold weather the barometer was very high over these islands, and an anticyclone was formed over those districts where the lowest temperatures were recorded. That the cold was the result chiefly of radiation is shown by the great difference in temperature at the hill and valley stations. For instance, at Farley, 640 feet above sea-level, 17.7° was registered on the 7th, while at Oakamoor, 300 feet lower, in the Valley of the Churnet, and less than a mile distant from Farley, the temperature fell to 11° . The effect of the cold upon the health of the community was very great. In London, the number of deaths referred to diseases of the respiratory organs increased to 799 in the week ending December 20th, and exceeded the week average by 228. The public journals record the fact that several persons were frozen to death in various parts of the country. The frost also caused great injury to plants, shrubs, and birds.

lute alcohol." Mr. Cohné does not mention his authority for this statement, which I find occurs in "Muspratt's Chemistry" (volume i., page 132) being reproduced *verbatim* in the new publication edited by Charles Vincent. As I stated before, I have no intention of discussing with Mr. Cohné the reason of the difference in the strength of proof-spirit as taken by him from that adopted by most chemists, but I can assure him, if he will enquire into the matter, that he will find proof-spirit at 60° F. has a density of 0.9184, and contains 49.24 per cent of absolute alcohol by weight, or 57.06 per cent by measure. If he does not care to work out the calculation for himself, he will find on reference to the "authorities" that this is the recognised strength of proof-spirit.

Mr. Cohné asserts that my formulæ give results which are incorrect. He takes the example, 95.1 per cent of alcohol by volume, what percentage of proof-spirit? Mr. Cohné works this out by my formula to 166.6 per cent of proof-spirit, and says this result "is wrong!" And yet Mr. Cohné works out the very same example (page 58), and obtains 66.1 over proof, or a difference of 0.5, due to the fundamental discrepancy in our definition of proof-spirit! Is it possible that Mr. Cohné has stumbled over the tyro's paradox that strong alcohol contains more than 100 per cent of proof-spirit? His statement that the result 166.6 "is wrong" certainly suggests that notion. What percentage of proof-spirit does Mr. Cohné make out corresponds to absolute alcohol.

Mr. Cohné writes—"How much is 20 per cent of proof, percentage by volume? By his (Allen's) formula $V = P \times 0.5706$ would give 11.46, which figure is, I suppose, intended to be deducted from the factor $57.06 = 45.6$, which in this case brings it to my (Cohné's) formula."

If Mr. Cohné maintains that 20 per cent of proof-spirit corresponds to over 45 per cent of alcohol by measure. I suppose he will next assert that 1 ton of coals may contain 45 cwts. of carbon! 11.46 per cent of alcohol by measure is the true answer, and there is no deduction from the factor either expressed or intended. It really appears that Mr. Cohné supposes the terms "20 per cent of proof-spirit" and "20 per cent under proof" to be synonymous. On page 58, Mr. Cohné works out 20 under proof to equal 45.55 of alcohol by measure, so that, according to him, spirits of 80 under proof and of 20 under proof are equally strong in alcohol.

I work out this last example by Mr. Cohné's formula and by mine that your readers may judge for themselves which is the simpler plan:—

$$\text{Cohné.}—57.27 - (20 \times 0.5727) = 45.55.$$

$$\text{Allen.}—80 \times 0.5706 = 45.648.$$

In the second case it is unnecessary to write down a single figure besides those shown. By Mr. Cohné's method it is impossible for an ordinary person to work out the answer without writing down the product of 20×0.5727 under 57.27. To quote Mr. Cohné—"It needs not the ability of a Cambridge Wrangler to decide which formula is the more concise." Neither his nor mine have much claim to originality.—I am, &c.,

ALFRED H. ALLEN.

Sheffield, Feb. 16, 1880.

CORRESPONDENCE.

ALCOHOL TABLES.

To the Editor of the Chemical News.

SIR,—Mr. Cohné (CHEMICAL NEWS, vol. xli., p. 57) maintains the superiority of his formula for alcohol calculations, asserts that mine are erroneous, and states that proof-spirit contains 49.5 per cent by weight of absolute alcohol. All three of these positions are in error, as I propose to show.

Mr. Cohné quotes from an unmentioned author that "proof-spirit has a gravity of 0.9186 at 60° F., and contains 57.27 per cent by volume, or 49.5 by weight of abso-

THE CHEMICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—The following grants have been made by the Council of the Chemical Society from their Research Fund:—

£100 to Dr. C. R. A. Wright, for determinations of chemical affinities in terms of electrical magnitudes.

£100 to F. D. Brown, for experiments on vapour-tensions.—I am, &c.,

W. J. RUSSELL.

Chemical Laboratory,
St. Bartholomew's Hospital, E.C.,
Feb. 24, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 5, February 2, 1880.

Experiments on the Compression of Gaseous Mixtures.—L. Cailletet.—From the author's experiments it appears probable that under high pressures a gas and a liquid may mutually dissolve each other, so as to form a homogeneous whole.

Remarks on the New Metals from Gadolinite and Samarskite.—M. M. Delafontaine.—See p. 72.

Artificial Reproduction of Skorodite.—MM. Verneuil and Bourgeois.—This mineral is produced by treating iron at 140° to 150° in a sealed tube with a concentrated solution of arsenic acid.

Gazzetta Chimica Italiana.
Anno 9, Fasc. 10, 1879.

Researches on Cobalt and Nickel, and on the Methods of Distinguishing them from Each Other.—G. Papasogli.—(See p. 74.)

Constitution of Ellagic Acid.—Ugo Schiff.—A hypothetical paper, not susceptible of useful abstraction.

Determination of Acetyl by means of Magnesia.—Ugo Schiff.—In the first place it is important to have a pure magnesia. Neither the *magnesia usta* nor the *magnesia alba* of commerce give good results, because they contain alkaline carbonates which are not removed without difficulty. It is necessary to make use of magnesia precipitated from the pure sulphate or chloride by caustic alkali, avoiding excess. After long and careful washing, the magnesia thus obtained is preserved in the form of a moist paste. In a flask of hard green glass are mixed about 5 grms. of this paste with 1 to 1.5 grms. of the acetylic derivative finely pulverised. A uniform paste is made with the addition of a little water, and after adding 80 to 100 c.c. more water it is heated for four to six hours. In many cases the decomposition is complete in two or three hours. The contents of the flask are then evaporated down to a third, and when cold are filtered through an aspiration filter into a conical test-glass. The residue is washed with a little water, mixed with ammonia and sal-ammoniac, and precipitated with a strongly ammoniacal solution of ammonium phosphate. The vessel is left in a warm place over night, the mixture filtered, the precipitate re-dissolved in dilute hydrochloric acid, and the operation repeated.

Action of Ozone on Certain Noble Metals.—Prof. Aless. Volta.

On Para-oxymethyl-phenyl-cinnamic Acid, and on Oxymethyl-stilben.—Dr. A. Ogliaro.—An account of the properties and composition of these compounds.

Action of Perchloride of Phosphorus on Molybdic Anhydride.—Arnaldo Piutti.—These substances react according to the equation—



On Certain Derivatives of the Naphthols.—C. Marchetti.—A description of monobromo-methylic α -naphtholate, of methylic α -naphtholate, and of methylic β -naphtholate.

Distribution of Copper in the Animal Kingdom.—Dr. Michele Giunte.—The author has detected copper in many animal species belonging to widely distinct groups.

On the Amins corresponding to α -Toluolic Alcohol.—Dr. P. Spica.—An extensive paper, not suitable for abstraction.

Preparation of Hydroxylamin.—Dr. Giacomo Bertoni.

Transformation of Hydroxylamin into Nitrous and Nitric Acids.—Dr. Giacomo Bertoni.—For these two papers also the reader is referred to the original.

An Easy and Rapid Process for Detecting at once Nitrogen, Sulphur, and Chlorine in Organic Bodies.—Dr. P. Spica.—Into a small tube, of about 5 m.m. diameter, is put a small quantity of the substance under examination along with a globule of sodium or potassium. It is heated strongly, and when the tube is red-hot the contents are thrown into a small beaker containing a few grammes of pure water. The liquid is heated, filtered, and divided into three portions. If a drop, placed upon a slip of polished silver, produces a black spot, sulphur is present. Another portion of the liquid is treated with a mixture of ferric and ferrous salt, and then with a few drops of dilute hydrochloric acid, to dissolve the ferric hydrate. A blue colouration or precipitate indicates the presence of nitrogen. The remaining portion of the liquid is treated with dilute nitric acid, and then with silver nitrate, if the results of the former tests have been negative, in order to detect chlorine (bromine or iodine). If one or both of the former tests have given positive results the liquid is heated for two minutes with its own volume of pure concentrated sulphuric acid, in order to decompose alkaline sulphide and cyanide, and silver nitrate is finally added.

Correspondenz-blatt des Vereines Analytischer Chemiker.
No. 1, January, 1880.

Analysis of Phosphates containing Silica and Fluorine.—H. Gilbert.—In the determination of silica in fluoriferous phosphates, especially apatite and phosphorite, losses are often experienced when the separation of silica is effected by digesting or evaporating the sample with concentrated hydrochloric or nitric acid, as considerable quantities of silicofluoric gas are volatilised. This loss is avoided or brought to a minimum if the calcium phosphate and fluoride is dissolved by gently heating in strongly diluted hydrochloric acid, the solution separated from silica by decantation, and the latter substance afterwards perfectly freed from ferric oxide by digestion in concentrated hydrochloric acid. The author in the analyses here quoted digests 2 grms. of powdered apatite in a platinum capsule, with 50 c.c. of dilute hydrochloric acid (1 part acid at sp. gr. 1.12 with 10 parts of water), for ten minutes on the water-bath, decants the solution through a filter, treats the residue once more in the same manner, and finally dissolves the last traces of iron by heating with concentrated hydrochloric acid, collecting the silica upon the filter which has been already used. The fluorine present in phosphorite was calculated from the lime, and was as a check determined by Penfield's method. For this purpose 5 grms. of finely-ground phosphorite was mixed in a platinum crucible with 10 grms. of powdered quartz, the mixture ignited, placed in the apparatus along with 50 c.c. of concentrated sulphuric acid and distilled. The alcoholic solution of potassium chloride was placed not in a U-tube, but in a Bunsen's washing bottle containing a little mercury, into which the delivery tube dipped. A second washing bottle, containing a similar solution, was connected as a matter of precaution, but the decomposition was completely effected in the first. The liberated silica was equally diffused in the liquid.

Analysis of Children's Foods.—N. Gerber and P. Radenhausen.—(The term "kinder-mehl" is applied to mixtures of condensed milk with the prepared flour of cereals.) The authors determine moisture by drying at 100° to 110° C. for five to six hours in a platinum capsule. The residue is afterwards incinerated at low redness, and in the weighed ash the phosphoric acid is determined by means of uranium. For fatty matter 2 or 3 grms. of the sample are placed in a filter, dried in an air-bath at 50° to

60°, allowed to cool, and extracted in Gerber's apparatus. The necessary quantity of ether is preferably poured not direct into the flask, but upon the dried sample. The extraction is complete in one to two hours. The ethereal solution is allowed to evaporate, dried at 100° to 110°, and the residue (fat) is weighed. The meal remaining on the filter serves for the determination of soluble and insoluble carbohydrates. The authors extract the residue from the last process with 250 c.c. of alcohol of 50 per cent with frequent agitation. This operation is performed in a flask, and requires about ten hours. The liquid is then filtered by means of a filter-pump, washed with alcohol of the same strength, and the whole is made up to 500 c.c. Of this, 100 c.c. is evaporated down in a tared platinum capsule. The residue, deducting the ash, when multiplied by 5, gives the soluble carbohydrates in the quantity of the sample operated upon. The residue on the filter is introduced, still moist, into a flask containing 400 c.c., covered with 200 c.c. water and 20 c.c. hydrochloric acid, and heated for three hours in boiling water, closing the flask with a cork. The inversion is then completed, and the whole is filtered through a folded filter. The filtrate is neutralised with alkali, and made up to 1000 c.c. If, on standing, albuminates, which have been again rendered insoluble by the neutralisation, should be deposited, which is generally the case, an aliquot part of the solution is filtered through an unwetted filter into a dry beaker. The solution contains the grape-sugar generated from the starch, and is titrated with Fehling's solution. In calculating the result, the authors assume 108 grape-sugar = 99 starch. The albuminates are determined as difference.

Analysis of Women's Milk.—N. Gerber and P. Radenhausen.—5 c.c. of the sample were mixed with 100 c.c. of distilled water, 3 c.c. solution of copper were added and titrated back with 2.5 c.c. potassa solution, the result being a clear liquid and a finely flocculent coagulum. This precipitate is treated as the authors have elsewhere recommended for cows' milk, and the clear filtrate is made up to 250 c.c.

An Extremely Sensitive Reagent for Lime.—This is sodium tungstate, as proposed by Sonstadt. As calcium tungstate is somewhat soluble in sodium tungstate it is necessary where very small quantities of lime are present, e.g., in the analysis of water, to add the reagent drop by drop.

Reimann's Färber Zeitung,
No. 3, 1880.

The German patent for a benzol washing machine has been officially cancelled. The machine had been known and in general use for ten years before it was made the subject of a patent. [That a patent could, under such circumstances, be obtained, is a proof of the utter uselessness of those "preliminary examinations" which the German and the American patent laws require, and which certain legal authorities wish to introduce into the English system.]

*Verhandlungen des Vereins zur Beförderung des
Gewerbflusses.* No. 10, December, 1879.

Corrosion of Metallic Zinc when used as Boiler Protector.—Dr. Kosmann.—Zinc introduced into steam-boilers to prevent incrustation proves very useful in case of selenitic waters, but as against the carbonates of lime, magnesia, and iron it is of little value. The zinc is soon rendered brittle and porous and is in course of time reduced to a powder.

Die Chemische Industrie.
No. 12, December, 1879.

Ammonia and Leblanc Soda.—Dr. Reinhold Hoffmann.—The author shows that the Leblanc process,

thanks to the recent improvements, now yields high-class alkali, containing 98 per cent of sodium carbonate. The only distinction worth naming is that the ammonia (Solvay) soda is richer in sodium chloride and insoluble matter, whilst in the Leblanc soda the portion not carbonate consists chiefly of sulphate. Dr. Hoffmann considers that the question between the two processes is essentially one of price, in which the Leblanc system has important allies in the production of hydrochloric acid and in the recovery of sulphur from the tank-waste.

NOTES AND QUERIES.

Purification of Glycerine.—Can any correspondent inform me of the best process for distillation and purification of glycerine, with the names of makers of plant.—J. R. B.

Redonda Phosphates.—Your correspondent "Unprejudiced," in the *CHEMICAL NEWS*, vol. xli., p. 58, says there are thousands of tons in this country lying idle. Will "Unprejudiced" oblige by stating where half cwt. or so can be had containing there or about 36 per cent P_2O_5 . He says this represents 80 per cent calcic phosphate.—**PHOSPHATES.**

MEETINGS FOR THE WEEK.

MONDAY, March 1st.—London Institution, 5.

Medical, 8.30.

Royal Institution, 5. General Monthly Meeting.

Society of Arts, 8. "The Manufacture of India-rubber and Gutta-percha," by Thomas Bolas, F.C.S.

TUESDAY, 2nd.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."

Civil Engineers, 8.

Zoological 8.30.

Pathological, 8.30.

WEDNESDAY, 3rd.—Society of Arts, 3. Alexander J. Ellis, F.R.S., "The History of Musical Pitch."

Pharmaceutical, 8.

THURSDAY, 4th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."

London Institution, 7.

Chemical, 8. Prof. T. E. Thorpe, "On the Relation between the Molecular Weight of a Body and its Specific Gravity when in the Liquid State."

FRIDAY, 5th.—Royal Institution, 8. Mr. H. N. Mosely, "Deep Sea Dredging and Life in the Deep Sea," 9.

SATURDAY, 6th.—Royal Institution, 3. Mr. Saintsbury, "Dryden and his Period."

TO CORRESPONDENTS.

W. Robinson.—The following work is a good illustrated text-book of chemistry in German:—"Grundzüge der Chemie in ihrer Anwendung auf das Praktische Leben," by Professor Wilibald Artus. Leipzig: Hartleben.

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THIRD SUPPLEMENTARY VOLUME

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London: LONGMANS and CO.

THE CHEMICAL NEWS.

VOL. XLI. No. 1058.

ON THE SOLUBILITY OF SOLIDS IN GASES.*

By J. B. HANNAY, F.R.S.E., F.C.S., and
JAMES HOGARTH, F.C.S.

THE research which we now have the honour of submitting to the Royal Society, and of which a preliminary notice was published in the Society's *Proceedings*,† was instituted with the view of throwing some further light upon what Dr. Andrews calls the "critical state" of matter. In his famous Bakerian lecture, after a very careful discussion of the question, Dr. Andrews says:‡—"Carbonic acid at 35.5° C., and under a pressure of 108 atmospheres, stands nearly midway between the gas and the liquid, and we have no valid grounds for assigning it to the one form of matter any more than to the other." This conclusion is based on the relation of volume to pressure for different temperatures, and on the failure of optical tests to show any distinction between gas and liquid when pressure is applied above the critical temperature. It occurred to us that some knowledge might be gained by examining the solvent properties of some fluid for a non-volatile solid, in the passage of the solvent from the liquid to the gaseous state. The property of dissolving solids being supposed peculiar to liquids, it seemed to us that on passing through the critical state there would be a precipitation of solid, or if, on the other hand, no separation occurred, a further proof of the perfect continuity of the gaseous and liquid states would be presented. To put the matter to the test, a solution of potassic iodide in alcohol was prepared, and a strong tube filled to about one-half with the solution. After sealing, the tube was placed in an air-bath, and heat applied. No precipitation of solid could be seen, even at a temperature of 350° C., more than 100° C. above the critical point of alcohol.

For the examination of the phenomena of solution some form of pressure apparatus was necessary, so that the volume of the fluid experimented on might be altered at pleasure. After several experiments we adopted a modified form of Dr. Andrews's apparatus, which allowed us to change an experimental tube in a few minutes, and showed no sign of leakage at the highest pressure reached. It is made of wrought-iron tubing, such as is used in hydraulic machinery, about $\frac{1}{2}$ inch internal, and 1 inch external diameter. The length of the apparatus is 9 ins., and there is a side tube welded on, measuring about 3 ins., for the insertion of the manometer tube. On the ends of the tube are screwed strong caps, also of wrought-iron; through one of these the pressure screw works, the others serve for the admission of the experimental and manometer tubes. The distinguishing feature of the apparatus is the packing for the screw and tubes. It consists of a solid plug of india-rubber, about $\frac{1}{2}$ inch thick, placed in the hollow cap, so that it serves both for packing the screw or tube, and for making the joint between the cap and the iron tube tight. To prevent the pressure screw from cutting the india-rubber, a lining of thin leather, well soaked in lard, was placed between it and the rubber. The tubes were similarly packed, and to keep them from being forced out through the aperture in the cap a thickening was made in the glass by fusing and pressing together, and between this bulbous part and the iron a strong leather washer was placed. In some cases, when a very high pressure was wanted, the tube was cemented in with

oxychloride of zinc, which effectually prevented it from coming in contact with the iron cap. With this mode of packing the pressure screw did not require to be very fine, the one commonly used being $\frac{1}{4}$ inch in diameter and screwed 30 threads to the inch. The only limit to the pressure was the frailty of the glass tubes: we have, however, attained a pressure of 880 atmospheres as shown by an air manometer, and even at this enormous pressure the screw could be freely removed in either direction without leakage. The apparatus was as tight with mercury as with water, so that there was no necessity for using two liquids as in Dr. Andrews's machine. When a high temperature was required the experimental tube was bent over, as shown in the drawing, and passed into an air-bath formed of two concentric iron cylinders. The cylinders had mica windows, through which the experiment could be observed, and to ensure perfect regularity of temperature the inner one was lined with cement about $\frac{1}{2}$ inch thick. As in most of our experiments it was quite unnecessary to know the pressure; a simple straight tube was used instead of the T-shaped one described. It was screwed to fit the same caps, and from its simplicity made a very convenient and efficient apparatus.

The experiments with the sealed tube showed us that no deposit of solid occurred above the critical point. With the assistance of the apparatus just described we were able to show a crystal of potassic iodide dissolving in alcohol gas (using the word gas, as Dr. Andrews proposes, for a fluid at any temperature above its critical point). Having fixed a tube (internal diameter 0.75 m.m., external 7 m.m.) in the apparatus previously filled with mercury, some alcohol was sucked in by gradually withdrawing the pressure screw. By a further withdrawal of the screw the little thread of alcohol was moved inwards past a constriction made in the glass tube near the end, and a small fragment of fused potassic iodide dropped in so as to rest on the narrow neck. Sufficient heat was now applied to boil the alcohol and expel the air, and the end of the tube strongly sealed with the blowpipe. The tube being placed in its position in the bath, heat was applied, and the pressure regulated so as to keep the alcohol from passing round the bend into the other limb of the U-tube. The tube and its contents were thus raised to a temperature of 300° C. (the critical point of alcohol, as will be shown, is 234.4° C.), the crystal never having come in contact with the liquid alcohol. Pressure was now applied until the alcohol was reduced to about the volume occupied by it while still liquid, when the fragment of iodide was seen to dissolve gradually and completely. On withdrawing the screw slowly no deposit occurred even when the volume was very considerably increased, but by taking off the pressure very rapidly a crystalline film appeared on the glass, and in some cases a cloud of fine crystals floating in the menstruum. These crystals could be easily re-dissolved by again increasing the pressure. The deposit most frequently occurred just at the surface of the mercury, and this we ascribe to a local rarefaction caused by the slightly higher temperature of the mercury. The fact of the existence of solvent power above the critical point being established by this experiment, we next sought to examine the phenomenon by using other solvents and solids. The sealed tube method was found most convenient for this purpose, an experiment with the pressure apparatus being resorted to when we wished to vary the volume. The tube was filled from half to two-thirds with the solution to be tested, and by a few experiments the strength of the solution was so adjusted that no separation of solid took place, when the tube was completely filled by expansion.

The behaviour of a solution of potassic iodide we have already described. Bromide of potassium gave a similar result. The solubility of chloride of calcium in alcohol was found to be uninterrupted by the passage of the alcohol through the critical state. Cobaltous chloride (anhydrous) remained in solution at 320° C., the highest temperature reached in this experiment, and the beautiful

* Extracts from a Paper read before the Royal Society, February 26th, 1880.

† *Proc. Roy. Soc.*, vol. xxix., p. 324.

‡ *Phil. Trans.*, 1869, p. 588.

blue colour of the solution was unimpaired. Ferric chloride (sublimed) gave a yellow fluid at a temperature of 260°C . This yellow colour rapidly disappeared, and a reddish-coloured powder separated out, which remained insoluble on cooling. On opening the tube a strong ethereal odour was perceived. This was most probably due to the formation of ethylic chloride. Some peculiar phenomena observed when heating the ferric chloride solution deserve description. At a temperature of 120°C . a very strongly coloured liquid layer appeared at the bottom of the tube, while the uppermost layer was perceptibly paler. As the temperature rose the two layers gradually mixed, and the fluid was quite homogeneous at and above the critical point.

A solution of ferric chloride in ether was distinctly coloured at least 30°C . above the critical point, but the colour gradually faded, and did not return on cooling. The separation and re-dissolving of part of the ferric chloride occurred also with ether.

A solution of sulphur in carbon disulphide showed no separation of sulphur when raised 50°C . above its critical point. Of course sulphur at this temperature is a liquid, but its vapour tension is comparatively so small that its solution is worth recording. Selenium also remained in solution in carbon disulphide above its critical point, and separated out in part when the solution cooled. Chemical action evidently took place in this experiment, for on evaporating a portion of the solution after heating a reddish yellow residue containing selenium was obtained. The liquid had a distinct horse-radish smell, and on burning gave a dark stain on a piece of porcelain held in the flame. Sulphide of selenium was most probably formed. An attempt was made to dissolve cadmic sulphide in carbon disulphide, but without success. In an experiment with metallic arsenic and carbon disulphide the arsenic seemed to dissolve; but as an orange-coloured film appeared on the glass on cooling, we are inclined to doubt its solubility. When some sulphur was enclosed in the tube with arsenic and carbon disulphide, and the temperature raised above the critical point, solution occurred, and when the temperature was allowed to fall a separation of dense liquid drops took place at the critical point. At a lower temperature these solidify and form reddish yellow crystals. The apparent solubility of arsenic was thus probably due to its conversion into a sulphide, and the subsequent solution of the sulphide formed.

Carbon tetrachloride was also tried as a solvent; but as the number of substances soluble in this fluid is very small, we can only record a few negative experiments. Cupric chloride did not dissolve, but appeared to be acted on chemically above the critical point, its colour changing to brown. Chromic chloride showed no sign of solution. Zinc oxide did not dissolve, but at a temperature 20° or 30° above the critical point a sudden chemical change occurred, and as a result the critical point was found to be lowered from 280°C . to about 200° . On cooling some transparent globules were seen, and on opening the tube there was an outrush of gas. The transparent globules dissolved in water, and were found to be chloride of zinc.

The use of water as a solvent was quite out of the question, both on account of its high critical point and the corrosion of the glass tubes, so that we were debarred from the use of the liquid whose examination promised most.

The mixture of light paraffins known as paraffin spirit was tried as a solvent for some organic substances. A solution of resin in this liquid remained perfectly bright and without sign of separation at a temperature of 350° to 360° , almost 100° above the critical point.

The examination of liquids to determine their critical temperature and pressure was not exactly in the line of our research; but as Cagniard de la Tour's numbers, so far as we are aware, are the only ones on record, we have made a number of experiments in this direction. To obtain accurate measurements of pressure is a matter of very great difficulty, and as for our purpose these were not

required, it would have made our research unnecessarily tedious to attempt it. Our numbers are as follows:—

	Alcohol.	Carbon Disulphide.	Carbon Tetrachloride.
Mean temperature ..	234.6°	272.96°	277.9°
Number of experiments	47	12	4
Probable error of a single experiment ..	2.06°	1.34°	0.69°
Mean pressure ..	65 atmos.	77.9 atmos.	58.1 atmos.
Number of experiments	14	12	4
Probable error of a single experiment ..	1.0 atmos.	1.5 atmos.	0.55 atmos.

The alcohol used had a specific gravity of 0.7949 at 15.5°C . In the carbon tetrachloride experiments the mercury was attacked, but our numbers were taken as quickly as possible, and before chemical action had taken place to any great extent.

Cagniard de la Tour gives the following results for alcohol and carbon disulphide:—

Alcohol	158.7°C .	119.0 atmospheres.
Carbon disulphide	275.0°C .	77.8 "

The differences between Cagniard de la Tour's numbers and our own in the case of alcohol are no doubt due to the difficulty of obtaining that body anhydrous. As will be seen from the density given, the alcohol used in these experiments contained only a very small quantity of water, and was indeed practically anhydrous.

To determine whether the presence of a solid in solution affected the critical temperature and pressure, a cold saturated solution of potassic iodide in alcohol was prepared. Experiments with this solution gave as the average of three trials—temperature 237.1°C ., pressure 71.1 atmospheres. The probable error of a single experiment is for the temperature 0.37°C ., for the pressure 0.5 atmosphere. The exact amount of solid present in solution when the readings were made it was impossible to determine; for in altering the volume, so as to observe the critical point, a small quantity was deposited on the glass just at the end of the mercury column. To make sure that some salt did remain in solution we brought the mercury column up so as to cover the crystalline film, and, after carefully examining to see that no solid particles were above the mercury, we allowed the fluid to cool under pressure. When cold the end of the capillary was broken off, and the liquid, which had been shut off by the mercury from contact with the crystalline deposit, received in a watch-glass. On evaporation it gave an appreciable residue. These experiments seem to show that a liquid has its critical point raised by the solution in it of a solid.

As the solubility of potassic iodide in alcohol is very slight (1 part in 40 at 13.5°C .) we thought it advisable to try a solution of chloride of calcium in alcohol, so that the effect of solution on the pressure and temperature might be shown in a more marked degree. On heating a cold saturated solution of calcic chloride (anhydrous), a dense liquid layer was observed to form at the bottom when the temperature reached 230°C .; at 240°C . the line separating the lighter layer of liquid from vapour was very indistinct, and could not be distinguished at all above 250°C .; at 255°C . the dense layer dissolved, but reappeared when pressure was taken off. When the volume was again reduced it re-dissolved, and at 270°C . expansion caused a deposition of crystals. In this experiment there was no sharp critical point, and no pressure readings were taken. A dense layer, it will be remembered, was also noticed in the ferric chloride experiment. A combination of the chloride with part of the alcohol evidently occurs, and at a higher temperature diffusion takes place until the fluid is quite homogeneous.

While working with cobaltous chloride an interesting question presented itself to us—Is the absorption spectrum of a substance dissolved in a fluid above its critical point the same as in liquid solution or in the solid state? To

answer this question a solution of anhydrous cobaltous chloride was sealed in a tube as before, and its spectrum observed while heat was applied. No change could be detected in its passage through the critical point, and a careful comparison of the spectra of the same solution at 15° C. and at 300° C. showed no difference beyond the fainter and more nebulous character of the bands caused by expansion. The position of the bands was not changed. Chloride of cobalt, however, was not well fitted for our purpose, the measurement of the bands being rather uncertain on account of the nebulous nature of their edges. At the suggestion of Prof. Stokes, we turned our attention to the product of the decomposition of chlorophyll by acids. The beautiful spectrum of a solution of this body in alcohol was quite permanent, even at a temperature of 350° C., although when heated in air it decomposes below 200° C. A direct comparison of the hot and cold solutions failed to show any change in the position of the lines.

A good opportunity of testing the question further seemed to be offered by the blue solution of sodium in liquefied ammonia, described by Gore.* To prepare this we first tried compression of the gas by the pressure apparatus; but the quantity of liquid got by this method was too small for experiment. We next tried a combination of Gore's method with the pressure apparatus. A tube, shaped as in the drawing, had the wide part charged with chloride of calcium saturated with ammonia, and the end sealed. By applying slight heat the air was expelled by the liberated ammonia. A small piece of sodium was then placed in a pocket near the end of the experimental tube, and, after sealing, the tube was placed in the pressure apparatus and surrounded with mercury. The iron tube was now heated by flaming with a Bunsen, and the experimental tube cooled in a freezing mixture. Distillation of ammonia commenced, and when a sufficient quantity of liquid was obtained the pressure screw was advanced, until, coming in contact with the wide tube, it broke the narrow neck joining it with the experimental tube, and allowed the mercury to rush in and enclose the liquefied ammonia. Further pressure brought the ammonia in contact with the sodium and solution commenced, but an unforeseen difficulty caused the experiment to fail, for the sodium solution coming in contact with the mercury was rapidly decolourised by it. It was therefore necessary to dispense with the use of mercury. Several methods were proposed, and we finally arrived at the following simple and effective one:—A piece of tubing, about $\frac{1}{4}$ inch diameter, was joined to a piece of marine barometer tubing, and just beside the join the latter was drawn out to an exceedingly fine bore. The wide tube was now charged with ammonia-saturated chloride of calcium, and the end sealed. A small piece of solid paraffin was then dropped into the capillary tube so that it lay next the drawn-out portion. When all air was expelled by heating slightly, a few fragments of sodium were placed in the tube, and the whole sealed. The end containing the sodium being placed in a freezing mixture, the wide tube was carefully heated until sufficient ammonia was distilled over. When re-absorption commenced the fragment of paraffin was melted by the application of a very small flame, and capillary attraction caused it to run into the fine bore, where it solidified and prevented any passage of ammonia. The tube now containing a solution of sodium in ammonia was next cut off near the join, and the end sealed in the blowpipe flame, the paraffin plug preventing outward pressure. On heating the tube thus prepared in the air-bath, the blue colour disappeared before the critical point was reached, and the loss of colour was accompanied by the deposit of a white powder. It was at first thought that this was due to the presence of water in the ammonia, but many experiments, in which every care was taken to have the ammonia perfectly dry, failed to give any other result. In one case only, when the temperature had been raised very rapidly, did the blue colour remain above the

critical point, and this only for a few seconds. This chemical action most probably consists in the formation of sodamide, and this view is supported by the fact that one of the tubes opened under mercury gave a quantity of gas not dissolved by water.

As the solution of an alkali metal seemed best adapted for spectroscopic examination, we made an attempt to dissolve sodium in compressed hydrogen. For this purpose a tube with a very large expansion was made, and filled with carefully dried hydrogen. While the stream of gas was still passing, a small piece of sodium was placed in a pocket at the end of the tube, and the aperture closed before the blowpipe. In this experiment we used no manometer, our intention being to increase the pressure slowly and to make it as great as possible. After about half an hour, at a pressure so far as we could judge of about 300 atmospheres, the mercury, which had never come in contact with the solid sodium, was observed to become thick and pasty. At this point the tube burst (no further pressure had been put on) close to the iron cap, and on examining the broken piece small semi-solid globules of mercury were observed adhering to the glass, marking the highest point to which the mercury had reached. These were taken out and placed in water, when small bubbles of gas were observed rising from them. In another experiment a small rod of sodium, quite filling the capillary bore, was observed to be gradually corroded, until only a shell adhering to the glass remained. The very frequent bursting of the tubes, made more frail by the reduction of lead oxide, due to the action of hydrogen on the hot glass, makes the demonstration of the solubility of sodium in hydrogen exceedingly difficult, but the phenomena described, we are convinced, can only be explained on this hypothesis. In several experiments we have seen little pieces of sodium slowly disappear, leaving only the thin shell of oxide formed round them while they were being placed in the tube. There seemed in many cases to be a sudden chemical action, for the tube burst, not while pressure was being applied, but often several minutes after the pressure screw had been advanced. A beam of light passed through the tube showed no absorption when examined by the spectroscope.

Having described our experiments, it remains only for us to consider the state of the solvent at a distance above the critical point. We cannot do better than quote Dr. Andrews's consideration of the subject. He says,* "What is the condition of carbonic acid when it passes at temperatures above 21° C. from the gaseous state down to the volume of the liquid without giving evidence at any part of the process of liquefaction having occurred? Does it continue in the gaseous state, or does it liquefy, or have we to deal with a new condition of matter? If the experiment were made at 100° C., or at a higher temperature, when all indications of a fall had disappeared, the probable answer that would be given to this question is, that the gas preserves its gaseous condition during the compression, and few would hesitate to declare this statement to be true, if the pressures, as in Natterer's experiments, were applied to such gases as hydrogen or nitrogen. On the other hand, when the experiment is made with carbonic acid at temperatures a little above 31° C., the great fall which occurs at one period of the process would lead to the conjecture that liquefaction had actually taken place, although optical tests carefully applied failed at any time to discover the presence of a liquid in contact with a gas. But against this view it may be urged, with great force, that the fact of additional pressure being always required for a further diminution of volume, is opposed to the known laws which hold in the change of bodies from the gaseous to the liquid state. Besides, the higher the temperature at which the gas is compressed the less the fall becomes, and at last it disappears." Leaving out of consideration the solubility of sodium in hydrogen, we have had iodide of potassium in solution in alcohol at temperatures much

* *Proc. Roy. Soc.*, vol. xxi.* *Phil. Trans.*, 1869, p. 575.

too high for measurement by the mercurial thermometer, and which may safely be estimated at 150°C . above the critical point ($=380^{\circ}\text{C}$.). We think therefore we are justified in saying that at this high temperature the solvent was truly gaseous. An examination of Dr. Andrews's curves for carbonic acid shows us that at a temperature only 16°C . above the critical point, the sudden fall in volume corresponding to the boiling line has disappeared, and although the curve does not coincide with the air curve, it is regular, and shows no sign of liquefaction having occurred. Our experiments were made at temperatures further removed from the critical point than this, and as no sudden change in solubility could be detected in passing from the liquid state to these high temperatures, we conclude that, in these experiments, we have further proof of the perfect continuity of the liquid and gaseous states, and also a complete proof of the solubility of solids in gases.

ON THE ARTIFICIAL FORMATION OF THE DIAMOND.*

By J. B. HANNAY, F.R.S.E., F.C.S.
PRELIMINARY NOTICE.

WHILE pursuing my researches into the solubility of solids in gases, I noticed that many bodies, such as silica, alumina, and oxide of zinc, which are insoluble in water at ordinary temperatures, dissolve to a very considerable extent when treated with water-gas at a very high pressure. It occurred to me that a solvent might be found for carbon; and as gaseous solution nearly always yields crystalline solid on withdrawing the solvent or lowering its solvent power, it seemed probable that the carbon might be deposited in the crystalline state. After a large number of experiments it was found that ordinary carbon, such as charcoal, lampblack, or graphite, were not affected by the most probable solvents I could think of, chemical action taking the place of solution.

A curious reaction, however, was noticed, which seemed likely to yield carbon in the nascent state, and so allow of its being easily dissolved. When a gas containing carbon and hydrogen is heated under pressure in presence of certain metals, its hydrogen is attracted by the metal, and its carbon left free. This, as Prof. Stokes has suggested to me, may be explained by the discovery of Profs. Liveing and Dewar, that hydrogen has at very high temperatures a very strong affinity for certain metals, notably magnesium, forming extremely stable compounds therewith.

When the carbon is set free from the hydrocarbon in presence of a stable compound containing nitrogen, the whole being near a red-heat and under a very high pressure, the carbon is so acted upon by the nitrogen compound that it is obtained in the clear, transparent form of the diamond. The great difficulty lies in the construction of an inclosing vessel strong enough to withstand the enormous pressure and high temperature, tubes constructed on the gun-barrel principle (with a wrought-iron coil), of only half an inch bore and 4 inches external diameter, being torn open in nine cases out of ten.

The carbon obtained in the successful experiments is as hard as natural diamond, scratching all other crystals, and it does not affect polarised light. I have obtained crystals with curved faces belonging to the octahedral form, and diamond is the only substance crystallising in this manner. The crystals burn easily on thin platinum-foil over a good blowpipe, and leave no residue, and after two days' immersion in hydrofluoric acid they show no sign of dissolving even when boiled. On heating a splinter in the electric arc it turned black—a very characteristic reaction of diamond.

Lastly, a little apparatus was constructed for effecting a combustion of the crystals and determining their compo-

sition. The ordinary organic analysis method was used, but the diamond crystals were laid on a thin piece of platinum-foil, and this was ignited by an electric current, and the combustion conducted in pure oxygen. The result obtained was, that the sample (14 m.grms.) contained 97.85 per cent of carbon—a very close approximation, considering the small quantity at my disposal. The apparatus and all analyses will be fully described in a future paper.

(Received February 25, 1880.)

Extract from a letter from Mr. Hannay, dated Feb. 23:—"I forgot, in the preliminary notice, to mention that the specific gravity of the diamond I have obtained ranges as high as 3.5; this being determined by flotation, using a mixture of bromide and fluoride of arsenic."

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.
(Continued from page 94.)

VI. ON THE USE OF THE PERIODIC LAW FOR THE COM- PLETION OF OUR KNOWLEDGE RELATIVE TO THE FORMS OF CHEMICAL COMPOUNDS.

AT the time when the discovery of metaleptic phenomena created a change in the ideas of chemists, all the chemical reactions were considered as cases of substitution, and the theory of types was drawn up under several forms. Gerhardt was even inclined to think that the reaction by which C_2H_4 combines with Cl_2 was an act of substitution of H by Cl, and a formation of $\text{C}_2\text{H}_3\text{Cl} + \text{HCl}$. Everything which did not correspond with these ideas was declared to be molecular addition. $\text{C}_2\text{H}_3\text{Cl} + \text{HCl}$ in the preceding example should, according to this, form a whole body like $\text{CNa}_2\text{O}_3 + 10\text{H}_2\text{O}$. Although the interpretation of chemical phenomena has, thanks to the theory of types, acquired a simplicity and harmony which was before impossible, this theory nevertheless shared in a measure the fate of the doctrines which preceded it, and that was because the notions on which it was based were expressed in a too absolute manner, and because it neglected a large class of phenomena and considered them to be molecular additions. It was not until attention was turned towards the study of metallo-organic compounds (towards 1855), in which the phenomena of substitution and addition* are represented both in the same degree, that the phenomena of addition commenced to receive the attention they deserved: Frankland, Cahours, &c., have introduced to chemistry the idea of there being a limit to chemical compounds. These ideas have since been applied to the carbon compounds.†

The ideas which have been entertained about *limits*, about *saturated* compounds, about the aptitude of *non-saturated* compounds to show phenomena of addition, ideas connected with the opinions relating to substitutions, correspond, in my opinion, to the most complicated problems that chemical investigation can propose. However, the theory of limits, in its primitive form and free from everything tending towards hypotheses, has not settled itself in the science of chemistry; for it is in the destiny of science that the most important discoveries of an era lead first of all to extreme hypotheses. I consider as such the theory of atomicity at present in vogue.

Couper, later on Kékulé, and after them others, in comparing the composition of saturated compounds of an element with chlorine or hydrogen, have thought that each

* I have treated in detail, in the *Bulletin de l'Académie des Sciences de St. Pétersbourg*, 1858, the differences between the phenomena of addition and substitution in the sense of the unitary theory, and bearing in mind the modifications arisen from the physical properties

† Notably in my memoir, "Essai d'une Théorie sur les Limites" (*Ibid.* vol. v., 1861.)

* A Paper read before the Royal Society, February 26, 1880.

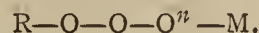
element was capable only of saturating a certain number of equivalents of all the other elements. But not stopping there, these investigators, and afterwards their successors, thought that it was possible to deduce the preceding hypothesis from determined conditions relative to the fixation of elements in the molecule. Everybody is aware of the excellent results which have been arrived at in starting from this hypothesis in the study of isomerics: but all the same it must not be forgotten that other hypotheses, now neglected, have had these periods of glory and popularity. The discoveries due to adepts of the electro-chemical theory have been shown up in a new manner by the theory of types. But the discoveries concerning isomerics are completely explained without it being necessary to have recourse to the hypothesis of the combination of elements in the molecule by parts of their affinity. We have a very simple manner of showing this circumstance, which was discovered and interpreted exactly from cases of isomerism in the aromatic and other compounds, even without using the preceding theory, and uniquely by a clear idea of substitutions. Below are given some considerations directed against the different forms of the hypothesis of valence or of atomicity.

1st. The corner-stone of this hypothesis is the division of all chemical compounds into atomic and molecular compounds; but the hypothesis only touches on the compounds of the first mentioned kind; molecular compounds are neither considered nor generalised. The above-mentioned division—a division which has long existed—lacks any solidity; it is so artificial and so arbitrary that even chloride of ammonium and perchloride of phosphorus are by some people considered as molecular compounds. In reality, there is not one single character which sharply defines the limit of the category of the above-mentioned compounds. That they contain complete independent molecules, one by the side of the other; that they are incapable of being turned into vapours; that there is only a feeble modification in the chemical reactions when combination takes place;—all these are insufficiently distinct characters; if not, $\text{PtCl}_4 = \text{PtCl}_2 + \text{Cl}_2$ and KClO_4 , &c., might be considered as molecular compounds. But if it is impossible to establish any limit between the molecular and atomic compounds, the possibility of fixing the equivalence or the atomicity of element, above all when based on the compounds with hydrogen and chlorine, disappears. PtCl_4 is not a limited compound, for it can still combine with $2\text{HCl} + 6\text{H}_2\text{O}$ or with $8\text{H}_2\text{O}$, and other molecules can be substituted for these last-named. The formula of hydro-fluosilicic acid, SiH_2F_6 , which is reproduced for many elements, shows, so to speak, the hexatomicity of silicium, whilst this element, compared with hydrogen and chlorine, is tetratomic. To prove that this form of combination is not determined by the presence and by the particular properties of fluorine, it suffices to mention PtH_2Cl_3 and the corresponding salts, as well as the double cyanides.

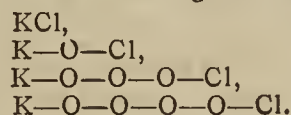
2nd. To appreciate the atomicity of elements, we start with combinations with hydrogen, although out of the sixty-three elements which are known up to the present seventeen only have been combined with it. Further, hydrogen only gives one compound with one atom of an element, whilst other elements, and even chlorine, can furnish several compounds. Therefore, it is at least of not much practical value to judge compounds of an element with other elements according to its compounds with hydrogen. Carbon itself only forms with H one compound, CH_4 , and not even CH_2 , although it gives with oxygen CO_2 as well as CO . The existence of molecules SnCl_2 and SnCl_4 , HgCl and HgCl_2 , PCl_3 and PCl_5 has always embarrassed the partisans of the hypothesis of the atomicity of elements, precisely because we started from compounds with hydrogen, and because hydrogen only gives one form of combination.

3rd. All the elements except fluorine can enter into combination with oxygen. These compounds are the most widely spread of any in Nature. Their chemical

character is very distinctly shown; many elements give several compounds with oxygen. However, the hypothesis of atomicity does not furnish us with any law relative to the number of atoms of oxygen, because, according to this hypothesis, oxygen can enter into any complete molecule in its character of a biatomic element. A given body, RM, can receive an indefinite number of molecules of oxygen; it then forms what the hypothesis in question calls a chain—



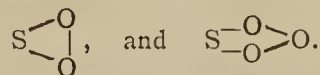
The best example which can be given is the following:—



and

In other cases this hypothesis is not considered admissible; thus there is no chain in CO_2 . The instability of the cateniform compounds of oxygen cannot serve as a distinctive character, for CO_2 , H_2O , Cl_2O , are all one as easily decomposed as the other, although at different temperatures. PtO_2 , again, is more easy to decompose than KClO_3 , in spite of the admitted difference relatively to the mode of fixation of the atoms. Further, we cannot imagine why KClO is more unstable than KClO_3 , nor why the longest chain, KClO_4 , is more stable than the shortest chains. But what there is worthy of note is that the forms of combination of oxygen, like hydrogen and other elements, are held between narrow and analogous limits. This may be noticed at first sight if we take care to put next to each other those forms of combination which contain the most oxygen, OsO_4 , KClO_4 , K_2SO_4 , K_3PO_4 , K_4SiO_4 . The periodic law, in fixing the limit of oxygenated compounds, fills up an important gap in the atomic theory.

4th. The most logical partisans of the above-mentioned hypothesis are those who see in atomicity a fundamental or *invariable* property of atoms. As a rule, they further hold that free affinities cannot exist in molecules. They consider carbon to be invariably tetratomic, and nitrogen always triatomic, &c. However, it is necessary to give in before facts. Thus, at present, the greater number of partisans of atomicity consider this property to be variable. In plainer words, to admit that the atomicity of an element is variable is almost to renounce the hypothesis in question; this hypothesis is no longer anything but that of the number of equivalents in an atom; atomicity varies like the equivalents of elements, according to the law of multiple proportions. If atomicity is a variable property, if we admit that in the greater number of cases a part of the bonds of affinity is latent, we must give up all idea of determining the atomicity. Sulphur was long considered to be a diatomic element, and this diatomicity was deduced from the compounds SH_2 , SHK , SK_2 , SCl_2 ; and SO_2 and SO_3 were declared to be compounds in the form of a chain,—



But in this manner the well-known and distinct analogy between SO_2 and CO_2 , in which different structures were admitted, remained inexplicable. Further, the analogues of sulphur, Te and Mo, give TeCl_4 and MoCl_5 . The compound SAe_3I was also discovered, so that sulphur was looked upon as being also tetratomic. Let us add that the existence of WCl_6 and SO_3 caused sulphur to be considered as hexatomic; perhaps Cl may sometimes be heptatomic. What is it that proves to us the hydrogen and oxygen cannot be polyatomic? The first severe blow was dealt to this theory when variable atomicities, free and latent bonds of affinity were admitted; the fundamental principles on which the hypothesis is built were shaken to the foundations.

5th. After having lost, by the adoption of variable atomicities, the value of a rigorously scientific system, and after being changed into a tabulated form of equivalents,

the above-mentioned hypothesis could still hold its position in science as a means of representing the forms of combination, on condition of not including the *junction* of elementary atoms in the molecule by means of their bonds of affinity. This part of the hypothesis cannot be put in harmony with the much more certain notions which are now entertained on the construction of matter and on the laws of attraction; we must therefore reject it absolutely. We ought to represent the atoms in a molecule as being in a certain state of unstable equilibrium of reciprocal reactions. The entire system is maintained by forces which belong to each individual particle; for we can only imagine two parts of a whole to be uniquely under the influence of a third part, without giving rise to reciprocal influence, particularly if all that we know concerning these two parts shows that they exercise a distinct and constant chemical action. If we admit that in CH_4 the 4H are only held by one atom of carbon, this idea is not, so to speak, unreasonable in itself; but if one H is replaced by Cl it is difficult to admit that the four atoms (H_3Cl) are held only by the carbon—that the reciprocal powers of attraction of Cl and H remain dormant during or after the reaction. This proposition is not admissible except on the ground that Cl is nearly three times as heavy as carbon. If the term “junction” should only be a conditional expression of the distribution of the elements in space; if it signifies that in the just-mentioned example Cl occupies the *same position* as H—that is to say, that it fulfils the same conditions with respect to carbon and other elements—then this would only be a repetition of what has been stated since the discovery of metalepsy. I have often heard the polyatomic elements, and the elements combined with them, compared to the sun and planets; but this comparison is not tenable, because if one planet were replaced by another, whose mass would be even greater than that of the sun itself, the planetary system would not for that reason cease to exist. The partisans of atomicity may suppose that chemical attraction, differently to universal gravitation, does not depend on the mass, but only on the atomicity of the elements. The periodic law in recognising a strict dependence between the atomicity and the mass of atoms completely annuls such an hypothesis, and authorises us to suppose that chemical attraction, like any other attraction, depends upon the masses.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Ordinary Meeting, February 28, 1880.

Prof. W. G. ADAMS in the Chair.

A PAPER was read by Mr. RIDOUT on “*Some Effects of Vibratory Motion in Fluids.*” It was found by Savart and Tyndall that jets of water were sensitive to notes, or air vibrations, like flames, and the author conceived the idea of vibrating the jet of water from within. To do this he caused an electro-magnetic arrangement to pinch the tube conveying the water 400 to 500 times per second so as to communicate a vibratory motion to the stream of fluid. The issuing jet spread out in two streams, beautifully broken into drops, and representing the fundamental note. When the pinching lever vibrated irregularly, harmonics were observed. When the water was thrown into vibration in two different planes the resulting jet rotated in the tube. Froude’s deduction that a liquid moving in a tortuous tube has a tendency to straighten the tube was illustrated by oscillating a pipette with its nozzle in a vessel of water, and filling a coloured liquid into it, which is seen to flow from the nozzle through the water in a

tortuous line. By giving the pipette also a motion round its axis the line becomes a spiral; a sounding body produces no disturbance in the stream. The author also showed that the cardboard experiment of M. Clement Desormes can be extended to water. In this experiment a card is attracted to another card by blowing a jet of air through the latter upon the surface of the former. Mr. Ridout allows a jet of water to flow out of a glass tube with a cup-shaped mouth upon the surface of a glass ball, and when the ball is within a certain distance of the mouth it is attracted towards the latter and sticks in the mouth. In explanation of this fact it was shown that the ball and cup remained in such a position that the outflow of water was greater than if the globe had been entirely absent.

Prof. PERRY explained this action by the hydrodynamical fact that the pressure is less at the centre of the mouth of the cup than at the edges.

Prof. GUTHRIE said that he had tried a similar experiment with a funnel-shaped mouth and a glass cone, but failed. He surmised that perhaps the cohesion of the water for itself as it formed a shell round the ball might help to cause the success of the ball method.

Prof. ADAMS pointed out that with the cup and ball there was less difference of head of water between the centre of the mouth and the edge where the water escaped than with the funnel.

Dr. STONE stated that he had been able recently to imitate many physiological sounds, such as the murmur of the heart, by means of constrictions in tubes through which water and air were flowing. His demonstrations were made before the Royal College of Physicians.

Dr. C. W. WRIGHT then read an important paper on a “*Determination of Chemical Affinity in Terms of Electromotive Force.*” After giving a history of the subject, he described his original experiments. These consisted in performing electrolysis of sulphuric acid and measuring the heat evolved in the process, and by recombination of the materials. A voltmeter with spade-shaped platinum electrodes, soldered to stout copper wires, and sealed by a large plug of gutta-percha, was employed for the electrolysis. An ordinary water calorimeter was used to measure the heat given off, as Bunsen’s was found to contain sources of loss of heat. The strength of the current employed was varied from 6 webers to $\frac{6}{10}$ weber. The volume of gas produced was measured by Joule’s plan. Radiation loss was corrected for by three methods. From an average of eighteen experiments the value of e , the electromotive force, was found to be 1.5038 C.G.S. or volts. Taking the formula—

$$J = \frac{e}{(H+h)x},$$

where J is Joule’s equivalent, H is the heat actively evolved, h the heat evolved by recombination, and x a constant to which Kohlrausch gives the value of 0.000105. Dr. Wright finds that Joule’s equivalent should be 4.196×10^7 instead of 4.20×10^7 as given, to answer the formula. The author thinks that Joule’s water-friction experiments gave the truest value of J, and that his electric heating experiments gave a result about $\frac{1}{2}$ per cent too low, owing to the B.A. unit of resistance being about 2 per cent too high and other causes.

Determination of Nitrogen by the Will and Varrentrapp Process.—In case the distillate is coloured Dr. Hauffe titrates it in a flask, closed with a stopper having two orifices. Through one of these a tube passes to the bottom of the flask, serving for the introduction of the standard alkali. Through the other there passes a piece of tubing, which ends just below the stopper, and supporting a moistened piece of red litmus paper. When this is turned blue by the liberation of ammonia the reaction is at an end. The flask is kept during the process over a source of heat.—*Ber. Oest. Gesell. Förder. Chem. Industrie.*

NOTICES OF BOOKS.

The Retrospect of Medicine, being a Half-yearly Journal containing a Retrospective View of Every Discovery and Practical Improvement in the Medical Sciences. Edited by W. BRAITHWAITE, M.D., and JAMES BRAITHWAITE, M.D. Vol. lxxx., July—December, 1879. London: Simpkin, Marshall, and Co.

THIS semi-annual still pursues its course of utility. Among the most interesting facts here brought together we find several cases of poisoning from arsenical wall-papers, recorded by Mr. Jabez Hogg, and, it would seem, well authenticated. Thus, we have successive inmates of a house where brilliant green wall-papers were present seized with the same symptoms,—obstinate skin disease—which resisted medical treatment, but disappeared spontaneously when the sufferers were removed out of the house, and did not return again after the suspected papers were stripped off and others put in their place, though the workmen employed in the alterations had been attacked with cramp, sickness, &c. Thus, according to the methods of "agreement" and of "difference," it may fairly be concluded that the paper was casually connected with the disease experienced. Lastly, the presence of arsenic in the paper was directly established by analysis, and the symptoms observed in the patients were exactly such as are capable of being produced by chronic arsenical poisoning. Two at least of the other cases are equally decisive and well established. It is very much to be desired that the makers of wall-papers would, of their own motion, discontinue the use of arsenical greens, using Guignet's green, certain aniline greens, and the green of coffee (viridic acid) in their place. If they persist in the employment of arsenical colours the inevitable result will be legislative action so mismanaged as to have an injurious influence upon the tinctorial and decorative arts.

It must not be forgotten, meanwhile, that arsenic is from some inscrutable reason introduced into colours where it can play no useful part, as in umbers and sepia. The comparative rarity of cases of disease distinctly traceable to the random use of arsenic can only be explained by the fact that upon many constitutions it has little immediate effect. Still it is strongly to be suspected that we have here the source of not a few of those mysterious affections which puzzle the practitioner and embitter the life of the patient. For their own sakes, therefore, we would advise all concerned to banish arsenic wherever its presence is not at once necessary and innocuous.

Dr. G. de Gorrequer maintains the unity of the poison in scarlatina, puerperal fever, typhoid, diphtheria, and erysipelas, the results being differentiated according to the constitutional peculiarities and the special circumstances in each case. The contrary view, of a specifically distinct *contagium vivum*, can, he considers, scarcely be upheld without having recourse to the theory of abiogenesis.

In a paper on the treatment of deafness the interesting fact is brought forward that some deaf persons hear best amidst noises, sometimes, indeed, better than those whose hearing is in the normal condition. Hence it has been proposed to use, in such cases, intensified sound as a curative agent.

Concerning certain remedies of no very long standing, such as nitrite of amyl and salicylic acid, the usual conflict of opinion is going on. Some practitioners take a wide and sanguine view of their utility, whilst others are equally impressed with their shortcomings and dangers. Time must elapse before they can find their due level.

Solubility of Alloys of Tin and Lead in Dilute Acetic Acid.—Dr. Ambühl finds that dilute acid readily extracts the lead from such alloys. Banca tin was found perfectly free from lead. English bar tin contains a slight trace.—*Schweiz. Wochensch. Pharm.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Correspondenz-blatt des Vereines Analytischer Chemiker.
No. 2, January 15, 1880.

A New Colorimeter.—C. H. Wolff.—This paper cannot be intelligibly reproduced without the accompanying illustrations.

Universal Method of Determining Arsenic.—Millot and Magneune treat the substance in which the arsenic has been detected with nitric acid. The filtered solution is evaporated to dryness, taken up in water acidulated with sulphuric acid, and introduced into a Marsh apparatus. The gases, which contain only arsenic and antimony, are passed into nitric acid, where they are oxidised. After about two hours the reaction is generally at an end, but it is well to ascertain this point by substituting for the delivering-tube of the apparatus a tube drawn out to a fine point. The flame of the escaping hydrogen should not produce the well-known spots upon porcelain. The nitric solution is evaporated to dryness and treated with water, which dissolves only the arsenic acid, leaving undissolved any antimony which may be present. In general the solution is precipitated with magnesia-mixture, and the precipitate is weighed on the filter as ammonio-arsenate of magnesia. The authors, however, prefer the following method. They dissolve 100 grms. crystalline sodium acetate with 50 grms. acetic acid, and make up with water to a litre. Secondly, they dissolve 20 grms. uranic nitrate in $\frac{1}{2}$ litre water, add a few drops of ammonia till precipitation begins, then add exactly as much acetic acid as is necessary, and make up with water to 1 litre. Thirdly, 100 grms. potassium ferrocyanide are dissolved in water to 1 litre. Fourthly, 5 grms. arsenious acid are dissolved in boiling nitric acid, evaporated to dryness, and dissolved in water to 1 litre. The last solution, which contains in 10 c.c. 0.0581 arsenic acid, serves to titrate the uranic solution. 10 c.c. of this normal solution of arsenic acid are mixed with water in a porcelain evaporating basin to about 1 decilitre, 10 c.c. of the sodium acetate solution are added, the whole brought to a boil, and the uranic solution is added drop by drop till the arsenic acid is completely precipitated as uranic arseniate. This is recognised by placing a drop of the boiling solution upon a plate and mixing it with a drop of the ferrocyanide solution, when a faint reddish colouration is produced. The volume of the uranic solution consumed is read off on the burette, 0.5 c.c. is deducted as a correction, and on dividing 0.0581 by the corrected number the contents of the solution per 1 c.c. is obtained. One c.c. of the uranic solution represents, therefore, 0.005 grm. arsenic acid. The same process is then repeated with the solution under examination. On multiplying the number of c.c. of the uranic solution, which are consumed by the value as above ascertained, the quantity of arsenic in the substance is obtained.—*Journal de Pharm. et de Chimie.*

Chemiker Zeitung.
No. 1, 1880.

Technological Pictures from Western England.—The first of this series of papers consists of a very full and accurate description of the Cheshire salt trade.

The Austrian Society for the Promotion of Chemical Industry offers a prize of 200 florins in gold for the best treatise on the action of light upon and the consequent destruction of the colouring-matters of indigo, alizarin, and rosanilin, and upon the products generated. A prize of 100 florins in gold is offered for the best research on

the peculiar colouring-matter of jute, and with the object of discovering a process for its destruction without injury to the fibre.

Detection of Free Mineral Acids in Vinegar.—Hager neutralises 20 c.c. of the vinegar with ammonia, pours it into a shallow glass capsule, and exposes it to spontaneous evaporation at a temperature of 70°. If the vinegar is pure nothing remains but a brown imponderable stain. If other acids are present there is a crystalline residue. This process is applicable only to vinegar prepared from alcohol by the "quick" method.—*Pharm. Central-Halle*.

Determination of Acetic Acid in the Acetate and Sub-acetate of Lead.—Dr. F. Salomon prepares for this purpose an acetic acid containing exactly 50 grms. acetic anhydride per litre, and a solution of potassa exactly equivalent. 10 c.c. of the solution to be examined or 2 to 4 grms. of the sugar of lead dissolved in water are placed in a flask holding 100 c.c., precipitated with an excess of the standard potassa solution, the flask filled up to the mark with distilled water, and the whole well shaken. The bulk of the lead is thus precipitated as hydrate, the residue being dissolved in the excess of alkali. After the precipitate has settled, 50 c.c. of the filtrate are mixed with phenol-phthalein, and the standard acetic acid is added till the red colour of the liquid disappears.—*Dingler's Polyt. Journal*.

Acidimetry of Coloured Acids.—Dr. F. Salomon places 2 grms. potassium bicarbonate in the caoutchouc receiver of an apparatus resembling that of Scheibler for the determination of carbonic acid, whilst 10 c.c. of the acid, diluted, if needful, are introduced into the decomposition-vessel. The proportion of acid is calculated from the carbonic acid given off.—*Dingler's Polyt. Journal*.

Bleaching by Means of Hydrocarbons.—A. Viol and G. F. Duflos bleach ostrich feathers by exposure to sunshine in an atmosphere saturated with the vapours of benzine, turpentine, or petroleum.

No. 2, 1880.

Materials, Fuel, and Labour required for the Production of 100 kilos. Ultramarine.—C. Furstenau.—For the finest, or violet quality, the cost is, for materials, 33.08 mark; for fuel, 2.31; and for labour, 9.11, giving a total of 44.5 m. These figures would assume different proportions in England.

Percentage of Anhydride in Fuming Sulphuric Acid.—C. Furstenau.—The author gives his results in the form of a table, and remarks that commercial samples may contain Glauber's salt.

New Glass Weights.—Weights of glass are now manufactured in Leipzig, and are said to be little inferior to those of rock crystal.

An Experiment on Sulphur.—T. Gross mixed the purest commercial "milk of sulphur" with linseed oil, so that to every part of sulphur there was not quite one part of oil, and heated the mixture gradually in a very wide capsule. Offensive vapours were given off, the mass swelled up strongly, and there remained a black porous body, no longer combustible, under conditions in which sulphur burns briskly. If the finely-pulverised black mass is heated with pure concentrated sulphuric acid, sulphurous acid is evolved, and there remains a liquid resembling syrupy sulphur, from which sulphuretted hydrogen throws down a substance soluble in ammonium sulphide, which Gross considers as something hitherto unknown. He states several reasons for doubting the elementary character of sulphur—a question which he is endeavouring to decide experimentally.—*Chem. Central-blatt*, x., 785.

Chloride of Zinc as a Reagent for Alkaloids.—A. Jorissen.—1 gm. of the chemically pure chloride is dissolved in a mixture of 30 c.c. concentrated hydrochloric acid and 30 c.c. water. The solution of the alkaloid or its hydrochlorate is placed upon the inverted lid of a

porcelain crucible, and evaporated upon the water-bath till every trace of moisture has disappeared. Two or three drops of the zinc chloride solution are then poured upon the dry substance, and the lid is again heated on the water-bath, when the characteristic colourations appear. Strychnin appears of a bright rose colour; thebain, yellow; narcein, olive-green; delphinin, brown-red; berberin, yellow; veratrin, red; quinin, pale-green; digitalin, chestnut-brown; salicin, violet-red; santonin, violet-blue; and cubebin, carmine-red. The alkaloids or glucosides examined should be free from admixtures.—*Monit. Industriel*.

Propagation of Disease.—According to the experiments of Dr. Bollinger, the milk of cows suffering from tubercular disease is capable of communicating this affection to human beings. As 5 per cent of cows when advanced in life suffer from this disease the danger is considerable. Boiling the milk is not a safeguard.

No. 3, 1880.

The Manufacture of Condensed Milk in Middlewich.—The author of this paper, which is a continuation of "Technological Views of Western England," declares that there is not the least foundation for the assertion of Soxhlet that the cream is removed from the milk prior to condensation.

Arsenic in Water-Colours.—G. Wagner.—The writer, a manufacturer of water-colours, protests against the notion that raw sienna containing 1½ per cent of arsenic can prove injurious to persons who lick or suck the brush in drawing. He declares that he has eaten from ½ to 1½ of a cake of the colour without experiencing any evil effects, and is prepared to repeat the dose.

The Wine Manufacture in Hungary.—A circular from a firm in Buda-Pest gives the prices of Riesling bouquet, muskatel bouquet, essence of Medoc, essence of Tokay, "red wine colour," deep and fiery, and other liquids for communicating the flavour and colour of wine to artificial mixtures. These ingredients, it is boasted, are exported to Sweden, Russia, and even to South America!

A Method for Detecting and Determining Iodine in Presence of Bromine and Chlorine.—E. Donath.—Whilst potassium dichromate occasions in solutions of potassium iodide no separation of iodine, this result is at once produced by chromic acid. A determination of the liberated iodine is not possible on account of the action of the excess of chromic acid upon sodium dithionite. The iodide of starch is precipitated by solutions of chromic acid as an almost black precipitate. Hence, starch paste cannot serve as an indicator. The iodine is therefore distilled off, and is determined in the distillate by means of hyposulphite of soda. The alkaline bromides and chlorides are not decomposed by concentrated solutions of chromic acid at common temperatures, and the chlorides not even on boiling. Dilute solutions of chromic acid decompose the bromides only to a very small extent at the boiling heat, but the proportion increases with increasing concentration. The method is therefore suitable for accurate determinations of iodine in presence of chlorine, but not when it occurs along with bromine. The solution of chromic acid employed contains from 2½ to 3 per cent, and is freed from traces of sulphuric acid by boiling with pure barium chromate.

The Solution of Arsenical and Antimonial Compounds.—E. Donath.—Rammelsberg's method of separating arsenic and antimony from the metals of the fifth group by fusion with sodium carbonate and sulphur and solution in water, has the drawback that the aqueous solutions contain highly sulphuretted compounds, which, on exposure to the air, deposit incrustations of sulphur, and on decomposition with hydrochloric acid precipitate the sulphides of arsenic and antimony mixed with much sulphur, which is very inconvenient in dissolving the arsenic sulphide, or in converting the antimony sulphide into antimonious acid. By using hyposulphite of soda, pre-

viously completely dehydrated by cautious fusion in a capsule, this inconvenience is avoided, and the sulphides thrown down from the aqueous solution of the melt are contaminated with but little free sulphur.—*Zeitschrift für Anal. Chemie.*

Injury to Vegetation by Acid Gases.—Dr. Schröder.—The poisonous action of coal smoke and the fumes of metallurgical and chemical works upon vegetation depends almost exclusively upon the effects of acid gases, especially the sulphurous and the hydrochloric, the former of which is by far the more pernicious. Hence the combustion-products of coal and lignite are the more pernicious the more sulphur is present. Wood smoke has no action upon vegetation. The leaves of trees exposed to sulphurous acid turn brown over their entire surface, whilst hydrochloric acid appears to attack principally the edges. Annual crops resist better than trees. Broad-leaved trees may be thus arranged in the order of their sensitiveness: red beech, birch, lime, poplar, alder, ash, plane, and oak. [The walnut tree is more sensitive than any of the above-mentioned, and from observations in Lancashire we should give the oak a different place.]

New Application of Methyl-chloride.—C. Vincent.—The author applies methyl-chloride, previously liquefied and purified by passage through concentrated sulphuric acid, for the purpose of extracting perfumes from flowers.

MISCELLANEOUS.

Crystallised Carbon in Abstract.—The announcement that Mr. Hannay's paper on the Artificial Formation of the Diamond was to be read at the Royal Society on Thursday, February 26, brought together a large number of Fellows and their friends. After the meeting a scrap of paper with the following lines pencilled on it was picked up not far from where the Treasurer of the Royal Society had been sitting:—

"The crystallised carbon, as diamond known,
Manufactured can be, Mr. Hannay has shown.
From some hydrocarbon set hydrogen free
Near a nitrogen compound, that stable must be,
In the presence of metal—magnesium or sodium—
And the brilliant result is the jewellers' odium."

The London Water Supply.—In his annual report on the quantity and quality of the water supplied to London during 1879, Dr. C. Meymott Tidy remarks as follows:—"The year through which we have passed has been an exceptionally trying one for the water companies—indeed, far more trying than any of which I have had experience. The excessive rainfall has rendered the Thames more frequently than usual thick and turbid. It was to have been expected that the analyses would show a larger average amount of organic matter than in previous years, and the only thing which need surprise us is that it is not greater than results prove it to be. And yet, notwithstanding this, the death rate of London for 1879 is the lowest on record. When I write my next report it is not unlikely that the water supply will have passed out of the hands of the companies, and the management be vested in some other body. I will not venture to predict the future of the London water supply, but I again repeat what I have said more than once before, that in my opinion no better sources of water can be found for the Metropolis, considering all the facts of the case, than the rivers Thames and Lea. I trust, when the companies have passed into the hands of Government, that the general management of the several works will be as good as now, and that the change the public have clamoured for, and the more efficient working that the public expect, will be seen, not merely in lessened taxation, but in the improved health of the people and in a lower death rate."

MEETINGS FOR THE WEEK.

MONDAY, March 8th.—London Institution, 5.
— Medical, 8.30.
— Society of Arts, 8. "The Manufacture of India-rubber and Gutta-percha," by Thomas Bolas, F.C.S.
TUESDAY, 9th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
— Civil Engineers, 8.
— Photographic, 8.
— Anthropological Institute, 8.
WEDNESDAY, 10th.—Society of Arts, 8. "Recent Advances in the Production of Lambeth Art Pottery," by John Sparkes.
— Geological, 8.
— Microscopical, 8.
THURSDAY, 11th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
— London Institution, 7.
— Royal, 8.30.
— Royal Society Club, 6.30.
— Society of Arts, 8. "Balmmain's Luminous Paint, by C. W. Heaton, F.C.S."
FRIDAY, 12th.—Royal Institution, 8. Dr. C. William Siemens on "The Dynamo-Electric Current and some of its Applications," 9.
— Astronomical, 8.
— Quekett, 8.
SATURDAY, 13th.—Royal Institution, 3. Mr. Saintsbury, "Dryden and his Period."
— Physical, 3.

INSTITUTE OF CHEMISTRY.

PRIZES.—DR. FRANKLAND has offered a Prize of £50 for the best Original Investigations involving GAS ANALYSIS; and Dr. C. MEYMOTT TIDY has also offered a Prize of £25 for the best Original Investigation on SPECIAL REACTIONS OF THE ALKALOIDS, AND THEIR SEPARATION FROM ORGANIC MIXTURES. These Prizes will be open to Associates, and to all persons (except Fellows of the Institute) who shall, before the 31st December next, have qualified for the Associateship in all respects short of passing the prescribed practical Examination, and successful competition for these Prizes will be accepted in lieu of such practical Examination. Further information may be obtained on application to the Secretary, Mr. C. E. GROVES, Somerset House Terrace, W.C.

NOTICE.

It having come to the knowledge of the BADISCHE ANILIN AND SODA FABRIK, of Stuttgart and Ludwigshafen on Rhine, that their Dyes numbered and patented as follows, viz.:—

Acid Magenta S,	numbered 3731,	and patented 8th October, 1877.
Acid Maroon S,	" 3731,	" " 8th October, 1877.
Methylene Blue		
(Marine Blues),	" 3751,	" " 9th October, 1877.
Fast Red	" 786,	" " 25th February, 1878.

are being used and dealt in by persons who have not obtained the same from the Badische Anilin and Soda Fabrik, or their duly Authorised Agents,

NOTICE IS HEREBY GIVEN that the Badische Anilin and Soda Fabrik are the Sole and only Patentees for the said Dyes, and legal proceedings will be at once instituted against all persons found manufacturing, dealing in, or using the same, or having them in their possession, unless the packages or tins bear the patent labels and trade-mark of the Badische Anilin and Soda Fabrik.

All persons supplying to the Badische Anilin and Soda Fabrik, or to Messrs. Schott, Segner, and Co., of 4, Charlotte Street, Manchester, full proof of any infringement of the said Patents of the Badische Anilin and Soda Fabrik will be suitably rewarded.

Dated this 1st day of March, 1880.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1059.

THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

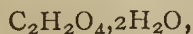
(Continued from page 106.)

I WOULD add that the theory of atomicity arose from the study of the organic compounds of carbon, to which it is easily applied. The legitimacy of this application is shown by the two following considerations:—

1. Carbon takes an equal number of equivalents of hydrogen and of oxygen, and at its limit gives the compounds CH_4 , CO_2 , CCl_4 . It is otherwise with the elements of other groups (see Table II.).

The elements of the fifth group give RH_3 and R_2H_5 , that is to say, that they are triatomic compared with hydrogen, and pentatomic compared with oxygen; the elements of the sixth group are biatomic compared with hydrogen, and hexatomic compared with oxygen, &c.

2. As far as we know carbon does not give any so-called molecular compounds, like so many of the other elements do. The combination of oxalic acid and water,—



belongs to the type of hydrates, because it corresponds to the limit compound C_2H_3 , in which all the H are replaced by (OH); $\text{C}_2(\text{OH})_6 = \text{C}_2\text{H}_2\text{O}_4, 2\text{H}_2\text{O}$; in separating by a comma the water of crystallisation, we only want to express this well-known fact that an atom of carbon in a compound retains only a slight residue of water. The incapacity of carbon to form molecular compounds is noticed particularly when we compare it with silicium, analogous in its other properties, and giving like carbon limiting compounds, SiH_4 , SiO_2 , and SiCl_4 . If in organic compounds we replace a quantity of carbon by silicium, bodies analogous in many respect to the veritable carbon compounds are produced, as the splendid researches of Friedel and Ladenburg have shown; nevertheless, there exists a great difference between the two elements, and it consists principally in the faculty possessed by Si of forming molecular compounds. Perchloride of silicium combines, like TiCl_4 , BCl_3 , PCl_5 , SCL_2 , &c., with other chlorides, and forms moderately stable bodies, whilst chloride of carbon, as far as we know, is not capable of so doing.* The aptitude of Si for molecular combinations does not arise only from the existence of SiH_2F_6 , and hydrates of silicic acid (we do not know of any hydrates of CO_2), but also from numerous properties of silica. This last point ought to be cleared up. When we compare the compounds of carbon with those of silicium, we constantly notice that the compounds of silicium boil at lower temperatures than the corresponding compounds of carbon (or sometimes, but very rarely, at the same temperature), and that they have a greater (or equal) molecular volume. There is an intimate relationship between the two phenomena:—

	Molecular Volume.	Boiling-points.
CCl_4	94	76°
SiCl_4	112	57
$\text{CAe}_4 = \text{C}_9\text{H}_{20}$	"	about 120
$\text{SiAe}_4 = \text{SiC}_8\text{H}_{20}$	"	150
CaCO_3	37	"
CaSiO_3 (wollastonite)	41	"
CHCl_3	78	60
SiHCl_3	84	34
$\text{C}(\text{OC}_2\text{H}_5)_4^\dagger$	186	158
$\text{Si}(\text{OC}_2\text{H}_5)_4$	201	160

* This question should be very carefully studied.

† Basset's ether.

We can admit that, in other cases also, analogous phenomena will spring up; however, we have contrary evidence in comparing CO_2 with SiO_2 . The boiling-point of silicic acid is at a temperature we cannot attain, and in accordance with this the volume is less than that of carbonic anhydride, liquid or solid. The volume of CO_2 is about 44, the volume of amorphous $\text{SiO}_2 = 27$. This contradiction can be explained only by the hypothesis of a case of polymerism of silica (Si_nO_n), because the phenomena of polymerisation are always accompanied by a diminution of volume and a rise in the boiling-point.*

But as the substances liable to other additions are the only one which become polymeric (CH_2 , C_2H_2 , C_3H_8 , for example), because the polymerisation is an act of combination of homogeneous molecules, the differences between silica and carbonic acid betray the property which silicium has of forming complicated compounds of this kind, compounds which are unknown in the history of carbon.

Carbon by its principal properties is clearly distinguished from the other elements; it is for this reason that a number of conclusions, completely exact when they apply to carbon compounds, are not true when we wish to apply them to other elements. We have a proof of this in the ideas on constant atomicity, which ideas, confirmed by the study of organic compounds, do not apply to the compounds of other elements. If, from the doctrine relative to the chemical structure of compounds founded on the atomicity of elements, we exclude all that is in contradiction to the mechanical theory of the structure of bodies, and to the ideas which have only been confirmed specially by carbon, there remains that which has already supplied the theories of substitution, and of the limits of chemical combination. The hypothesis of atomicity appears to me to be unsteady, perhaps because it has led to no general law, and is not built on solid foundations. If to what remains of the atomic theory we add what is given by the periodic law, we arrive at the following general idea of the forms of chemical compounds.

All the ideas on the structure of chemical compounds rest on three principal propositions. I will try and make them clear.

1. *The Principle of Substitution.*—When a molecule divides into two parts, these parts are equal (they can replace one another). This principle is comparable to the principle of mechanical conservation, and the equality of forces, in virtue of which, action and reaction are equal. It is for this reason that H_2 and O, HO and H, CH_3 and H, CH_2 and H_2 , C and H_4 , Cl and H, K and Cl, K and H, NH_2 and H, NH_4 and Cl or H or K, &c., are equivalent. The phenomena of substitution, homology, &c., are thus more generalised.

2. *The Principle of Units.*—When a molecule decomposes, one at least of the molecules of decomposition can combine with a quantity of elements equivalent to the second molecule formed simultaneously. C_2H_4 formed from $\text{C}_2\text{H}_6\text{O}$ by eliminating one molecule of H_2O , can combine with Cl_2 (equivalent to H_2O), with HCl, &c. This principle comprises the phenomena of addition, the production of limited forms, as well as of body far from the limit, &c.

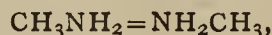
3. *The Periodic Principle.*—The highest forms of combination of an element with hydrogen and with oxygen, and consequently with equivalent elements, are determined by the atomic weight of this element, of which they are a periodic function. This principle keeps down the number of possible forms, but it still requires elaboration.

I will now explain the most important consequences of these three principles.

All elements can combine with hydrogen and produce one of the four following forms: RH , RH_2 , RH_3 , RH_4 . In each form one hydrogen can be replaced by an equivalent residue of another form (up to a certain limit deter-

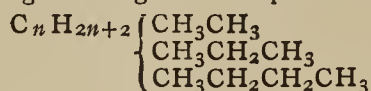
* In adopting polymerisation, we can also explain other proper ties of silica. See "Principles of Chemistry" (in Russian), 1st edition, chap. xvii., vol. ii.

mined by the nature of the element). For example, from CH_4 we can obtain CH_3NH_2 , N acting here as an element which gives NH_3 . Another substitution is possible in the body formed, viz., CH_2ClNH_2 . The order in which substitution takes place has not any influence on the properties of the bodies; it depends entirely on the form under which the substitution is effected. Thus—

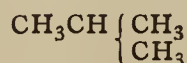


but CH_2ClNH_2 is simply isomeric with CH_3NHCl . In this manner homology and isomerism are explained. When in a compound of an element with hydrogen one hydrogen is replaced by equivalents of hydrogen compounds of the same element, homologues are formed.

The following homologues correspond to CH_4 :—

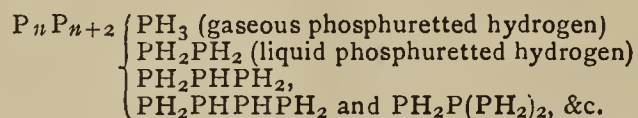


or—



&c.

The following homologues correspond to phosphuretted hydrogen :—



To sulphuretted hydrogen, S_nH_2 corresponds; SH_2 ; SHSH ; SHSSH .

The number expressed by n can have different values, according to the nature of the element. For C, as far as we know, this number is unlimited; for N it is 1; for P it is not greater than 4; for S it equals 6. The last case seems to show a dependence between the value of n and the number of atoms forming a molecule, because the molecule of sulphur is S_6 ; we can thus foresee that the molecule of ozone O_3 will give O_3H_2 , as O_2 gives O_2H_2 ; but O_3H_2 will be still more unstable than H_2O_2 , because O_3 is more unstable than O_2 .

Further, the limited compounds of which we have spoken can form, with elimination of molecules of hydrogen, hydrogen compounds far from the limits. It is thus that P_4H_6 is changed in P_4H_2 (solid phosphide of hydrogen) but losing 2H_2 . It is thus that by means of $\text{C}_n\text{H}_{2n+2}$, we obtain the series C_nH_{2n} , $\text{C}_n\text{H}_{2n-2}$, &c.

Up to this point our manner of looking only differs in appearance with that adopted by the partisans of the atomicity of elements; they are really similar up to here. But we have only spoken of hydrogen and its compounds. Further on we shall meet with essential differences; but we must notice that there are only some few elements capable of giving hydrogenated compounds, and above all giving homologous bodies. Carbon only, as far as we know, gives the bodies in any great number.

(To be continued.)

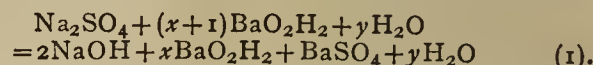
The Action of Light and the Function of Chlorophyll in Plants.—M. Pringsheim.—The author draws the following conclusions :—The possibility of the destruction of chlorophyll by light in the living plant is fully shown. It is proved that plants take up oxygen even in the full sunshine. The colouring-matter of chlorophyll by means of its strong power of absorbing light reduces the quantity of respiration by rendering inactive that part of the rays which possess the greatest photo-chemical activity. The substances which are consumed in the respiration of plant-cells are now distinguished. From the extinction of the blue rays in the chlorophyll it becomes manifest why the rays of mean refrangibility are most efficacious in the evolution of oxygen by plants, and why the assimilation-curve in plants coincides with the brightness-curve of the human eye.—*Berlin Monatsber.*, 1879, 532.

A NEW METHOD FOR THE ALKALIMETRIC DETERMINATION OF SULPHATES, AND ITS APPLICATION TO THE COMMERCIAL ASSAY OF SALTCAKE.

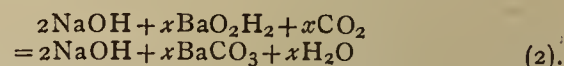
By J. GROSSMANN, Ph.D.

FROM the title of this paper it will be seen that for the present I introduce my method only as a method for the commercial assay of saltcake or like products. Though no doubt it will prove of very general applicability (with the exception of cases where sulphates are in neutral solution, besides oxalates, tartrates, phosphates, &c.), I shall confine myself here to its special application, as so far I have investigated the accuracy of this method only with regard to the conditions under which I make the assay of saltcake. I am engaged in studying the results which this method yields under different conditions, and on a future occasion I shall show how far it may be applicable for the determination of small quantities of alkali sulphates, of mixtures of potash and soda (by indirect analysis), of sulphates of the metals, &c. The method is based on the following well-known reactions :—

If we add an excess of a cold saturated solution of barium hydrate to the solution of sodium sulphate, barium sulphate is precipitated and sodium hydrate and barium hydrate are left in solution.



If we now separate the precipitate of barium sulphate from the solution of barium hydrate and sodium hydrate and pass carbonic acid through the clear filtrate, barium carbonate is precipitated and sodium hydrate left in solution.



And the quantity of sodium hydrate formed in these reactions is equivalent to the quantity of sodium sulphate originally in solution, and can be estimated by means of standard sulphuric acid.

It is evident that the operation cannot be performed from beginning to end in the same vessel. For we are obliged to use an excess of carbonic acid, and thus some barium bicarbonate and sodium carbonate and bicarbonate are formed; and it is well known that the latter to a certain extent decompose barium sulphate, forming again sodium sulphate. It will now be easily understood why I use the *modus operandi* which I shall now describe.

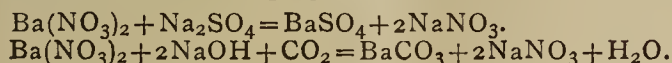
The neutral solution of sodium sulphate (3.55 grms.) is put into a 500 c.c. flask, a measured quantity of barium hydrate in solution (250 c.c. of a cold saturated solution) is added in excess, the flask filled up with water, and shaken up. Of the filtered clear liquid 250 c.c. are put in an ordinary flask, carbonic acid passed through for about ten minutes, and then the contents of the flask boiled so as to decompose any barium bicarbonate which may be in solution. After cooling, the contents of the flask are again transferred to the 500 c.c. flask, the latter filled up with water to the mark, shaken up, and filtered. 250 c.c. of the filtrate—i.e., one-fourth of the original quantity used—are then titrated with one-fourth normal sulphuric acid. The number of c.c. of one-fourth normal acid used multiplied by two will give the percentage of sodium sulphate.

There are, however, four sources of error in the experimental working of this method which make certain corrections necessary. They arise—

- (1.) From the impurities of the caustic baryta.
- (2.) From the inaccuracy of the graduated vessels used.
- (3.) From the precipitate formed in them.
- (4.) From certain constant losses.

The commercial caustic baryta always contains barium nitrate, and sometimes barium chloride. It is evident that on adding a solution of barium hydrate which con-

tains barium nitrate to a solution of sodium sulphate, a quantity of the latter, equivalent to the quantity of the barium nitrate present, will be converted into sodium nitrate, and thus escape our alkalimetric test. I am not in a position to say whether this error will take place in the first or second part of the reaction, and at the present state of our knowledge it would be very difficult to decide on this point. It is, however, quite immaterial for our purpose at which stage in our experiments this loss occurs, the result being finally influenced in the same way, as will be seen by the following equations:—



It is therefore necessary to measure approximately the quantity of baryta solution used, so as to know the amount of barium nitrate introduced into our process. The latter can be easily ascertained by passing carbonic acid in excess through the cold saturated solution of barium hydrate, boiling, filtering, and precipitating the baryta left in solution by sulphuric acid as usual. 250 c.c. of a baryta solution which I used for the experiments given in this paper yielded 0.0280 grm. BaSO_4 , which corresponds to 0.0171 grm. Na_2SO_4 , or 0.96 c.c. of one-fourth normal acid; and it follows that for every 250 c.c. of this baryta solution I found 0.0171 grm. Na_2SO_4 too little, or that I must add 0.24 c.c. of one-fourth normal acid to the result of my final titration (of one-fourth of the original quantity). If the baryta contain caustic alkali we shall find a corresponding quantity of barium nitrate less by our test; but it is easily understood that our calculations will not be influenced as long as the barium nitrate is in excess of the caustic alkali, which is always the case in good commercial baryta.

The second source of error, as stated above, arises from the inaccuracy of the graduated vessels used, in this particular instance the 500 and 250 c.c. flasks. In these experiments we are only concerned with the relative, and not with the absolute, capacity of the vessels. All that is necessary is that the 250 c.c. flask should hold exactly half the quantity of liquid which the 500 c.c. flask holds. A. Gavalovski* has published an interesting paper on the graduating of standard vessels in cases where this cannot be done directly on the balance. I have modified his method in my experiments, and it will be seen how necessary it is to check the vessels sold as being of a certain capacity.

To do this I dissolved pure barium chloride in about 250 c.c. of water, and added measured quantities of hydrochloric and sulphuric acid. In two experiments 1.7980 grms. yielded 1.7165 grms. BaSO_4 (calculated 1.7169 grms. BaSO_4), and 1.7742 grms. barium chloride yielded 1.6935 grms. BaSO_4 (calculated, 1.6942 grms. BaSO_4), the first instance giving 99.98 per cent, and the second 99.96 per cent of the calculated quantity; and I may assume that on an average I should find 99.97 of the calculated quantity of barium chloride in my experiments.

I then dissolved a weighed quantity of barium chloride in the 500 c.c. flask, filled up to the mark with water, then filled the 250 c.c. flask from it, and added to this quantity the same amount of hydrochloric and sulphuric acid as in the previous experiment, and weighed the barium sulphate. In two experiments 3.4956 grms. barium chloride gave 1.6630 grms. BaSO_4 , and 3.5058 grms. barium chloride gave 1.6664 grms. BaSO_4 . Calculated on the basis that I should find only 99.97 per cent of the theoretical quantity of BaSO_4 , 3.4956 grms. would yield 3.3370 grms. BaSO_4 , and 3.5058 grms. would yield 3.3467 grms. BaSO_4 . We find thus the capacity of the quarter-litre flask not 0.5 of the half-litre flask, but 0.4979 and 0.4983, or the mean 0.4981, i.e., 0.95 c.c. too little. It is now very easy to rectify the 250 c.c. flask. We fill it with water up to the mark, and pour into it $13.6 \times 0.95 = 12.92$ grms. mercury, and mark with the diamond the line at which the water stands then. In this way we avoid the troublesome cor-

rection which we should otherwise be compelled to calculate each time. To get correct results it is necessary to work as I have shown just now; i.e., the different determinations of barium sulphate must be made under the same conditions, so that the results be independent of the errors of the method. Fresenius* has lately pointed out again that the determination of barium sulphate by precipitation is not always as accurate as is generally assumed.

The third error arises from the precipitates of barium sulphate and carbonate taking up some space in the 500 c.c. flask, the final results thus being found too high. If we assume that a cold saturated solution of baryta contains about 23 grms. BaO per litre, we shall be near enough for all practical purposes if in our experiments, working with about 3.55 grms. of Na_2SO_4 and 250 c.c. of baryta solution, we subtract 0.4 per cent from our final results for this error.

Three experiments made with 3.55 grms. of pure ignited sodium sulphate gave the following results:—

I.	
Used one-fourth normal acid ..	49.37 c.c.
Add for $\text{Ba}(\text{NO}_3)_2$	0.24 "
<hr/>	
	49.61 "
= 99.22 per cent Na_2SO_4 .	

II.	
Used one-fourth normal acid ..	49.21 c.c.
Add for $\text{Ba}(\text{NO}_3)_2$	0.24 "
<hr/>	
	49.45 "
= 98.90 per cent Na_2SO_4 .	

III.	
Used one-fourth normal acid ..	49.37 c.c.
Add for $\text{Ba}(\text{NO}_3)_2$	0.24 "
<hr/>	
	49.61 "
= 99.22 per cent Na_2SO_4 .	

The average of these three experiments gives us 99.1 per cent, and if we subtract from this 0.4 per cent for the precipitate, we get 98.7 per cent instead of 100.

I have not been able to find where this loss of 1.3 per cent takes place, though I hope that my experiments with different quantities of sulphates will throw some light on this point. In working with 3.55 grms. of sulphate in the given dilution it appears that this loss is a constant, and by dividing all our results by 0.987 we will be able to get right results.

It now remains to show the peculiar applicability of this method to the assay of salt-cake and like substances, and this can be done in a few words. The following will be easier understood if I give first a complete analysis which I made of a sample of salt-cake.

Moisture	0.49
Insoluble	0.29
Free sulphuric hydrate	0.38
Aluminium sulphate	0.23
Ferric sulphate	0.42
Lime sulphate	1.17
Sodium chloride	2.00
Sodium sulphate (by difference) ..	95.02
<hr/>	
	100.00

In order to make a good test of salt-cake it is now necessary to estimate seven constituents, to find by difference the quantity of actual sodium sulphate, which is the only constituent wanted.

We will now consider what happens when barium hydrate is added to a solution of salt-cake. The free acid is precipitated, so are alumina and iron, and the sulphuric

* Chem. Centralblatt, 1879, p. 236.

* Zeitschrift für Analytische Chemie, 1880, p. 53.

acid combined with them and with lime. The lime is partly thrown down as such, and what is left as lime in solution is precipitated as carbonate in the second operation. Thus, whatever other sulphates be present, only the sodium sulphate is given in our test; and by one simple test we are thus able to get a result which formerly could only be attained by a tedious complete analysis.

The saltcake, of which I have given a complete analysis above, was tested by my alkalimetric method. 3.55 grms. required—

One fourth normal acid 46.93 c.c.
Add for Ba(NO₃)₂ 0.24 „

47.17

=94.34 per cent Na₂SO₄.

(94.34 - 0.40) = 93.94.

93.94 : 0.987 = 95.2 per cent Na₂SO₄.

We find thus by the alkalimetric test 95.2 per cent Na₂SO₄, whereas the analysis gives 95.02 per cent. If we consider how difficult it is to wash soda salts completely from precipitates, we are not astonished to find the result too low in the complete analysis, as in five precipitates a very minute quantity will make up 0.2 per cent.

It is hardly necessary to point out that none of the figures for the correction of the four errors enumerated above, and given in this paper, can be used by anyone else working by this method, but that they must be ascertained in every individual case. I may also mention that it is absolutely necessary to ascertain after the first operation that there is no sulphate, and after the second (before titrating) that there is no baryta in solution.

St. Helens, Lancashire, February, 1880.

ON THE

REDUCTION OF CHLORIDE OF GOLD BY HYDROGEN IN THE PRESENCE OF PLATINUM.

By Dr. D. TOMMASI.

WHEN a solution of chloride of gold is acted on by hydrogen in the presence of platinum the chloride of gold is reduced to the metallic state. To what is this reduction due? Let us examine the facts:—An aqueous solution of chloride of gold has no action on a strip of platinum, neither has the hydrogen any action on the chloride of gold; but the platinum put in the presence of hydrogen has the property of absorbing a certain quantity of this gas.

The starting-point of the reduction of the chloride of gold is in the condensation of the hydrogen on the platinum. The platinum in absorbing the hydrogen disengages heat, and it is precisely this heat which determines the reaction between the hydrogen and the chloride of gold. This reaction once commenced can continue of its own accord, and give place, like all chemical reactions, to an electric current.

The electric current observed by Dr. Phipson is not, therefore, the initial, but the final action; the disengagement of electricity does not start the reaction between the chloride of gold and the hydrogen, but it is, on the contrary, this chemical reaction which starts the electric current. What is, in Dr. Phipson's opinion, the cause of this reaction is, according to my idea, the effect of it.

By substituting for the strip of platinum another metal which has no action on the chloride of gold, and which does not absorb hydrogen, I am convinced that no reduction will take place; and I believe that if, instead of platinum, palladium were used, the reduction of the gold would take place with much greater energy, because palladium can absorb much more hydrogen than platinum absorbs.

ON VESBIUM AND NORWEGIUM.

By C. RAMMELSBURG.

SIGNOR A. SCACCHI in a session of the Neapolitan Academy of Sciences, made a communication on the examination of certain green and yellow incrustations which cover the fissures in the lava of Vesuvius of the year 1631. They consist of silicates, and contain copper, lead, and a body which he considers as new, and which he designates as Vesbium, from the old name of Vesuvius. The small quantities of this substance have allowed merely preliminary investigations. It appears to be a metallic acid of a red colour, giving colourless alkaline salts, which turn yellow on the addition of an acid. The silver salt is red or orange; the copper salt, a yellowish green. Sulphuretted hydrogen gives a brown precipitate and a blue liquid which is rendered brown by zinc.

Before the blowpipe it communicates to microcosmic salt a yellow colour in the outer flame and a green in the inner.

S. Scacchi does not believe in the presence of molybdenum or vanadium, though many considerations render the latter not improbable.

The memoir contains no numerical facts beyond the statement that the silver salt contains 48.8 per cent of silver (AgVO₃ = 52.1 Ag).

More definite are the statements made by Dahll in the *Zeitschrift d. Geol. Gesell.* (vol. xxxi., p. 480), on the supposed new metal from the red nickel pyrites of Krageröe, which he calls Norwegium (Ng). He finds it white, not very ductile; sp. gr. 9.44; melts at 350°, and dissolves in nitric acid with a blue colour, which becomes green on dilution. On the reduction of the brownish oxide in hydrogen the proportion of oxygen was found, 9.6 to 10.15. On the supposition NgO we have Ng = 150.6 or 141.6. The solutions are precipitated green by alkalies, but the precipitate dissolves in excess with a blue colour. Sulphuretted hydrogen gives a brown precipitate which does not dissolve. Zinc reduces the sulphate first to a brown liquid, and on boiling to metal. Before the blowpipe it gives yellow glasses, which turn blue on cooling, and upon charcoal with soda there is formed a yellowish green coating.—*Berichte der Deutsch. Chem. Gesell.*

ON THE NEW HYDROGEN LINES

AND THE

SUPPOSED DISSOCIATION OF CALCIUM.

By H. W. VOGEL.

ACCORDING to the views hitherto prevalent, the spectrum of hydrogen consists essentially of four lines. In my photographs of Geissler tubes filled with hydrogen I obtained along with the four chief lines a great number of others, among which several in the violet and the ultra-violet were distinguished by their intensity and definite character. The photographs of the spectra were published in the *Monatsberichte der Akad. der Wissenschaft. Berlin* (1879, p. 550).

To show that these new lines pertain to hydrogen itself and not to any admixture of foreign gases, I repeatedly photographed the spectrum of electrolytic hydrogen, in my opinion chemically pure. I obtained the same lines as heretofore, and can no longer hesitate to ascribe them to hydrogen.

Amongst these new lines one is particularly remarkable for its brightness and for its coincidence with Fraunhofer's line H', generally attributed to calcium, to which body Fraunhofer's line H'' also belongs. The existence of this conspicuous line, which is present in all my hydrogen spectra, and according to more recent measurements appears rather less refrangible than Fraunhofer's H', and

which I designate as the fifth main hydrogen line (Hd_{ϵ}),* leads me to certain conclusions not, I believe, devoid of interest.

Lockyer, founding on the fact that the spectrum of calcium becomes modified at elevated temperatures, has conjectured that this body becomes dissociated at elevated temperatures into two elements, X and Y, to one of which the first H-line belongs, and the second to the other. He has not, however, hitherto succeeded in effecting this dissociation of calcium with terrestrial sources of heat.

He believes, however, that this takes place in the high temperature of the "white" fixed stars, which are regarded as the hottest. He depends upon an observation of Huggins, who has photographed the spectra of Sirius and Vega, and has ascertained that in them only the first of the two thick H-lines of the solar spectrum is present, the second being entirely wanting or scarcely visible.

I interpret this fact in another manner, and ascribe the solitary H' line, seen in the spectra of the fixed stars, not to calcium, but regard it as the fifth hydrogen line.

I am the more justified in this view, as the hydrogen lines are developed in a conspicuous manner in the spectra of the above stars, and appear broader and more intense than the hydrogen lines of the solar spectrum.

My opinion is further supported by the details of the observations on star spectra published by Huggins in No. 2 of the *Comptes Rendus* for 1880. He gives a list of the star lines in the violet and the ultra-violet according to wave-lengths. The first two of the 12 which he mentions are the known hydrogen lines, Hd_{γ} and Hd_{δ} ; the following ones agree so manifestly with my new hydrogen lines, the wave-lengths of which I published eight months ago, that their identity can scarcely be doubted.

I have not obtained further, more refractive lines, as I worked with glass prisms, which absorb the ultra-violet light strongly. Huggins used quartz prisms and obtained six further lines. The striking coincidence of my hydrogen lines with Huggins's star lines warrants the conjecture that the remaining lines in the spectrum of the white stars belongs to hydrogen.

There requires a further investigation with apparatus of greater dispersive power in order to decide if the new hydrogen lines are contained in the solar spectrum. The fifth line will be hard to recognise, as it is covered by the very broad adjacent calcium line, H' .

Lockyer refers to the observation of Young, according to which the H' Fraunhofer line was seen 25 times, but the H'' line 50 times "injected into the chromosphere, and explains this isolated appearance of the H' line (without H'') by the dissociation of calcium. On the contrary I take this line thus seen alone to be the fifth hydrogen line Hd_{ϵ} .—*Berichte der Deutschen Chem. Gesellschaft.*

ON THE USE OF THE SPECTROSCOPE IN DISCRIMINATING ANTHRACENS.

By BENJAMIN NICKELS, F.C.S., F.I.C.

IN my last paper referring to this subject, I stated that the magnificent absorption spectra observed with yellow chrysen was due to an associated substance fusing at $280^{\circ}C.$, extremely difficult to remove for examination, and "containing nitrogen either as an element of constitution or of a still further associated body unrecognised. I am now in receipt of a communication from the Continent, and from which I learn that the substance in question has already received the attention of Graebe, who ascribes to it a fusing-point of $330^{\circ}C.$, and the following formula:—



* The author marks hydrogen as Hd to avoid confusion with Fraunhofer's H .

viewing it as a "phenylen naphthylenimid," or a carbazol containing the group phenylen and naphthylen as replacements. The compound was described by that chemist last year, but I am told no abstract of the paper has appeared in our English journals.

A very beautiful specimen of the preparation has been kindly sent to me, and which in appearance closely resembles that of my own, differing only in tint and degree of purity; it is of a deeper green colour, and higher fusing-point, and, examined spectroscopically, presents the superb and coincident absorption-bands.

104, Leadenhall Street, E.C.,
March 6, 1880.

SOME NEW AZO-COLOURS.

By JAMES H. STEBBINS, jun., B.S.

WHEN I first began the study of the azo-compounds I selected as my starting-point the action of diazobenzol upon the amines, amides, and phenols, reserving the privilege of experimenting with the higher diazo-compounds at some future period.

The following compounds are the results of my experiments:—

Azobenzol-trinitro-oxybenzol, $C_6H_5N=NC_6H(NO_2)_3OH$.

This compound was obtained by treating an alcoholic solution of picric acid with an aqueous solution of one molecule of diazobenzol nitrate.

After standing a short time the liquid became filled with long brown needles. These had then to be filtered rapidly, as only a short contact with the mother-liquor was sufficient to decompose them. They were then washed several times with cold alcohol, and dried in the air-pump over sulphuric acid.

When dry they appear as long, brown, prismatic needles with a strong metallic lustre. They are very explosive, a temperature of about $70^{\circ}C.$ being sufficient to explode them. The melting-point could not be obtained owing to this circumstance.

They are insoluble in cold, but slightly soluble in boiling water, under partial decomposition. On the other hand, they are readily dissolved in warm alcohol, with a pretty yellow colour.

The analysis of the above compound leads to the following result:—

	Theory.		Found.
C_{12}	144	43'21	43'33
H_7	7	2'10	2'59
N_5	70	21'02	20'93
O_7	112	33'67	—
	333	100'00	—

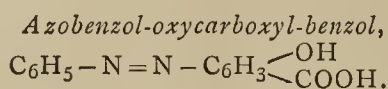
These figures prove the correctness of the assumed formula, viz., $C_6H_5-N=N-C_6H(NO_2)_3OH$.

Azobenzol-pyrogallol, $C_6H_5-N=N-C_6H_2(OH)_3$.

This was obtained by treating an alkaline solution of pyrogallol with an aqueous solution of 1 mol. of diazobenzol nitrate. Immediately a brick-red precipitate is formed, which increases on standing. It was then collected on a filter, washed with a little alcohol, and dried. It dissolves in glacial acetic acid and nitrobenzol, from which it crystallises in dark red-brown needles. An alcoholic solution of these dyes silk and wool of a pretty old-gold colour without mordants. The analyses gave the following results:—

	Theory.		Found.	
	Anal. 1.		Anal. 2.	
C_{12}	144	62'60	62'36	62'28
H_{10}	10	4'34	5'23	4'64
N_2	28	20'86	—	—
O_3	48	12'20	—	—
	230	100'00	—	—

These figures evidently lead to the following formula:—
 $C_6H_5-N=N-C_6H_2(OH)_3$.



This pretty orange dye was obtained by the action of diazobenzol nitrate on an alkaline solution of salicylic acid. It crystallises in orange-red needles, which are insoluble in water, but easily soluble in alcohol or ether. If this compound be treated with strong sulphuric acid it dissolves, forming a sulpho-acid which, to me, has all the appearances of the compound obtained by P. Griess from diazosulphanilic and salicylic acids.

Azosulphoxylbenzol-phloroglucin.

This was produced by the action of diazosulphanilic acid on an alkaline solution of phloroglucin. A heavy orange precipitate is thus formed, which is collected on a filter, dissolved in water, and precipitated as a soda salt by means of common salt. The compound thus obtained has the appearance of an orange crystalline powder, and is easily soluble in water. The free acid may be obtained by treating a concentrated aqueous solution of the soda salt with an excess of strong hydrochloric acid. It crystallises in thin orange tablets with a beetle-green lustre.

Animal fibre is dyed a fine orange with these.

Azobenzol-diamido-toluol Nitrate.

This pretty orange dye-stuff is obtained by treating an aqueous solution of diazobenzol nitrate with a solution of alpha-toluylen-diamin, melting-point 99° . Immediately the mixture assumes an orange-red colour, and, after an hours' standing, the liquid becomes filled with fine red needles. These are then thrown on a filter and allowed to drain; they are then dissolved in boiling water, and the base is set free with ammonia in the form of a yellow-coloured crystalline precipitate. These crystals are then washed and dried at 100° . The analysis leads to the following formula:— $C_6H_5-N=N-C_6H_2(NH_2)_2CH_3$.

C_{13}	68.35	per cent
H_{14}	7.61	"
N_4	24.60	"

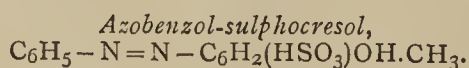
The base unites readily with acids to form salts; but the best form of salt is the chloride, as it is by far the most easily soluble and convenient to handle.

If a concentrated solution of the chloride be treated with an aqueous solution of zinc chloride a heavy, crystalline, orange-red precipitate of the zinc double salt is formed. Platinic chloride produces a precipitate of beautiful crimson needles, which constitute the platinum salt.

If this dye be dissolved in strong H_2SO_4 , and heated for a short time, and then thrown in cold water, a heavy precipitate is formed, which constitutes the sulpho-acid. This dissolves in water readily, but has a browner tinge than the previous dye. Reducing agents decolourise the solution, regenerating toluylen-diamin.

Diamido-azonaphthalen Hydrochlorate,
 $C_{10}H_7N=N-C_{10}H_5(NH_2)_2.HCl$

I obtained this brown dye by the action of diazonaphthalen hydrochlorate in aqueous solution, on a solution of naphthalen diamin dissolved in alcohol. If a little of the solution be evaporated the colouring matter will crystallise out in long brown needles. These are insoluble in water, but quite readily soluble in alcohol. Strong sulphuric acid dissolves it with a fine blue colour. Reducing agents decolourise the solution.



If an alkaline solution of cresol-sulpho-acid be treated with an aqueous solution of diazobenzol nitrate the result is a deep orange-coloured oily liquid. If this be allowed to stand for a short time, and then be treated with an ex-

cess of hydrochloric acid; the acid by this means set free will, after a short time, crystallise in long brown needles with strong metallic lustre. These are quite soluble in alcohol—less so in hot water, to which they impart a fine orange colour.

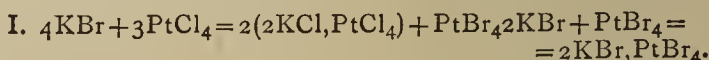
I am now working on the higher diazo-compounds, and have already obtained a series of new colours, far surpassing the previous ones in beauty and durability, and will, on some future occasion, lay before you the results of my work.—*Journal of the American Chemical Society*.

COMPOUND PLATINATES AND A NEW PLATINO-POTASSIUM SALT.

By LUCIUS PITKIN.

WHEN a solution of platinum tetrachloride is mixed with a solution of potassium bromide in some degree of concentration, a precipitate is formed which although resembling in many points the ordinary double chloride of platinum and potassium yet is of a much darker colour. The shade of colour depends largely on the rapidity of the precipitation, rapid precipitation in the cold forming a dark orange powder, while a slower formation favours the production of small reddish crystals. As will be seen further on, the amount of potassium bromide present influences the colour of crystal, as does also the presence of hydrochloric acid in greater or less amount.

Two formulæ can be given for the reaction mentioned above. In the first we may suppose the chlorine of the platinic chloride combines directly with the potassium forming a double chloride, while platinic bromide is left in solution: the latter now unites with a fresh portion of potassium and platinum.



In the second case, however, we may suppose a direct combination of the bromide with the chloride forming a salt with the composition $2KBr, PtCl_4$.



It is obvious that whether a mechanical mixture of double chloride and bromide takes places in the proportion of two (2) to one (1), or whether a chloro-bromo-platinate of potassium is formed, the percentage of each constituent remains exactly the same.

In order to determine then whether a new salt of definite proportions has been formed, or whether we are dealing with a mixture, we must resort to the use of solvents of fractional crystallisation.

The percentage of platinum in both cases is 34.35, which was found to agree most closely with an analysis of the pulverulent precipitate formed by adding platinic chloride to potassium bromide in excess. In order to ascertain if hydrochloric acid would exercise a replacing power, and allow the formation of a double chloride, three determinations were made of precipitates produced under varying circumstances.

In the first determination 4 grms. of potassium bromide were dissolved in a minimum amount of water, 2 c.c. of strong hydrochloric acid added, then 0.500 gm. of $PtCl_4$ in solution. A pulverulent precipitate was the result, 0.4175 gm. of which yielded 0.144 gm. of metallic platinum, or 34.49 per cent.

The determination of platinum was made in the usual way, i.e., a known amount of the thoroughly dried salt was decomposed by heat, with the aid of oxalic acid, and, after being thoroughly washed, was dried and weighed in the state of spongy metal.

The conditions of the second determination were the same, except 1 c.c. of concentrated hydrochloric acid was used in place of the 2 c.c. of the previous experiment

0.3565 grm. of the precipitate gave 0.123 grm. of metallic platinum, which is equivalent to 34.47 per cent.

In the third determination no hydrochloric acid was used, and the resulting precipitate contained 34.1 per cent of platinum, 0.1444 grm. being obtained from 0.4236 grm. of the substance.

We see from this that hydrochloric acid has no effect on the chemical constitution of the precipitate, the average yield of platinum being 34.32 per cent against a theoretical 34.35 per cent.

But the acid produced one effect which is interesting to note: without changing the constitution it produced a change in the colour, which was not accidental or confined simply to these determinations, for a second trial fully confirmed the first; the change consists in the tint of the precipitate growing lighter in direct proportion to the amount of acid used. It being then proven that the precipitate was constant under variations in the amount of hydrochloric acid used, it remained to determine whether it were simply a mixture, or whether a new salt—not noted in any work on chemistry which I have consulted—had been formed.

The method chosen was fractional crystallisation, as offering the best results, for the separation, if a mixture, or for the confirmation of the definite character, if a new and constant salt. The result has been such as to leave no doubt but that a salt of the composition 2KBr.PtCl_4 is formed under favourable conditions, while the investigation necessary to prove the fact has led to some interesting points in the relation of chloro-platinates to the bromo-platinates of potassium.

In order to crystallise successive crops a strong solution of potassium bromide was made, and platonic chloride in solution added, the bromide being in excess; by cautious evaporation five crops of crystals were obtained, which it was supposed would in the case of a mixture yield a successive difference in the ratio of platinum, or else prove the existence of chloro-bromo-platinate by exhibiting a constant percentage of platinum, and the percentage would be 34.35.

But neither result appeared, as the tabular view of the experiments will show, crops 1, 3, and 5 being selected for determination.

EXPERIMENT I—CROP 1.

Amount ppt. used	0.5371
Amount platinum	0.1406
Percentage platinum..	26.1800

EXP. 2—CROP 3.

Amount ppt. used	0.3680
Amount platinum	0.0960
Percentage platinum..	26.1000

EXP. 3—CROP 5.

Amount ppt. used	0.2688
Amount platinum	0.0724
Percentage platinum..	26.9000

It will be seen that the crystallisation has shown no change in the composition of the five specimens; but what it has shown is this, that boiling a haloid platinate of potassium with a strong solution of potassium bromide insures the formation of a double bromide of platinum and potassium. The theoretical amount of Pt in 2KBr.PtBr_4 = 26.3 per cent, while the average of the above gives 26.39 per cent of metallic platinum. Here also a curious colour change is apparent without change in chemical constitution, each successive crop becoming darker and darker. The first crystals were bright red. The second crystals were red. The third crystals were brownish red. The fourth crystals were dark brownish red. The fifth crystals were blackish red.

In order to put the question to a crucial test, as to whether the boiling with potassium bromide produces double bromides, some double chloride was formed in the

usual way, re-washed with alcohol, and then treated with boiling KBr solution; on re-crystallisation and washing with alcohol it proved to be a double bromide yielding 26 per cent platinum, while the double chloride yields 40.6 per cent. A new set of crystals were made now by taking 2 grms. of PtCl_4 , 0.1404 grm. of KBr, and making two solutions: upon pouring these together a copious precipitate appeared; this was filtered, washed with alcohol, and dissolved, and from its solution successive crops of crystals formed.

It will be seen that in this case little time was given for the full action of the bromide on the chloride; and if the tendency is to form first a double chloride, which is afterwards acted on by the bromide, no chance was afforded by the process.

The resulting crystals, three crops of which were separated, were all of the same tint, a rich bright yellow, and gave—

First crop, 38.3 per cent platinum.
 Second crop, 38.0 per cent platinum.
 Third crop, 39.0 per cent platinum.

The proportions used of the bromide and chloride were capable of forming the chloro-bromo-platinate if opportunity was given for full action and the platonic chloride were perfectly pure: it was of Merck's manufacture, and none remained to test as to purity.

For the final and decisive test, as to the existence of platinate of potassium in which bromine and chlorine are both united, I prepared my platonic chloride from spongy platinum, and afforded full opportunity for mutual reaction by boiling several hours.

The proceeding was as follows:—2000 grms. of platonic chloride were dissolved in 20 c.c. distilled water, and added to a solution of 1.404 grms. of potassium bromide in 50 c.c. water; the solution was then boiled for four hours, and finally evaporated slowly, and three crops of crystals recovered.

The crystals were all of the same tint, a rich dark orange, and gave uniform results in the determination of the amount of platinum contained.

CROP No. 1.

Amount used	1.0123
Pt found	0.3465
Percentage platinum..	34.2200

CROP No. 2.

Amount used	0.6311
Platinum found	0.2205
Percentage platinum..	34.7000

CROP No. 3.

Amount used	0.6479
Platinum found	0.2228
Percentage platinum..	34.3900

These percentages strike an average of 34.43 per cent, a variation of only 0.08 per cent from the theoretical composition of chloro-bromo-platinate of potassium, while their absolute identity in tint supply another strong reason for considering them as such.

To resume, then, in closing, the following facts may be taken as well established:—

First. The formation of a compound containing bromine when potassium bromide is used with platonic chloride.

Second. The conversion of chloro-platinates into double bromides by action of hot solution of potassium bromide.

Third. The variation in shade of colour, in regular gradation, of crystals separated out of boiling bromide solution, as also the colour effect of hydrochloric acid.

Fourth. The formation of a chloro-bromo-platinate, by suitable action of platonic chloride on potassium bromide, which salt is of an orange colour, and crystallises in fine octahedra.

In the commercial analysis of potassium compounds the amount is generally estimated by the weight of the dried double chloride or by the amount of metallic platinum contained in that salt. If the latter method is pursued no objection can be urged, as one (1) equivalent of platinum corresponds to two (2) of potassium, whether in the form of double chloride, bromide, iodide, or chloro-bromo-platinate. But it is plain that no such relation exists between the amount of potassium and the salts mentioned.

If the weight of the precipitate is also known, it becomes an easy calculation to estimate the approximate amount of chloride and bromide present together, from the ratio between the dried precipitate and the platinum obtained from it.

Fresenius, in his "Quantitative Analysis," recommends treating salts of potassium other than chlorides with hydrochloric acid, and evaporating to dryness.

Rose proposes the use of chlorine water in the same way: the latter method decomposes the bromide better than the former, but is rendered unnecessary if a determination of the metallic platinum is made.—*The School of Mines Quarterly, Columbia College, New York.*

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 4, 1880.

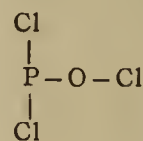
Mr. WARREN DE LA RUE, President, in the Chair.

THE list of Officers as announced at the last meeting was read from the Chair. The following certificates were read for the first time:—A. E. Black, C. H. Gimmingham, W. Regeater, W. Robinson, H. C. Stephens.

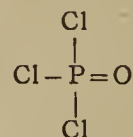
THE PRESIDENT then called on Prof. THORPE to deliver his lecture, "*On the Relation between the Molecular Weights of Substances and their Specific Gravities when in the Liquid State.*"

The Lecturer said that he proposed to give the substance of the results of some work with which he had been engaged for the last four to five years. It consisted of a critical and experimental examination of Kopp's laws relating to specific volume. Many attempts had been made to trace a relation between the specific gravity and molecular weight of a liquid, but no definite conclusions were arrived at until Kopp published his well known memoirs, the principal reason of failure being that previous observers did not compare the liquids under comparable conditions. Kopp was the first observer who—by dividing the specific gravities of liquids, taken at the temperatures at which their vapour tensions are equal to the standard atmospheric pressure, *i.e.*, at their ordinary boiling-points, into their molecular weights—obtained certain comparable values which are known as specific volumes. If the specific gravities are referred to water at 4° C., these values represent the number of c.c. occupied by the relative molecular weights of the liquids expressed in grammes at their respective boiling-points under normal pressure. Thus, the specific volume of water is 18.8, *i.e.*, 18 grms. water at 100° occupy 18.8 c.c. The numbers so obtained were first shown by Kopp to exhibit certain definite relations which may be briefly indicated as follows:—(1) In many instances differences in specific volume are proportional to differences in corresponding chemical formulæ. Thus a difference of CH₂ in a homologous series corresponds to a difference of about 22 in the specific volume. (2) Isomeric and metameric liquids have, as a rule, the same specific volume: exceptions occur in certain sulphur and oxygen compounds. (3) The substitution of an atom of carbon for two of hydrogen makes no alteration in the specific volume of members of certain groups of organic

liquids. It would seem to follow from Kopp's observations that the volume of a liquid formed by the union of two other liquids is equal to the sum of the specific volumes of its components. Another conclusion which Kopp states with caution is that members of the same family of elements possess identical specific volumes: thus the common value for Si, Ti, and Sn seems to be about 35; for P, As, and Sb about 27. On the basis of these conclusions Kopp was able to calculate certain fundamental values which are, as a rule, constant for each element. Thus carbon has always the value 11, hydrogen being 5.5. Certain exceptions, however, occur with oxygen and sulphur. Thus the specific volume of water being 18.8, and of hydrogen 11, we get as specific volume of O 7.8. In aldehyd, C₂H₄O, we have (C)₂₂ + (H)₂₂ = 44; its specific volume is 56.2: thus the value for oxygen is 56.2 - 44 = 12.2, and so on for other substances. It appears that when oxygen is united to an element by both its affinities its specific volume is 12.2; when attached by only one it is 7.8. Sulphur has similarly two values, 28.6 and 22.6. Thus the constitution of such bodies as phosphoryl trichloride may be elucidated. If P is trivalent the constitution of this substance must be—



If pentavalent—



From determinations of the specific volume of POCl₃ it seems that the phosphorus is trivalent. The specific volume of nitrogen also seems to be variable; from Kopp's researches it has a value in the amines of 2.3; in cyanogen and certain nitro compounds of 17; and from recent researches of Ramsay its value seems to vary in different amines, and, moreover, has a distinct value in the pyridin series. Buff suggested, as the atomic value of an element is variable, its specific volume might be a function of that value: the evidence adduced is not, however, conclusive. There is no known reason why O, S, and N should alone possess variable specific volumes. Recently much interest has accrued to the question from the relations which have been shown to exist between the atomic weight and the chemical and physical characters of an element: probably the specific volume is a periodic function of the atomic weight. On reviewing the question, the following problems seemed to the author well worthy of solution:—

(1) Has an element in combination an invariable specific volume? or may not the volume be modified by the number of the atoms of that element in the molecule? Is it quite independent of the general complexity of the molecule, or may not the specific volume of the molecule be a function of its weight? (2) Do the various members of a given family of elements possess identical specific volumes, or may not the volume be a function of the atomic weight? (3) Would a re-examination of the cases of the variable atomic values show that the specific volume of an element is a function of that value, as Buff supposes? (4) The hypotheses of Mendelejeff and Meyer indicate the need for additional and more exact determinations of the values for the specific volumes of the elements. (5) To multiply examples of the aid afforded by a knowledge of the specific volume of a compound in elucidating its constitution. The author then drew up a scheme of work involving the determination of the specific volumes of fifty-two liquids, inorganic and organic, those liquids being preferred which boiled below 200°, and which could, without great difficulty, be obtained pure and in large quantity. The results of the observations determine the specific volumes of the following 17 elements:—H, F, Cl, Br, I, O, S, Cr, N, P, V, As, Sb, C, Si, Ti, Sn.

The author then gave a short account of the methods used to prepare some of the liquids and the care taken to ensure and ascertain their purity. Great reliance was placed on vapour-density determinations; a modified form of Hofmann's apparatus was employed. The vapour required to heat the liquid was introduced from below, the vapour-density tube having no etched scale, to prevent the risk of cracking, only one mark being made on the tube, readings being taken by a separate movable brass scale divided into millimetres. The whole mass of mercury within the tube is uniformly heated to a known temperature: 300 c.c. of mercury and 150 c.c. of liquid (for the boiler) are required to work the apparatus. To determine the specific volumes the author has adopted the principle used by Kopp—(1) To determine the specific gravity of the liquid at some convenient temperature; (2) to ascertain its boiling-point with the utmost exactitude; (3) to determine with great care its rate of expansion, say between 0° and the boiling-point. In this way, at all events, a number of accurate and important physical data would be put on record, even if the main object of the investigation were not attained. The author then described the apparatus and methods used in these three determinations. Thermometers—two sets were used, one of three by Casella, from -9° to 160°, graduated into tenths, each tenth being about 1 m.m. The second set was by Geissler; it also consisted of three, with a range from -14° to 170°, graduated into tenths, each degree being about 6 m.m. These were carefully compared with Kew standards, and calibrated. Careful determinations of the fixed points of the thermometers were made, and their variations plotted in curves. The Dilatometers were constructed of flint glass; the stem of each instrument was about 50 c.m. long, graduated into millimetres; the bulbs contained 2 to 4 c.c. Each bulb was heated and cooled several hundred times before being used: no important change of capacity was observed during the determinations. The dilatometers were calibrated by the introduction or removal of known weights of mercury. Air was removed by exhaustion with a Sprengel pump and subsequent introduction of pure warm mercury by a capillary tube which reached to the bulb. Nine dilatometers were used. As the thermometers used were long, great care was taken in obtaining a correct expression for the correction due to the portion of the stem not surrounded by vapour. In the formula $\delta(t-t')n$ the author has taken for δ (apparent expansion of glass for 1°) a mean value of 0.000143, and gives a table of corrections calculated on this value. The specific gravity bottles held 4 to 20 c.c., and were fitted with ground glass stoppers. All weighings were made by the method of vibrations, and are reduced to a vacuum. The author then described the arrangement adopted for filling the dilatometers with the liquids, originally devised specially for liquids altered by exposure to air, but found so convenient that it was adopted in all cases. The method used in determining the expansion was then given. Two forms of apparatus were employed, one for temperatures below 65°, filled with water, heated by steam, the other filled with oil, heated by a lamp; the capacity of each bath was over 20 litres. Up to 25° the temperature could be obtained absolutely constant, but above, two series of readings were taken, one with the temperature rising very slowly, the other with the temperature falling. In reducing the observations the author largely availed himself of the arithmometer of Thomas (de Colmar), without which the labours of the calculations would have been insuperable. Some experiments proved that Matthiessen's objection to the dilatometrical method—viz., that it gave uniformly low rates of expansion—was unfounded. The author arrived at the following conclusions:—That a difference of CH₂ in a homologous series does correspond to a difference of about 22 in the specific volume. That carbon has a constant specific volume of 11 and hydrogen of 5.5: there is no valid reason for accepting Buff's hypothesis that the specific volume is a function of the atomic value of an element. As far as sulphur and oxygen are concerned the author has obtained

results identical with those of Kopp. The inference of Kopp that members of the same family have the same specific volume does not appear to be well founded. Lastly, it seems that the specific volume is a periodic function of the atomic weight.

The PRESIDENT then proposed a vote of thanks to Prof. Thorpe for his lecture, and the able way in which he had explained the results of his elaborate investigations.

Dr. WRIGHT asked if any experiments had been made with chloric or perchloric acid in order to determine the quantivalence of chlorine in these bodies.

Dr. ARMSTRONG commented on the difficulties in accepting the view of the triad nature of P on POCl₃ when the reactions of the body were taken into consideration, especially as Dr. Thorpe had himself prepared a pentafluoride.

Dr. JAPP pointed out the danger of using the specific volume as an evidence of the atomicity of an element too freely. Thus, for instance, according to the evidence derived from a determination of specific volume, the two atoms of S in CS₂ had different values; one 28.6, the other 22.6.

Dr. GLADSTONE asked if Prof. Thorpe had compared the specific volume in any way with the refractive energy.

Prof. THORPE, in reply, said that he had no intention of experimenting with chloric or perchloric acid until he had finished all the observations he wished to make with his apparatus. He admitted that there were difficulties in assuming that phosphorus was a triad in POCl₃, but thought that there was evidence to support that view; also that there was some physical evidence that the two atoms of sulphur were not identical in carbon disulphide. He had not compared the specific volume with the refractive index.

The thanks of the meeting were given to Prof. Thorpe for his lecture.

The apparatus for determining the vapour-density, and the arithmometer were exhibited.

The Society then adjourned to March 18th, when Prof. Tidy will read a paper on River Water.

ROYAL INSTITUTION OF GREAT BRITAIN.

General Monthly Meeting, Monday, March 1, 1880.

EDWARD FRANKLAND, D.C.L., Ph.D., F.R.S.,
Manager, in the Chair.

Foster Fitz-Gerald Arbuthnot; Richard Claude Belt; Shelford Bidwell, M.A., LL.B.; James Crichton-Browne, M.D., LL.D., F.R.S.E.; Henry G. Bunbury; Miss Isabella Clerk; Vicat Cole, A.R.A.; Alfred Kingsford Coles; Frederick Thomas Jennings; Alfred Lloyd, B.A., F.R.G.S.; William Mansell MacCulloch, M.D.; Miss Louisa Millar; Miss Isabella Milne; Major H. C. Roberts; Isaac Seligman; Mrs. Isaac Seligman, were elected Members of the Royal Institution.

The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 6, February 9, 1880

Virulent Maladies, and the so-called Cholera of Fowls.—M. Pasteur.—A very long account of the disease in question, and of the experiments undertaken to elucidate its nature.

Spectrometric Measurement of High Temperatures.—A. Crova.—The account of the spectro-pyrometer devised by the author for this purpose, and of the method of its use, does not admit of abstraction.

Chemiker Zeitung.

No. 3, 1880.

Danger to Fish from the Liquid Refuse of Manufactures (Bleach Works).—Dr. Weigelt.—The author has made experiments on trout. Free chlorine to the extent of 0.005 grm. per litre killed in a few minutes trout of 5 to 20 grms. in weight. On longer exposure, solutions containing even 0.0002 grm. were found fatal to small fishes, *i.e.*, free chlorine appeared destructive even to the very limit of its detectability. Soda-lye is much less injurious. In solutions containing from 3 to 0.1 grm. of crystalline soda per litre, trout were able to remain from ten to forty-five minutes without injury. Sulphuric acid is far less pernicious than hydrochloric acid. Neutral salts, such as calcium and sodium chloride, had no injurious action in the proportion of 5 parts in the thousand. It is doubtful in how far these results are applicable to other species of fish.

Chemisches Central-blatt.

No. 1, 1880.

New Chemical Photometer.—J. M. Eder.—The author determines the ultra-violet rays in daylight by means of mercury oxalate, and investigates the photo-chemistry of mercuric chloride. He uses a mixture of the latter salt with ammonium oxalate.—*Wiener Anzeiger*, 1879, p. 240.

Spectra of Hydrogen, Mercury, and Nitrogen.—H. W. Vogel.—*Berlin Monatsber.*, 1879, p. 586.

Volumetric Equivalence of Oxygen and Chlorine.—A. W. Hofmann.—A full description of arrangement for a lecture experiment.—*Berlin Monatsber.*, 1879, 673.

The Action of Light and the Function of Chlorophyll in Plants.—M. Pringsheim.—The author has studied plants under the influence of intensified light. He points out that plants growing under coloured glasses or behind coloured liquids are, as compared with their normal conditions, in the dark. Chlorophyll is rapidly destroyed by intensified light of all colours, but much more rapidly in the blue than in the red ray. This change appears independent of heat, but does not take place in the absence of oxygen. The presence or absence of carbonic acid is indifferent.

No. 2, January 14, 1880.

Solubility of Mixtures of Sodium and Potassium Chlorides.—J. Schönach.—A determination of the solubility of such mixtures at all temperatures from 0° to 100°.—*Wiener Anz.*, 1879, 236.

Compounds from Animal Tar.—H. Weidel and J. Herzig.—By oxidising the bases of bone-tar, which boil between 150° and 170°, the authors obtained two well-characterised, nitrogenous, bibasic, isomeric acids, the isoquinchomeric and the lutidinic, $C_7H_5NO_4$.

No. 3, January 21, 1880.

Decomposition of Alkaline Tri-sulpho-carbonates in the Animal System.—L. Lewin.—The poisonous properties of these compounds are ascribed to the liberation of hydrogen and carbon sulphides under the action of carbonic acid.—*Virchow's Archiv.*, 76, 452.

Carbonic Oxide as an Agent for the Preservation of Meat.—H. Nietner and K. Zimmermann.—The authors have repeated the experiments of Hunt and Gamgee, but with a negative result. Carbonic oxide showed no power to prevent putrefaction.

Examination of the Earth of a Church-yard for Products of Animal Decomposition.—E. Reichardt.—The soil in question, after a period of disuse of 30 years,

still gave off animal oil when submitted to destructive distillation.—*Arch. Pharm.*, xv., 421.

Qualitative and Quantitative Determination of Albumen in Urine.—S. P. Ilimow.—The author acidifies the urine if necessary with acid phosphate of soda. It is then allowed to settle, cooled, and filtered for the removal of mucus and urates. It is then placed in a test-glass and mixed with dilute carbonic acid (1 part acid to 20 water). If no turbidity arises, even on slight warming, the absence of albumen may be assumed. If albumen is present, even in very small quantity, turbidity ensues, followed by a flocculent deposit.

Studies on the Thomas-Gilchrist Process.—Jos. v. Ehrenwerth.—From the *Oesterr. Zeitung*, xxvii., 599, 619, 629.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 18.

Chemical Cause of the Poisonous Character of Arsenic.—C. Binz and H. Schulz.—The authors consider that the poisonous action of arsenic depends on its easy conversion and re-conversion within the organism from the higher to the lower stage of oxidation, whereby the tissues are destroyed owing to the violent vibration of the atoms of oxygen. For the same reason the other members of the nitrogen group are also poisonous.

Reply to F. Seelheim's Criticism on the Chlorine Experiments.—Victor Meyer.—See *CHEMICAL NEWS*, vol. xli., p. 219.

Determination of the Densities of Vapours which Attack Porcelain at a Red Heat.—Victor Meyer and H. Züblin.—The authors describe the construction of a platinum apparatus to be used instead of porcelain.

Attempt at the Preparation of Di-methyl-phenyl-glycocoll or Phenyl-betain.—J. Zimmermann.—An equivalent of mono-chloroacetic acid was digested along with two equivalents of di-methyl-anilin, dissolved in ether. The ultimate product of the reaction is the hydrochlorate of a phenyl-betain, $C_{10}H_{14}O_2ClN$.

Halogen Derivatives of Æthan and Æthylen.—J. Denzel.—The following derivatives have been examined: Chlor-penta-brom-æthan, α -dichlor-tetra-brom-æthan, tetra-brom-æthan, penta- and hexa-brom-æthan. Several attempts at the preparation of unsymmetrical tri-brom-æthan proved unsuccessful.

Reactions of Ethyl-amin.—H. Köhler.—Not adapted for abstraction.

Product of the Action of Ferric Chloride upon Ortho-diamido-benzol.—C. Rudolph.—The product is the hydrochlorate of a new base, $C_{24}H_{18}N_6O$.

Further Contributions to a Knowledge of Acetylen-dicarboxylic Acids.—E. Bandrowski.—The author examines the copper salt of the above acid, also di-bromo-acetylen-dicarboxylic acid with its silver and lead salts.

Further Particulars on Pittacal and Eupittonic Acid.—A. W. Hofmann.—The question is raised whether eupittonic acid is formed from 2 mols. pyrogalllic acid and 1 mol. methyl-pyrogalllic ether, or if 1 mol. pyrogalllic acid and 2 mols. methyl-pyrogalllic ether have taken part in its formation. The author further examines the salts of eupittonic acid, its acid derivatives, its alcohol derivatives, and ethyl-ether, and iodine compound, and its behaviour with water.

Determination of Chromium.—Theodor Wilm.—Already given at some length.

Action of Hypochlorous Acid upon Acrylic Acid.—P. Melikoff.—A preliminary communication.

Action of Chlorine upon Naphthalin- α -sulphon-Chloride and γ -tri-chlor-naphthalin.—Oscar Widmann.—An account of the tetra-chloride of naphthalin- α -sulphon-chloride, of dichlor-naphthalin- α -sulphon-chloride, and γ -trichlor-naphthalin.

Dichlor-naphthalin- α -sulphonic Acid and its Salts.

—O. Widmann.—The formula of the acid is—
 $C_{10}H_5Cl_2SO_2OH$.

Its potassium, sodium, silver, barium, calcium, lead, and zinc salts, and its amide are here described.

Action of Hydro-Ferro- and Ferri-cyanic Acids upon Amines.—L. J. Eisenberg.—The author has obtained a hydro-ferrocyanate of aniline as a white crystalline salt, and has been similarly successful with ortho- and meta-toluydin, acetamid, brom-anilin, naphthylamin, &c.

On Tungsten Bronze.—J. Philipp and P. Schwebel.—This alloy, which is not perceptibly attacked in the wet way by alkalies nor by any acid except the fluoric, is decomposed if heated to boiling in a sealed tube along with a solution of silver acetate.

Toluylen-di-amins.—R. Nietzki.—The di-amins, when perfectly pure, may be easily recognised by their behaviour with oxidising agents. Whilst the ortho compounds deposit intensely coloured crystalline precipitates of a metallic lustre, the para and meta compounds display no such phenomena, and are characterised by the absence of the quinon reaction.

Hydroxylation by Direct Oxidation.—R. Meyer and A. Baur.—Not adapted for abstraction.

Constitution of Di-brom-ethylen.—E. Demole.—Not suitable for abstraction.

MISCELLANEOUS.

Royal Institution.—The following arrangements have just been made for the lectures after Easter:—Tuesdays—Prof. Huxley, Two Lectures on Dogs, and the Problems connected with them; Mr. Robert H. Scott, F.R.S., Four Lectures on Wind and Weather; Mr. John Fiske, Three Lectures on American Political Ideas from the standpoint of Universal History. Thursdays—Prof. Tyndall, Six Lectures on Light as a Mode of Motion; Mr. T. W. Rhys Davids, Three Lectures on the Sacred Books of the Early Buddhists. Saturdays—Mr. James Sully, Three Lectures on Art and Vision; Prof. Henry Morley, Five Lectures on the Dramatists before Shakespeare, from the Origin of the English Drama to the year of the Death of Marlowe (1593). The Friday evening meetings will be resumed on April 9th, Prof. Huxley on the "Coming of Age of the 'Origin of Species.'" Succeeding discourses will probably be given by M. Ernest Renan, Mr. W. H. Pollock, Mr. W. Spottiswoode, Mr. G. J. Romanes, Lord Reay, Mr. H. H. Statham and Mr. Francis Hueffer.

MEETINGS FOR THE WEEK.

SATURDAY, 13th.—Physical, 3. "Note on a New Source of Electricity," Prof. W. F. Barrett. "On the Surface of Certain Metals," W. Chandler Roberts.
MONDAY, 15th.—London Institution, 5.
—Medical, 8.30.
TUESDAY, 16th.—Royal Institution, 3. Prof. Schäfer, "Physiology of Muscle."
—Civil Engineers, 8.
—Zoological, 8.30.
—Society of Arts, 8. Captain C. E. Foot, "Transport and Trading Centres for Eastern Equatorial Africa."
WEDNESDAY, 17th.—Society of Arts, 8. W. Herbert Singer, "The Art of the Silversmith."
—Meteorological, 7.
THURSDAY, 18th.—Royal Institution, 3. Prof. Dewar, "Chemical Progress."
—Zoological, 4.
—London Institution, 7.
—Royal, 8.30.
—Chemical, 8. "On River Water," Prof. Tidy.
FRIDAY, 19th.—Royal Institution, 8. Prof. Tyndall, "Goethe's Farbenlehre (Theory of Colours), 9.
SATURDAY, 20th.—Royal Institution, 3. Mr. Saintsbury, "Dryden and his Period."

NOTES AND QUERIES.

Separating Starch from Maize.—Can any of your readers inform me of the best process for separating starch from maize and the makers of the necessary plant?—J. C. O.

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King Edward's School, Birmingham,
28th February, 1880.

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(Marine Blues),	" 3751,	" " 9th October, 1877.
Fast Red	" 786,	" " 25th February, 1878.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1060.

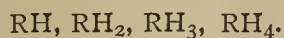
THE PERIODIC LAW OF THE CHEMICAL ELEMENTS.

By D. MENDELEEF.

(Concluded from page 114.)

OXYGEN combines with the atoms of each element R, in one or more of the following manners:—

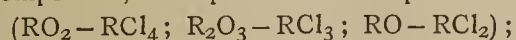
R_2O , RO , R_2O_3 , RO_2 , R_2O_5 , RO_3 , R_2O_7 , and RO_4 . We often obtain other lower forms of combination, such as R_4O , although in the cases of all the known elements higher forms have been observed. H_2 and O , judging from the constitution of water, are equal between themselves; if so, the first four lower compounds of oxygen are also equivalent to the forms of combination of hydrogen,



As the number of equivalents of hydrogen and oxygen which can be fixed by one elementary atom does not surpass eight in all the elements which give RO_4 do not form compounds with hydrogen; the elements which give R_2O_7 give also RH , and those which give RO_3 give also RH_2 ; those which give R_2O_5 give RH_3 , and those which give RO_2 give RH_4 . The elements corresponding to the highest form R_2O_3 have not as yet given any compounds with hydrogen, because such a form of combination RH_5 as it would require, does not exist. Therefore the aptitude of elements to combine with hydrogen diminishes when their aptitude for combining with oxygen increases, and *vice versa*; the highest forms or limits of these compounds are RO_4 and RH_4 , RH_4 corresponding according to the equivalent of water to the oxygen compound, RO_2 .

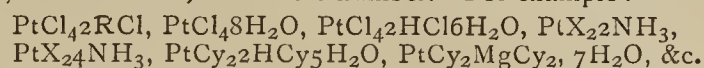
The formation of complex saline compounds with oxygen is determined by the forms of the simple oxygen compounds; for example, it must be admitted that in the hydrates O is represented by equivalent quantities of $(HO)_2$ or H_2 . Therefore, from SO_2 is formed $SO_2(OH)_2$, $SO_2H(OH)$, and SO_2H_2 . From CO_2 is derived $CO(OH)_2$, $COH(OH)$, and COH_2 . The other phenomena of substitution of this kind would take us too far. Some explain themselves, and I hope to discuss the others, later on, in a memoir developed on molecular combinations. The lower forms of combination are capable of being transformed into the highest forms, either by direct or indirect means. If the highest form that an element can give is RX_n , a given form RX_{n-m} can change into the limit form RX_n by the absorption of X_m , or of an equivalent quantity of other substances; that is easily understood without any further explanation.

The limit compounds of *chlorine*, or in general the *haloid* compounds, correspond to the compounds of oxygen



they are often met with in forms far removed from the limit, and then they often correspond to the hydrogen compounds. They have never higher forms than the oxygen compounds. For example, in the case of *Te*, $TeCl_6$ corresponding to TeO_3 does not exist, but $TeCl_4$ corresponding to TeO_2 does. In the case of *I*, there is not ICl_7 corresponding to I_2O_7 , but there is ICl_5 , corresponding to I_2O_5 . Again, taking *As*, there is no $AsCl_5$ corresponding to As_2O_5 , but $AsCl_3$, corresponding to As_2O_3 . In general, chlorine, like oxygen, often gives, besides the higher forms corresponding to the oxygen compounds, some lower forms.

The aptitude of several (but not all) elements, to form different forms of combination, is not expressed completely, either in the hydrogen compounds or in the highest forms of oxygen compounds, above all when a compound is composed of more than two elements. Entire molecules can combine one with the other and produce higher forms; both polymeric and so-called molecular compounds. As far as concerns *Si*, the form SiX_4 (corresponding to SiH_4 , to SiO_2 , to $SiCl_4$) does not show the limit to the compounds which this body can form. For *Si* does not only give SiO_2 , but also SiO_{2n} , SiO_2 , $SiF_4 \cdot 2HF$, &c. *Pt* does not only give PtO_2 , $PtCl_4$, or generally PtX_4 and PtX_2 , but also $PtX_{4n}A$; *A* here means an entire molecule, and *n*, in most cases, is a whole number. For example:—



Some of these forms of compounds are very stable, lend themselves to double decompositions, and are met with in many elements; such is the form RH_2X_6 for the elements which give RO_2 (*X* means a haloid or a haloid residue). For example:—



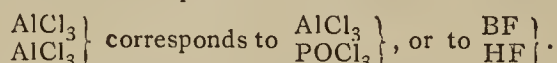
These forms of combination serve to characterise the elements and their forms (for example, the form of alum, the form of several salts, $FeSO_4 \cdot 7H_2O$, $RK_2(SO_4)_2 \cdot 6H_2O$, &c.). They therefore merit a comparative examination as exact as all the other forms with which they have no essential difference.

I do not pretend to erect, from the considerations that I have here set forth, a definite system. I know that many improvements and additions are still necessary, but I believe that the direction indicated and followed in this memoir will lead to the end desired by chemists more easily than any other will. Bold hypotheses have a peculiar attraction for us; they often cause a momentary progress; but more often they lead to inexact conclusions, and they themselves fall into disuse, above all if they are not built upon philosophical laws, for it is to the discovery of such laws that all scientific efforts should tend. I have endeavoured in the preceding developments to find support on the laws of substitution, on those of limits, as well as on the periodic law, and I believe that these laws should be taken as the basis of all generalisations on the forms of combination of the elements.

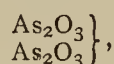
Independently of all that I have just said, I will add another observation and an example which clear my ideas on this subject in its essential parts. Two facts are necessary to actually identify an element; they are found by observation, by experience, and by comparisons. These are the atomic weight and the atomicity. The periodic law, in bringing to light the mutual dependence of these two values, at the same time affords us the means of determining one by the other; therefore, if the hypothesis of the atomicity of the elements determines the forms of chemical compounds, the periodic law does the same; but the latter goes still further, for it determines at the same time such forms of oxidation that the above-mentioned hypothesis neglected.

Boron and Aluminium, judging by their atomic weights, are analogues, and ought to give analogous compounds. Such is really the case, for they give oxides B_2O_3 and Al_2O_3 : these oxides are the only ones formed by boron and aluminium. The hydrogenated compounds BH_3 and AlH_3 possible for these elements do not exist, for the general formula for the compounds of *B* and *Al* corresponds to RX_3 . The form RX_5 is not represented in the isolated condition even in the hydrates; these last correspond to $R(OH)_3$. However, in consideration of the aptitude for more uncommon compounds, bodies composed according to the formula RX_5 are possible. Such bodies do in reality exist; they have been considered up to the present as being molecular compounds; for example, BHF_4 . We can say that ordinary octahedric borax, $Na_2B_4O_7 \cdot 5H_2O$ or $NaB_2H_5O_6$, answers by its composition to the form B_2O_3 ; that is to say, that this borax would be $B_2(OH)_5(ONa)$;

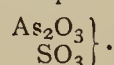
here again, then, the form BX_3 is preserved. As to the faculty which borax possesses of absorbing more water of crystallisation and being transformed into prismatic borax, $Na_2B_4O_7 \cdot 10H_2O$, it is explained not only by the property which is peculiar to boron (of forming compounds like BX_3), but also by the property possessed by Na, of forming $Na(OH)_3H_2O = NaH_7O_4$, independently of $Na(OH)$. The aptitude of the form BX_3 to change into BX_5 is again shown by the reunion of BCl_3 with other chloranhydrides. H. Gustavson prepared not long ago, in our laboratory, a compound BCl_3POCl_3 , perfectly crystallised. We know that aluminium possesses the same property, for it not only gives $AlCl_3NaCl$ and $AlCl_3POCl_3$, but also $AlF_3 \cdot 3NaF$. I see in this property a reason for admitting the existence of combinations of several molecules, either of $AlCl_3$ or of polymers. The composition—



We cannot determine from the forms BCl_3 and Al_2Cl_6 , corresponding chlorides, the different atomicities of the two elements. The partisans of atomicity compare Al_2Cl_6 with C_2Cl_6 , and from it they conclude that aluminium is a tetrad: however, we are quite as much authorised to consider Al as a pentad, for AlH_4 and AlH_2 do not exist. There is no reason to admit the existence of these hypothetical bodies: the existence of $AlCl_3$ corresponding to $AlAe_3$, BCl , and BAe_3 is much more natural. A polymeric transformation of $AlCl_3$ into Al_2Cl_6 would correspond better with the polymers S_2 and S_6 , CH_2 and C_2H_4 , &c. Alumina is apparently a polymeric modification ($Al_{2n}O_{3n}$) in the same way as $AsCl_3$ does not correspond with As_2O_3 , but with As_4O_6 . $AsCl_3$ combines with the chloranhydrides as As_2O_3 does with different anhydrides: therefore, two molecules of As_2O_3 can unite in one body—



analogous to the known compound,—



ON THE
PHENOMENON OF "FLASHING" IN
ASSAYS OF GOLD,
AND ON THE
INFLUENCE OF THE METALS OF THE
PLATINUM GROUP ON IT.

By Dr. A. D. VAN RIEMSDIJK,
Assayer-General of the Netherlands Mint, Utrecht.
Translated by W. CHANDLER ROBERTS, F.R.S.

WHEN 500 milligrms. of pure gold with 1 to 2 grms. of lead, or when 500 m.grms. of pure gold, 50 m.grms. of copper, with 4.5 to 5 grms. of lead, are cupelled at a temperature exceeding the melting-point of gold, and after the operation the liquid metal is withdrawn from the muffle, a comparatively long time will elapse,—thirty or forty seconds,—before the button sets. The cupelled gold, which is incandescent on leaving the muffle, cools below redness without changing its state. Suddenly the button emits a brilliant clear greenish light. This phenomenon is termed "flashing," (*l'éclair*) and the brilliancy of the glowing button diminishes while the metal cools, and ceases entirely when it sets.

"Flashing"† is always produced in a very distinct manner when 250 m.grms. of gold, 25 m.grms. of copper

(the copper is essential), and 625 m.grms. of silver are cupelled with 3 to 3.5 grms. of lead (this being the ordinary composition of "check" assays weighing 0.25 grm. for the standard goo), or double this when half a gramme of metal is taken for assay.

In order to obtain good results it is necessary that the following conditions be observed:—

1. The cupellation must be conducted at least at the temperature of molten silver.
2. The alloy of gold and silver must be liquid at the moment of withdrawal from the muffle.
3. The liquid globule must present a polished surface, and there must be no indication of internal or superficial movements.
4. In removing the cupel containing the button from the muffle, care must be taken to avoid the least shock or rough movement.
5. Cooling must be effected in a uniform manner.

When these conditions are duly observed, the metal, which is heated above its melting-point and luminous at the moment of removal from the muffle, cools sensibly below this temperature while remaining in the liquid state. It cools below redness, ceases to be luminous, and then suddenly "flashes." This development of intense heat again raises the metal to its melting-point (about clear cherry redness), and only then does it begin to solidify while gradually losing its latent heat of fusion.

I believe that the "flashing" may be perfectly explained by the superfusion to which gold and its alloy with copper or with silver and copper are subjected, when cupelled at a high temperature with lead in excess. There is a limit of temperature below which the fluid metal cannot cool without suddenly parting with all its latent heat of fusion, and this re-heats the cooled globule to its true melting-point, which in the case of gold and the alloy with silver, is above 1000° C. But it is an admitted fact that solid or liquid bodies so raised to a high degree of heat often emit a peculiarly intense light, and this light is known in the experiments before us as the "flashing" which precedes the solidification of cupelled gold.

The phenomenon of superfusion was observed first by Fahrenheit (1724) in the case of water, which he was able to cool to -9.4° C. without freezing. Blagden, Gay-Lussac, Despretz, Dufour, Berger, Mohr, Gernez, and other physicists have confirmed this remarkable fact,* which has been established also in the case of other bodies, such as sulphur, phosphorus, and naphthaline, as well as in acetic, sulphuric, and phenic acids.

Hitherto the metals do not appear to have been the object of special study from the point of view of superfusion.

Faraday,† in his memoir "On Regelation," Sept., 1858, states: "Acetic acid, sulphur, phosphorus, many metals, many solutions, may be cooled below the congealing temperature prior to the solidification of the first portions," while in the Treatises on Physics, of Daguin (vol. 1, p. 892, 1855) and of Jamin (vol. 1, p. 105, 1859), tin only is mentioned as being able to retain the liquid state to 225.5° C., although its true point of fusion is 228° C. The classical researches of D. Gernez‡ are of the highest interest in connection with the question before us. I will cite the following passage from M. Gernez: "It is found that certain supersaturated solutions and all the superfused liquids can be made to crystallise by the friction, more or less energetic, of two solid bodies within the fluid; but, apart from this mechanical action, which has no effect on most supersaturated solutions, the only cause that induces solidification within certain limits of temperature, is the contact of a body which must be rigorously isomorphous or identical with the substance melted or dissolved." In fact, if, after the button

† The phenomenon here called "flashing" is not identical with that which many writers on assaying have called "l'éclair," fulguration, coruscation, and brightening, but it is the same as the *l'éclair* of M. Levot in his "Etude de quelques-uns des phénomènes qui se produisent dans la coupellation des alliages d'argent et d'or" (1845).

* See also the very remarkable article of M. J. Jamin in the *Revue des deux Mondes*, for February, 1879, entitled "Le Verre du 23 janvier."

† "Experimental Researches in Chemistry and Physics," p. 379.

‡ *Comptes Rendus*, 63, p. 217 (1866); *Revue des Cours Scientifiques* vol. 4, p. 224 (1866-7).

of gold is withdrawn from the muffle in a liquid state, and when it has cooled down below its normal point of solidification, but before attaining the limit of temperature which the superfused metal cannot pass without changing state, it is touched lightly with the extremity of a fine wire of pure gold, "flashing" will be produced instantly. The metal then sets in accordance with the ordinary laws of solidification.

This explanation of the phenomenon of flashing appears to me to be more in accordance with the observed facts than the interpretation of M. Levot,* in his memoir published in 1845, which contains, however, many facts of the highest interest in connection with assaying.

I have also studied the effect of certain metals on the superfusion of gold cupelled with excess of lead. It may be well to observe that gold fused on a cupel *without the addition of lead* is not subject to superfusion; it sets gradually without "flashing" as soon as its temperature falls to the true point of solidification.

The metals which I have added to pure gold are the following:—Magnesium, aluminium, zinc, cadmium, iron, nickel, bismuth, tin, antimony, copper, silver, palladium, platinum, iridium, rhodium, ruthenium, osmium, osmium-iridium.†

The addition of some thousandths of all but the last five of these metals, that is, from magnesium to platinum inclusive, does not in any way interfere with the "flashing" of cupelled gold, or of ordinary assays on the standard 900 composed of gold, silver, and copper. On the other hand, when gold or its alloy, either with copper or with silver and copper, contains a more or less considerable quantity of iridium, rhodium, ruthenium, osmium, or osmium-iridium, the members of the platinum group which are non-malleable, refractory to heat, and resist the action of acids, the tendency of the cupelled metal to preserve its liquid state below the melting-point, and therefore to "flash" before final solidification, is entirely prevented.

The following is a brief statement of the results of a considerable number of experiments. On cupelling gold with excess of lead, together with eleven-thousandths of magnesium: a liquid button incrustated with a mixture of oxides of lead and magnesium was obtained. Notwithstanding the abnormal layer on its surface, the solidification of the metal was retarded, and it "flashed" very distinctly. With aluminium, zinc, cadmium, iron, nickel, tin, and antimony added to the pure gold in quantities not exceeding 12.5 thousandths, the flashings invariably took place, the buttons of cupelled gold presenting under these conditions the ordinary brilliant yellow surface.

According to the experiments of Dufay in 1727, confirmed later by Chaudet,‡ lead may be replaced in cupellation by bismuth. Pure gold cupelled with excess of bismuth "flashed" with great intensity. Copper promotes the superfusion of gold. When 500 m.grms. of gold and 50 m.grms. of copper are cupelled with 5 to 6 grms. of lead, the button remains liquid during 45 to 50 seconds before "flashing," while gold cupelled without the addition of copper sets in 30 to 35 seconds after the actual withdrawal of the cupel from the muffle.

Pure silver interferes with the phenomenon of flashing as soon as it is alloyed with gold in the proportion of 375 thousandths and upwards.

I explain this fact by the well-known property possessed by molten silver of absorbing oxygen, which is evolved when the metal solidifies, giving rise to the effect known as "springing." This destroys the internal quiescence of the liquid mass that is essential to its maintenance in the superfused state.

This resistance of silver to the phenomenon of "flashing" may be overcome by the addition to its alloy with gold of

a certain quantity of copper. When 625 m.grms. of silver* are cupelled with 250 m.grms. of gold and 25 m.grms. of copper and 3 grms. of lead, and the cupel is removed from the muffle at the moment when the button clears and shines brightly, the cupelled alloy remains for an abnormal length of time in the liquid state, down to a temperature well below the ordinary melting-point of the alloy in question. A sudden light terminates this state of superfusion and the button sets, presenting an even dead white surface with a slight depression at its centre. If, instead of waiting for the limiting temperature to be reached, momentary contact is made with a small portion of solid silver, the superfused state is abruptly disturbed, the metal "flashes," and then solidifies.

Palladium and platinum, the only metals of the platinum group that are malleable, ductile, and soluble in *aqua regia*, do not hinder the superfusion and flashing of cupelled gold. I have added as much as 12.5 thousandths of palladium, and even 22 thousandths of platinum to pure gold, or cupriferous gold (and its alloy, with silver), without preventing flashing, which occurred in fact with much intensity.

But the case is very different when it becomes a question of adding other metals of the same group. It is only necessary to add an infinitesimal portion of one or more of these metals before cupellation with excess of lead to produce an alloy, which, when removed from the muffle in the molten state, solidifies, almost directly, in the ordinary way without flashing, superfusion having been prevented. With cupriferous gold, or with an ordinary assay of 900 standard to which some traces of iridium, rhodium, ruthenium, osmium, or osmium-iridium† have been added, the same results are obtained; that is to say, there is normal and rapid solidification, absence of flashing, and the production (in the ordinary assay) of a solid button with a surface which is brilliant but irregular and rough.

The quantities I employed varied as follows:—

Iridium	from 0.33 to 25.5 thousandths
Rhodium	" 0.16 to 12.0 "
Ruthenium	" 0.27 to 12.75 "
Osmium	" 0.42 to 12.0 "
Osmium-iridium	" 0.50 to 4.5 "

The explanation of the absence of flashing in all these experiments may be found, I believe, in the following fact:—Gold and silver alloy perfectly with platinum and palladium. As in the molten state these alloys are homogeneous and perfectly liquid, the phenomenon of superfusion cannot, in their case, be considered extraordinary. But the other metals of the platinum group do not combine, or only with difficulty, with gold or with the gold-silver alloy, a fact which was observed long since by Berzelius in the case of osmium-iridium. It is only necessary to assume, however, that within the liquid cupelled metal the particles of iridium, ruthenium, &c., are present in the solid uncombined state, to account, on the theory of M. Gernez, for the entire absence of flashing, which, as we have seen, is closely connected with the retardation of solidification of gold (either pure or alloyed with copper) when it is cupelled with excess of lead.

There is another reason, in many cases, for the non-production of either superfusion or "flashing." When a little iridium, ruthenium, osmium, &c., is added to an ordinary assay of gold of 900 standard, and the cupellation is conducted at a high temperature, the button, after removal from the muffle, will clearly show a sparkling surface caused by the development of microscopic gaseous bubbles. These probably consist of osmic acid when osmium has been added, or of oxygen in the case

* *Ann. Chim. et de Phys.*, (3), vol. 15, p. 55.

† The well-known metallurgists, Messrs. Johnson, Matthey, and Co., of London, have furnished me with very pure specimens of metals of the platinum group.

‡ *Ann. Chim. et de Phys.*, vol. 8, p. 113 (1818).

* This refers to the assay piece of $\frac{1}{2}$ grm. of standard 900 composed of pure metals. Double this must be employed when $\frac{1}{2}$ a grm. of metal is taken for assay.

† A complex natural alloy which always contains rhodium and ruthenium in small quantities (Deville and Wolcott Gibbs) often found associated with gold and silver in Russia, America, and Australia.

of iridium or ruthenium. This agitation of the molten mass destroys the molecular quiescence necessary to preserve it in the superfused state below the normal solidification point.

I now come to the useful application of the results of these researches. They point to a very simple but effective method for detecting the presence of one or more metals of the platinum group (except platinum itself and palladium) in ingots of commercial gold, coins, or works of art. It is only necessary to cupel 0.5 grm. of the alloys with excess of lead at the temperature of molten gold; if the assays solidify directly they are withdrawn from the muffle without "flashing," it is safe to conclude that the metal undoubtedly contains iridium, ruthenium, rhodium, osmium, or osmium-iridium. This is true of ordinary assays provided they contain the requisite quantities of silver and copper. After cupellation, the alloys will remain in the superfused state, and will "flash" before solidification in the exceptional cases in which they are free from the above-named metals. In fact, commercial gold, most of the ingots furnished by refineries in which the sulphuric acid method of parting is employed, and nearly all coins, contain notable quantities of metals of the platinum group, probably osmium-iridium. The non-flashing of the assays, the bright and rough appearance of the buttons, the sparkling of the liquid mass before solidification, afford unmistakeable evidence of this fact, while the "check" assays, consisting of pure gold, silver, and copper, assayed side by side in the muffle with the experimental alloys, give buttons with a smooth, uniform, dull surface, which in all cases exhibit "flashing," and pass through the superfused state.

By the aid of *aqua regia* it is easy to separate from an alloy of gold which has not "flashed" after cupellation, the metals of the platinum group that are insoluble in the mixed acids. They will be found in the form of blackish powder mixed with chloride of silver on the filter through which the solution of gold, previously diluted with distilled water, is passed. The chloride of silver may be removed from the filter by boiling ammonia, and the gold precipitated from solution by sulphurous anhydride. Two experiments may be cited, as they entirely confirm the explanation we have given.

1. Wrap $\frac{1}{2}$ grm. of pure gold in the dry filter which contains the traces of iridium, &c., and cupel it with 1 or $1\frac{1}{2}$ grms. of lead. The non-flashing and non-superfusion of the alloy will afford evidence of the addition of the metals of the platinum group which are insoluble in *aqua regia*.
2. Wash and dry the finely-divided gold reduced from the chloride, and cupel $\frac{1}{2}$ grm. with 1 grm. of lead: the flashing will take place with intensity, for by the parting with *aqua regia* the commercial metal has been freed from the iridium and the three other insoluble metals of the platinum group.

The method of assaying gold, so exact when the alloys only contain silver and copper in addition to the gold, is not trustworthy in the presence of appreciable quantities of members of the platinum group that are insoluble in *aqua regia*. These metals, mixed with gold and silver, are not absorbed by the cupel, but are partially oxidised in the muffle in contact with oxygen and lead. MM. Deville and Debray* have proved this in the case of iridium and rhodium, and I have shown that it is also true in the case of ruthenium and osmium, the latter metal disappearing entirely in the state of gaseous osmic acid when it is cupelled alone with the necessary amount of lead. But if it is mixed with gold or silver it is impossible to separate the osmium entirely by this operation, as traces always remain in the button. By parting impure gold, after the usual cupellation, in nitric acid, and boiling, as is usual, twice, cornets are obtained which have a considerable "surcharge" caused by the iridium and other metals in-

soluble in nitric or sulphuric acid that are retained in the cornet. Such cornets cupelled with lead at a high temperature never "flash," while cornets from gold free from iridium, &c., always pass to the superfused state, and only solidify after intense "flashing." This affords, therefore, a valuable means of controlling the results of assays of commercial gold, for the absence of "flashing" in such assays proves:—1st. That the original alloy contained notable traces of osmium-iridium; 2nd, that the "fineness" deduced by calculation from the weight of the cornets is above the truth on account of the iridium, &c., which the parting by nitric acid has failed to remove. Availing myself of this method of investigation, I have been able to satisfy myself in a great number of cases—

1st. That when small quantities amounting to 5-10,000ths and over of ruthenium and osmium-iridium (which generally contains ruthenium) are present in the alloys submitted to assays, they are retained in the cornets.

2nd. That the presence of about 1-1000th of iridium in the metal to be assayed does not interfere with the accuracy of the result obtained by the ordinary method. On parting with nitric acid such an amount of iridium is removed with the silver; but if more than 1-1000th be present the buttons do not "flash," and the cornets have a considerable surcharge.

3rd. That 2-1000ths of rhodium may be added to gold without exercising any marked influence on the phenomenon of "flashing;" but if the metal contains more than this amount there will be neither superfusion nor "flashing." Rhodium is the only metal of the platinum group of which the presence of 25-10,000ths and upwards is indicated by distinctly modifying the process of cupellation. At the end of the operation the buttons "uncover," that is, the last traces of lead leave them with difficulty; they set directly they are withdrawn from the muffle, and adhere firmly to the cupel. The buttons are no longer characterised by the usual dull white surface of silver, but assume a rose tint, which becomes red-brown or even blackish when the quantity of rhodium is increased.

4th. That osmium added to gold in quantities of over 2-1000ths is found in part in the cornets. If the metal contains less than 2-1000ths the buttons "flash" in a very marked manner. If the amount of osmium exceeds 2-1000ths the assays sustain a considerable mechanical loss during cupellation, which arises from the evolution of gaseous osmic acid, globules of molten metal being scattered beyond the cupel. It is impossible in such cases to determine even approximately the true standard of the alloys under examination.

5th. The examination of *cornets* from assays of commercial gold, ingots, or coins from various sources, by the simple method I have indicated, teaches us the sad fact that many, if not most, of the buttons do not flash when the cornets are cupelled with lead. It will be evident from the preceding considerations that none of these cornets give, in the first instance, the true standard of the gold submitted to assay, on account of the "surcharge" arising from retention of osmium-iridium, a natural compound which is so often found in geological formations rich in gold and silver.

It sometimes happens that when metal from different parts of an ingot or coin of gold is assayed, some buttons flash and others do not. This may be explained by the irregular distribution of the iridium and other insoluble metals of the platinum group, in the gold and silver, with which metals they are not alloyed. The grains of osmium-iridium are, in fact, distributed through a mass of commercial gold just as fine grains of sand might be scattered through wax. They are certainly not in the state of solution or combination, which can alone produce an homogeneous alloy.

The processes adopted, both in Russia and in America,*

* *Ann. Chim. et de Phys.*, [3], v. 61 p. 81 (1861).

* Dubois, *Bull. Soc. d'Encouragement*, Jan. 1856, p. 31. Wysocki, *Dingler's Polyt. Journ.*, v. 146, p. 47. Wolcott Gibbs, *American Journ. of Science and Arts*, January, 1861.

for separating iridium and osmium-iridium from gold or from the gold-silver alloys, are based on difference in specific gravity, as well as on the fact that these metals do not combine with gold or silver.*

The molten mass is allowed to stand for some time in order that the grains of osmium-iridium may collect at the bottom of the crucible, and by carefully pouring away the supernatant metal, an alloy rich in osmium-iridium remains. This method appears to me to be inadequate for obtaining gold perfectly free from iridium, as it is often met with in the state of microscopic particles distributed through a medium, itself of high density.

It is only by chemical means that these metals can be perfectly separated from gold. This separation cannot be effected by the ordinary parting with sulphuric acid, which neither attack the gold nor the metals of the platinum group; but it is necessary to employ the process of parting with *aqua regia*, which has been used for some years with great success in the refinery of Messrs. Rössler,† at Frankfort, where gold extracted by boiling sulphuric acid from the silver coin withdrawn from circulation in Germany, is re-dissolved in *aqua regia*. The solution of auric chloride is allowed to clear, and is largely diluted with water, when the gold is precipitated in a very pure state by means of ferrous chloride. This gold is met with in commerce under the name of "Frankfurter Scheidegold," and I found on assay that its standard is 999·4. When cupelled with the necessary amount of lead it was freely maintained in the superfused state, and "flashed" with great intensity.

In conclusion, I would draw attention to this novel mode of investigating the causes of the non-malleability that is at times observable in the preparation of gold for coinage. The alloyed metal, although it may have shown no signs of fracture in passing through the ordinary rolls, is occasionally found to crack irregularly in traversing the draw-bench. I believe this to arise from the presence of streaks of iridium, osmium-iridium, &c., which destroy the continuity of the metal, rendering it locally brittle. The absence of "flashing," when such a gold alloy is cupelled with lead, will afford evidence of this being the case. I intend to thoroughly investigate this question as soon as the manufacture of gold coin is undertaken in the Netherlands Mint.

I sincerely wish that the large refineries of Europe and America would, for the future, follow the example of Messrs. Rössler, of Frankfort, in order that Mints might be supplied with ingots of gold free from osmium-iridium, for the admixture of gold with such metals of the platinum group seriously affects the malleability and homogeneity of the alloys used for coinage, and is the worst enemy of the method of assay now in use in our laboratories.

ON THE COMPOSITION AND ANALYSIS OF WELDON MUD.

By Dr. LUNGE,

Professor at the Federal Polytechnicum, Zurich.

THE mass obtained in the recovery of manganese by Weldon's process is, to the best of my knowledge, tested at all works practically in the same way, which has been indicated by Mr. Weldon himself, viz., for manganese peroxide, total manganese, and base. No doubt the testings made at different works differ slightly in details, but the principle remains always the same, and it is of course quite unnecessary for me to enter into any description of it. Some years ago I gave a description of these analytical processes in a German periodical (*Dingler's Journal*, vol. ccxv., p. 157), embodying some modifications

which I had found to be expedient, and this description is probably employed by all German works recovering the manganese according to Weldon's process as their guide in testing the mud.

Some months ago, Dr. Post, of Göttingen, published a paper on the composition of Weldon mud and some similar compounds (*Berliner Berichte*, vol. xii., p. 1454), and soon after another paper on the recovery of manganese peroxide (*ib.*, p. 1537) from which the conclusions would have to be drawn that the analytical methods universally employed in alkali works for testing the Weldon mud are utterly worthless and misleading, and that the theory of the existence of "manganites," of "base," &c., is quite wrong. It is only right to state that Dr. Post, whose name is very favourably known in Germany in connection with the *literature* of technical chemistry, does not possess any practical manufacturing experience, and thus would be more easily led to overlook the enormous bulk of contrary evidence which any practical alkali maker has at his fingers' end. But sometimes the theoretical men are in the right after all, and it would not do to leave their labours unheeded, even when they run contrary to the general opinion of practical men. In this special case, although I am on terms of personal friendship with Dr. Post, I felt it incumbent upon myself to investigate the question raised by him, not merely because his assertions concerning the inaccuracy of the ordinary testing methods indirectly attacked my publication on that head, but also because this matter necessarily occurs in my "Treatise on the Manufacture of Sulphuric Acid and Alkali," in which I am bound to notice every thing published in this line. I shall state at the outset that I disagree with Dr. Post's conclusions *in toto*, and that I believe to have completely established the accuracy of the ordinary testing methods, and of the assumption as to the existence of a "base" in the Weldon mud. But I have extended my investigations somewhat further, and revised the whole subject in question, as will appear hereafter.

Dr. Post rejects the testing for MnO_2 by means of a ferrous sulphate solution and re-titration of the same by potassium permanganate, because, on comparing it with the iodine method of Bunsen, he found a large difference between the two. The iron method in one instance indicated to him 77·19 per cent, the iodine method 69·15 per cent, which means that the former method yields results more than 10 per cent above the truth, Bunsen's method being considered the standard one. Dr. Post seeks the cause of this deviation in the presence of a large quantity of chlorides, which makes the final reaction in the permanganate method uncertain; the pink colour of the liquid, when first observed, goes away very quickly again. In order to have a standard of some sort, he accepted the colour first appearing, but quickly vanishing, as the final reaction; but he considers that consequently he used too little permanganate, and therefore found too much MnO_2 .

Now this reasoning appears to me faulty from the outset. First, Dr. Post need not have troubled himself as to the quick disappearance of the pink colour of the liquid, but he might have accepted without the least hesitation the first distinct pink, although it vanishes again in half a minute, as the *real* end of the reaction. That this is the case innumerable testings with pure iron solutions in the absence of HCl have shown to me; neither is it at all wonderful that the colour should disappear again. It is true that this fact, well known to everybody concerned, has met with a satisfactory explanation only a short time ago. The disappearance of the pink colour is simply due to this, that potassium permanganate acts upon manganese solutions, precipitating from the same MnO_2 . The small quantity of the latter is not observed at first in the yellow or brownish liquids so long as it is suspended, but it appears as a precipitate on standing for some time, often adhering to the sides of the beaker as a brown film. Recently this reaction has been utilised by several chemists

* Density of iridium 22·38
" osmium-iridium 18·9 to 21·1
" gold 19·3
" silver 10·5

† *Annalen der Chemie und Pharmacie*, vol. 180, p. 240 (1876).

for quantitative estimations of manganese, to which I shall refer later on. Consequently the *first* turning pink must be accepted as the point when the iron is just peroxidised; but this point is marked so sharply that there is never any doubt about a single drop.

It is true that the testing of iron solutions by means of permanganate has been found to be slightly inaccurate when the iron was in solution, not as sulphate or nitrate, but as chloride, and it is to this fact that Dr. Post refers. But he loses sight of the fact that the result of that error, if it were appreciable in this case, would be just the opposite one to that which he assumes. The reason why in hydrochloric acid solutions the permanganate may cause an error is this, that a little chlorine is set free, which in dilute solutions acts too slowly on the ferrous salt, so that it is partly lost, and *more* permanganate is used than ought to be the case (comp. Fresenius, "Quantitative Analysis," sixth German edition, vol. i., p. 281; or seventh English edition, vol. i., p. 219). But Dr. Post believes that he has used *less* permanganate than he ought, and the error accordingly can have nothing to do with the chlorides.

Moreover, the chlorides are in this case practically harmless. It has been noticed before (comp. Fresenius) that in the presence of a large amount of sulphates the permanganate gives correct results even if chlorides are present; and this condition is fully realised in our case, where a large excess of ferrous sulphate and of free sulphuric acid are added in testing the Weldon mud.

Still this question had to be decided experimentally, and this was all the more incumbent upon me, as the original instruction given by Weldon prescribes re-titrating by potassium bichromate, for which I had substituted the permanganate on my own responsibility for the sake of the much greater convenience of showing the final reaction within the liquid itself. As is well known, the chlorides, or even free HCl, do not affect the bichromate test at all; perhaps Mr. Weldon selected it for this reason, but more probably because it was believed some years ago that solutions of permanganate changed their standard in time, and required frequent re-standardising—an objection which has been long proved not to exist in the case of pure, crystallised permanganate. I therefore made a number of tests of the same Weldon mud, employing always the same pipette, carefully standardised liquids (each c.c. indicating 0.004 grm. oxygen), and calibrated burettes. The same iron solution was employed in all cases, made from pure crystallised ferrous sulphate, to which its own weight of pure sulphuric acid was added. In every case 5 c.c. of the mud was taken from the thoroughly shaken-up bottle, the pipette washed outside, its contents run into a beaker, the pipette washed out with water, 25 c.c. of the iron solution added, and, when the mud was dissolved, the whole diluted to 100 c.c., and at once re-titrated. The results were as follows:—

25 c.c. iron solution = 12.70 permanganate.
= 12.70 bichromate.

5 c.c. mud + 25 c.c. iron solution required:—

Of permanganate: 2.35–2.35–2.35–2.35–2.35, equal in all tests to 10.35 permanganate, equivalent to $\text{MnO}_2 = 0.2251$ grm.

Of bichromate: 2.30–2.30–2.25–2.25, equal on the average to 10.42 bichromate for the $\text{MnO}_2 = 0.2266$ grm.

The difference is extremely slight; whatever it be, it is as might be expected from the above reasoning, not as assumed by Dr. Post.

Another series of tests with a different mud yielded:—Iron solution 25 c.c. = 12.3 permanganate or bichromate (five tests).

4 c.c. mud (from a 2 c.c. pipette) + 25 c.c. iron solution = 5.45–5.45–5.45 permanganate, leaving 6.85 = 0.1490 grm. MnO_2 .

4 c.c. mud + 25 c.c. iron solution = 5.40–5.40–5.40 bichromate, leaving 6.90 c.c. = 0.1501 grm. MnO_2 .

A similar difference to the above; which of the two re-

sults was right could only be decided by a third mode of testing. For this I selected Bunsen's iodometrical test, both on account of its general reputation of accuracy, and because Dr. Post had taken it as his own standard of comparison. He had, as appears from his description, applied it to filtered and washed mud, which must have been very awkward to weigh (as it is hardly possible to expel the water without expelling some oxygen), and awkward to get into the boiling flask. I, for my part, simply emptied my 2 c.c. pipette into the flask, washed it out into the same, added some HCl, and distilled into a solution of KI, with the usual precaution. From a number of previous experiments I had found that by far the most convenient apparatus, combining perfect accuracy with the impossibility of any mishap at the end of the distillation, is that figured in Thorpe's "Quantitative Analysis," p. 165. As the delivery tube does not dip into the absorbing liquid, there is no danger of any of the latter re-entering the boiling flask at the end. It is, however, necessary to add a little ammonium carbonate to saturate the free acid distilling over; at any rate, the re-titration with sodium arsenite (which I generally prefer to hyposulphite) is only accurate under these circumstances. The ammonium carbonate may be added directly after the distillation. Five tests required for 2 c.c. mud: 17.2–17.0–17.15–17.10–17.10, on the average 17.10, or for 4 c.c. 35.20 decinormal arsenite = 0.1488 grm. MnO_2 .

This result agrees absolutely with that of the iron and permanganate test (= 0.1490) and establishes the perfect accuracy of the latter. I cannot explain the totally discordant results obtained by Dr. Post, but there *must* be some mistake about that; for one thing, he adduces only *one* comparative test, although he may have made more than he has published.

Ultimately I made a few tests with the direct iodometrical method, viz., without distillation. The manganese mud, mixed with some potassium iodide, and then with hydrochloric acid, instantly dissolves *in the cold* to a dark brown liquid, which should be neutralised with ammonium carbonate and re-titrated with sodium arsenite. The operation was performed by mixing the mud and KI in a closed flask, and introducing the HCl by a dropping funnel; but this precaution seems hardly necessary, as in the presence of KI no chlorine whatever is evolved, the whole reaction taking place in the cold, whilst without the KI the liquid must be boiled for some time before the chlorine is evolved, dilute as the liquid is in itself and by the washings of the pipette. But an accurate result can be only expected from the direct method, if no ferric oxide be present, since Fe_2Cl_6 equally sets free iodine from KI. This was confirmed by my results. I found in three tests the iodine from 2 c.c. mud = 17.4–17.45–17.30 c.c. sodium arsenite. The iron contained in the mud (estimated gravimetrically) was equal to 0.0019 grm. $\text{Fe}_2\text{O}_3 = 0.24$ arsenite solution, deducting which the results of the direct test agree with those of the distillation test.

I think I am entitled to say that the iron method, whether the re-titration is made by permanganate or by bichromate, indicates the percentage of MnO_2 in Weldon mud with perfect correctness. The above experiments, moreover, show that the taking of samples by means of a pipette, if done with proper care, is also an accurate operation.

(To be continued.)

Lectures to Schoolmistresses on Apparatus for Object Lessons.—It will doubtless interest many of our readers to learn that Dr. J. H. Gladstone, F.R.S., is giving a course of lectures to the mistresses of Board Schools and their assistants on the apparatus selected for illustrating object lessons. A knowledge of the elementary principles of physics and chemistry will thus in time be spread through the mass of the people, and though necessarily very rudimentary in its extent it must exert a wholesome influence on popular habits of thought.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Ordinary Meeting, March 14, 1880.

Dr. HUGGINS in the Chair.

NEW Members:—Prof. Minchin, Mr. Hulme, Mr. A. Stroh, Prof. D. E. Hughes, Lieut. Wingfield, Mr. J. Macfarlane Gray.

Mr. W. CHANDLER ROBERTS drew attention to an explanation which had recently been suggested by Dr. Van Riemsdijk, of Utrecht, to account for the "flashing" which attends the solidification of cupelled buttons of gold and silver. He showed experimentally that at the point of solidification the metals emit a flash of greenish light, which Dr. Riemsdijk thinks is probably due to the globules being really in what is known as the superfused or surfused state; that is, they fall some degrees below their point of solidification without setting, and the change from the liquid state is accompanied by the liberation of the latent heat of fusion, which again heats the globule and renders it incandescent. In an attempt to obtain indications as to the state of certain fused metals by the aid of the induction balance, Mr. Roberts was able to show that the resistance of silver in the molten state is far greater than when the metal is solid; and, on the other hand, he had confirmed De la Rue's statement that the resistance of molten bismuth is less than that of the solid metal; and he also obtained evidence that bismuth in cooling may be made to pass through a superfused state similar to that which occurs in the buttons of gold. (See page 125).

Mr. LOCKYER thought the greenish tint of the light might be due to a solid film on the globule.

The SECRETARY then read a paper by Prof. W. F. BARRETT, announcing that he had found a current of electricity to be generated by the rotation of the prepared chalk cylinder in the receiver of the Edison telephone. When the platinum stylus which rubs on the cylinder is connected through a galvanometer to the brass axle on which the cylinder is mounted a current is observed, whose E.M.F. is over $\frac{1}{2}$ volt. This current falls off as the rotation continues, owing, Prof. Barrett surmises, to the electrification of the surface of the chalk. Prof. Barrett attributes the current to friction solely, and seeks to account for the receiving action of Edison's telephone by the frictional current being modified by the transmitted currents, and not by the electrolytic action to which it is usually ascribed. These experiments originated with a suggestion of Prof. Sylvanus Thompson that the Edison receiver might act as a transmitter. Prof. Barrett had at length succeeded in making it act in this capacity by means of the frictional current.

Mr. SHELFORD BIDWELL exhibited some experiments bearing on Prof. Barrett's observations, which tended to show that the source of the current in the Edison receiver was due to the fact that a voltaic element is formed by the platinum rubbing point on the brass axle and the prepared chalk. This chalk is usually impregnated with phosphate of soda, or, as in the author's experiments, with caustic potash and acetate of mercury. The cylinder seems to be dry, but is probably moist; wetting it greatly increases the current. There is a very feeble current when no motion of the cylinder takes place, but rotation of the cylinder greatly increases it. Platinum is electro-negative to brass, and hence the positive current flows from the platinum to the brass through the galvanometer. This was demonstrated by substituting zinc for platinum, when the current was reversed, and flowed from the brass to the zinc, owing to the fact that brass is electro-negative to zinc. Mr. Bidwell showed, by means of a simple pile of copper and tin-foil separated by a moist cloth or paper, that the motion of the tin across the paper increased the current of the cell. In the case of a cell made of two tin plates

separated by moist paper, a current was set up by moving one plate over the other. The plate which moved relatively to the paper was always electro-negative to the other. Mr. Bidwell also showed by a simple experiment that the action of Edison's receiver was electrolytic. He caused the mere passage of a current to lessen the friction of a metal strap on a drum covered with moist paper, and thereby release the drum by the evolution of hydrogen.

Prof. AYRTON pointed out that the rubbing action in these experiments assisted the current by bringing up fresh electrolytic matter, a fact which had been taken advantage of in the construction of several batteries.

Prof. ADAMS remarked that this explanation did not seem to explain how the current was reversed in the cell composed of two tin-foil plates.

Prof. GUTHRIE then demonstrated by experiment a curious anomaly in frictional electricity. When flannel is rubbed with ebonite, the flannel is + electrified; when ebonite is rubbed with glass, the ebonite is + electrified and we should therefore expect that when flannel is rubbed with glass, the flannel would be still more + electrified but instead of that it is really feebly negative. Perhaps the fact that the heat of friction entered into one substance more than the other affected such results.

The SECRETARY then read a note from Mr. Ridout, stating that he had succeeded in Dr. Guthrie's funnel experiment, mentioned at last meeting, and by means of a stream of water flowing out of a glass funnel had attracted a glass cone towards the mouth of the funnel. The angle of the cone was greater than the angle of the funnel.

BIRMINGHAM PHILOSOPHICAL SOCIETY.

THE Council of the Birmingham Philosophical Society, having taken into consideration the advisability of establishing an Endowment of Research Fund, submitted the following scheme for the consideration of the Society at its meeting on March 11th.

"SCHEME FOR ESTABLISHING AND ADMINISTERING A FUND FOR THE ENDOWMENT OF RESEARCH IN BIRMINGHAM."

"The Council are of opinion that this Society would be omitting a principal means of the advancement of science—the end for which all such associations exist—if it neglected the question of the Endowment of Research. To maintain a successful investigator in his labours, even though no results of immediate or obvious utility can be shown to spring out of them, is of interest to the community at large. Indeed it is just because the practical usefulness of such work is not immediate or obvious, that it becomes necessary to give special support; for, otherwise, it would have its own market value, and endowment would be superfluous. But the proper and effectual administration of an Endowment Fund is perceived to be so beset with difficulty, as often to deter even those who recognise the principle from advocating it in practice. Most of the dangers usually foreseen would, however, as a rule be avoided, simply by the distribution of such funds from local centres, under such a scheme as is now proposed.

"THE Council are therefore anxious to establish a fund, in connection at once with the Society and the Town, for the direct endowment of scientific research. And they are further of opinion that the eminent merits of Dr. George Gore, F.R.S., as an investigator of exceptional originality and success in the domain of chemistry and physics, clearly point him out as fittest to be the first recipient of endowment from the fund. In accordance with these views the Council propose the following regulations for the fund:—

"General Regulations."

"I.—That the Fund be entitled, 'The Birmingham Endowment of Research Fund.'

"II.—That contributions be invited, payable either at once, or in instalments distributed over

term of years, as individual subscribers may desire.

"III.—That the money collected be deposited with the Birmingham Banking Company, in the name of the Council of the Birmingham Philosophical Society; and that all cheques on this fund be signed by the President, the Treasurer, and one of the Secretaries for the time being.

"IV.—That the management of the fund shall be in the hands of the Council of the Birmingham Philosophical Society, who shall have the power of allotting such sums and under such conditions, as they may deem fit, to any one or more persons engaged in scientific research, for the purpose of assisting them in carrying on their investigations.

"V.—The Council shall present a Report of their proceedings in connection with the fund at the Annual Meetings of the Society.

"Subject to the approval by the Society of the General Regulations, the Council have resolved—

"I.—That Dr. George Gore, F.R.S., be elected as the first recipient of an endowment from the fund.

"II.—That in order that Dr. Gore may have greater facilities for continuing in Birmingham his original researches, if the sum collected permit, the amount of £150 per annum for three years be allotted to him.

"III.—That the first cheque on the sum subscribed be payable on the 1st of July of the current year.

"H. W. CROSSKEY, } Secretaries."
R. LEVETT,

The above resolutions were carried unanimously at a full meeting of the Society on March 11, 1880.

NEWCASTLE CHEMICAL SOCIETY.

General Meeting, January 22, 1880.

Mr. R. C. CLAPHAM, President, in the Chair.

THE minutes of the last meeting were read and confirmed. The names of Mr. W. Beer and Mr. T. T. Sill were read for the first time.

THE PRESIDENT—I am very glad to be able to report that the two Chemical Societies are now practically united. I hope that the amalgamation will prove beneficial to both. Since we last met, one of our members, Mr. Maclear, has been conducting experiments on the crystallisation of carbon. Members may possibly have noticed the correspondence which has taken place on the subject in the *Times*, and I had hoped to have congratulated the Society to-night on the successful termination of the experiments. I have, however, had a letter from Mr. Maclear on the subject, in which he states that the substance which was supposed to be crystallised carbon is in reality not so. It is due to Mr. Maclear to state that, with a frankness which we must all admire, he wrote to the *Times* announcing his mistake as soon as he had discovered it.—The first paper which is down for discussion to-night is Mr. Berkley's, on "Magnesian Limestone Bricks." I may mention that I have had a letter from Smyrna from a Mr. Hadkinson, announcing that he is prepared to supply Greek stone in unlimited quantity at about £3 per ton, delivered in the Tyne.

MR. LYTE—I believe we could deliver the same article at about £2 per ton.

MR. PATTINSON—I don't think that it will be likely to come into the market at either £3 or £2 per ton when we have a material at hand which answers all the practical purposes, and costs about one-twentieth of the price. The

bricks which are made in Cleveland last for an almost indefinite time. In Bolckow and Vaughan's experimental furnace the sides lasted for over three months, only the bottom of the furnace goes. I should like to mention that I was very glad to see Mr. Snelus's claim, as the inventor of the process which was afterwards developed by Thomas and Gilchrist, so fully recognised in the addendum to the President's paper which was read at the last meeting.

THE PRESIDENT—The next paper is Dr. Lunge's on "Noxious Vapours." We shall be glad to hear the remarks of members on this subject.

MR. PATTINSON—The subject is a very important one; it is difficult, in fact almost impossible, to say, from the appearance of a crop which looks unhealthy, whether it has been destroyed by noxious vapours or by frost, or atmospheric influences. If a tribunal was appointed to hear actions for damage from noxious vapours, composed of scientific men and practical chemists, who had power to use the information obtained from the inspector's reports and from their daily tests, they would very much better apportion damages than the ordinary jury at an assize trial. I think legislation in that direction is very desirable, and I should hope that Parliament, having received the report of the Commission appointed to investigate the subject, will lose no time in taking steps in the matter.

MR. HILLS—I agree with Mr. Pattinson, that it is quite impossible to tell from the appearance of a crop the agent which has caused the damage. The average jury will generally lean towards the farmers—many of them are farmers themselves—and are really quite incapable of estimating the value of scientific evidence in a case of this nature. I should like to ask Mr. Pattinson's opinion as to the relative amount of damage caused by HCl and SO₂ in approximately equal quantities?

MR. PATTINSON—Dr. Smith has gone into that question. For myself I am inclined to think that damage is done mostly by sulphuric acid, which escapes into the air from the roasting of copper ores and regulus.

THE PRESIDENT—The information which Dr. Smith and his staff are giving to the world is certainly most valuable. Not only do the reports show that the escape of HCl has greatly decreased under the able inspectorship, but they go into many other matters as well which now, or are likely to become, of great interest. We have next to take the discussion on Mr. Swan's paper on "The Smoke of an Electric Lamp."

PROFESSOR HERSCHEL—I saw Mr. Swan to-day and he told me that the meeting to-night had quite escaped his memory, as indeed we might expect under the circumstances, and that he could not come. He therefore wished the discussion to be, if possible, postponed. He was so good as to let me have one of the lamps which had escaped the fire, and I have it on the table, wishing to exhibit by its means, after the meeting, the power of a small hand Gramme machine. This carbon candle is one of three which were saved from the fire, and as the whole of Mr. Swan's magnetic electric machines and the engine also escaped, we may hope that Mr. Swan's researches in this direction will not be long interrupted, and that he will shortly give us a lamp of greater permanency than Mr. Edison's cardboard horseshoe.

THE PRESIDENT—I think, if no member has any remarks to make on Mr. Procter's paper, our next business is to listen to a paper by Dr. Lunge on "Weldon Mud."

"On the Composition and Analysis of Weldon Mud," by Dr. LUNGE, Professor at the Federal Polytechnicum, Zurich. (See page 129).

DISEASE amongst Bees.—This pestilence, which is now raging in the province of Biella in Italy, is traced to minute organisms, which resist the action of potassic hydrate, ether, chloroform, and of desiccation, but succumb gradually if placed in concentrated sulphuric and nitric acids.—*Les Mondes*.

CORRESPONDENCE.

ACTION OF MERCURY ON SOLUTION OF SODIUM IN AMMONIA.

To the Editor of the Chemical News.

SIR,—Messrs. Hannay and Hogarth, in their paper "On the Solution of Solids by Gases" (CHEMICAL NEWS, vol. xli., p. 103) describe experiments made with solution of sodium in liquefied ammonia, and mentioned incidentally that in their first trial "an unforeseen difficulty caused the experiment to fail, for the sodium solution coming in contact with the mercury was rapidly decolourised by it" (p. 105). This result is interesting, bearing as it does upon the views expressed by Prof. Seely in his article "On Ammonium, and the Solubility of Metals without Chemical Action" (CHEMICAL NEWS, vol. xxiii., p. 169).

Prof. Seeley found that sodium amalgam was unaffected by liquefied ammonia, though he expresses the opinion that "it is probable that at a higher temperature, or with a larger excess of sodium, some of the sodium would be removed;" and adds, "I have not yet made the experiment to determine whether mercury will remove sodium from its ammoniacal solution, but it seems extremely probable that it would so act." It is this latter remark which Messrs. Hannay and Hogarth's experiment confirms, but it appears rather to negative the proposition laid down by Prof. Seely that "the force, whatever it be, which keeps together sodium and mercury is certainly weaker than that exhibited between sodium and ammonia," and to weaken still further his already weak argument that "If sodium amalgam is not a definite chemical compound, then surely the blue liquid cannot be." The "if" in the last sentence begs the question, for we have proof, on the other hand, that sodium amalgam is a definite chemical compound, by the evolution of heat on bringing the two metals together, by the product crystallising on cooling, and by the definite composition of the crystals. The truth seems rather to be that the force exerted between the sodium and mercury is greater than that exerted between the sodium and ammonia; and for this reason, and from the absence of evolution of heat when ammonia and sodium come together, and the ready volatility of the ammonia, leaving the sodium sometimes in crystals, one may safely conclude that at ordinary temperatures the solution of sodium in liquefied ammonia is simply a physical and not a chemical one.—I am, &c.,

W. H. WOOD.

Northgate Chambers, Halifax,
March 10, 1880.

BLOWPIPE ANALYSIS.

To the Editor of the Chemical News.

I HAVE just obtained a copy of Landauer's "Blowpipe Analysis" (Macmillan and Co.), and I must own that I was curious to see how the first orthodox (?) writer on this subject since Major Ross would deal with his predecessor. I have so much respect for Major Ross that I do not like to say that he is wrong in some of his views, I will only say that on some points (vesiculation and ellychnine pyrochrome, for instance) my experience does not coincide with his, but that he has taught the world at large more in the way of blowpipe analysis than anyone except Berzelius and Plattner is, I think, undeniable. But he has, at the same time, said some hard things, and some things which are awkward to refute, about one or two pet chemical theories of the present day, and this has caused a certain amount of opposition to him. Landauer has adopted Major Ross's discovery of aluminium plate as a substitute for charcoal so far as obtaining metallic incrustations go, and he does so with the proper acknowledgments and references, so that it is the more inexplicable why it is that he has overlooked the equally valuable discovery of the reac-

tions of the earths with boracic acid. These reactions determine (at once and clearly) lime and magnesia in all combinations except with phosphoric and silicic acids, and after some experience even with silica, if the proportion of silica is not too great. Also, by means of the differences between the reactions with boracic acid, borax, and microcosmic salt: alumina, silica, lime, magnesia, barium, and strontium may be distinguished from each other in salts certainly, and in mineral combinations generally; the predominant ingredient is always distinguishable. Now here we have an extension of blowpipe analysis such as has not been given to it since the days when Gahn first employed platinum wire as a holder for the borax bead, and yet not the slightest notice is taken of it in this the latest work on the subject. That this discovery will not be lost I am well aware, but it is in the hope of drawing attention to it and making it more generally known that I am induced to write you this letter.—I am, &c.,

TRITON.

TRANSPARENCY OF GOLD.

To the Editor of the Chemical News.

SIR,—I have frequently seen the statement that gold has the property of transmitting light when in thin plates, and Faraday's experiments are frequently quoted as demonstrating this fact. I have never yet seen the following experiment given, although it may have been noticed before.

If a solution of gold in *aqua regia* be neutralised with carbonate of soda, and the gold precipitated by adding solution of oxalic acid to the hot gold solution, the gold is precipitated as a yellow powder, showing bright gold-coloured spangles. On examining this precipitate by the microscope these spangles will be found to be triangular and hexagonal plates which transmit light, the colour of the light being dependent on the thickness of the crystal, and when one crystal happens to overlie another the edges are sharply defined by the difference in colour.—I am, &c.,

G. P. GIRDWOOD.

Laboratory, McGill College University,
Montreal, February 21, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 7, February 16, 1880.

Studies on Persulphuric Acid.—M. Berthelot.—The author has succeeded, by electrolysis, in preparing liquids containing 88 grms. of persulphuric acid, S_2O_7 per litre, and even reaching 123 grms.; the latter liquid containing at the same time 375 grms. of sulphuric acid, S_2O_6 , and 850 grms. of water. The relation between the weights of the two acids is 1 : 3, and the liquid contains seven times its volume of active oxygen. He obtains these results by placing dilute sulphuric acid, for instance, $SO_4H + 10HO$ in a porous vessel surrounded by a concentric vessel filled with the same liquid; the liquids are cooled by means of water circulating in two worms, and the electrodes consist of stout platinum wires soldered in glass tubes. It is convenient to employ 6 or 9 Bunsen elements grouped by twos or by threes. The slow decomposition of the persulphuric acid is attended with the formation of oxygenated water.

Certain New Derivatives of Nicotine.—A. Cahours and A. Etard.—The authors have previously (*Comptes Rendus*, lxxxviii., p. 999) obtained and described thio-tetrapyrudin. They have since obtained from it an acid, the

nicotianic or carboxy-pyridic of Laiblin, and a compound, which they regard as iso-dipyridin, and which is readily separable from nicotin by its insolubility in cold water.

New Interference Fringes.—M. Gouy.—The fringes which the author describes are produced by a single wave, resembling in this respect the supernumerary bows which sometimes accompany the rainbow, the theory of which has been given by Mr. Airy.

Density of Certain Gases at High Temperatures.—J. M. Crafts.—As a preliminary to experiments upon chlorine the author has made observations upon other gases—ammonia, carbonic acid, hydrogen, nitrogen, and hydrochloric acid. As regards the latter he has found its density normal at the highest temperature of the furnace. In a note the author suggests that the oxygen found by M. Meyer, and ascribed by him to the dissociation of chlorine, may have had its origin in the passage of watery vapour through the sides of the porcelain cylinder.

Action of Water upon Silicon and Boron Fluorides, and Solution of Cyanogen in Water.—H. Hammerl.—The mean heat disengaged by the reaction of an equivalent of SiF_4 upon an excess of water is $+22.34$ cal. That liberated by an equivalent of BF_3 under the same circumstances = $+24.51$ cal. In case of cyanogen the mean heat liberated by the solution of cyanogen = $+6.8$ cal. per molecular vol. 22.3 litres.

Reproduction of Amphigen.—P. Hautefeuille.—This mineral may be artificially produced, either by the method of MM. Fouqué and Lévy, or that which the author has used for the reproduction of orthose and albite.

Martite of Brazil.—M. Gorceix.—This mineral is found in abundance in Minas Geraes in brilliant crystals of an octahedral form, and yielding a red powder when crushed. The author considers that they are merely modified pyrites

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Moniteur Scientifique, Quesneville.
January, 1880.

Discussion on the Etiology of Carbuncle.—MM. Colin and Pasteur.—An account of a discussion at the Academy of Medicine.

Theory of the Saccharification of Amylaceous Matters.—M. Dubrunfaut.—Already noticed.

Report on Petroleum.—J. Lawrence Smith.—A report drawn up in connection with the Philadelphia Exhibition, containing much valuable matter but quite incapable of abridgment.

Determination of Soluble Phosphoric Acid in Superphosphates.—MM. E. Wein, Roesch, and J. Lehmann.—Already noticed.

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Les Mondes, Revue Hebdomadaire des Sciences.
No. 5, January 29, 1880.

Description of the Processes for Plating with Nickel.—Paul Laurencin.—This summary, taken from *Science pour Tous*, is intended to meet the requirements of amateurs.

Mechanical Equivalent of Fat in Cattle being Fattened.—M. Dubrunfaut.—The author points out that the increase of muscular fibre (meat, properly so-called) is not connected with the fattening process. An adult ox properly fed does not materially change in weight. If yoked to the plough it effects in a day of nine or ten hours a quantity of mechanical work, not less than that of a good horse. This work is effected by the consumption of 20 kilos. of good hay and may be valued at 2,160,000 kilogrammetres, representing a thermic equivalent of 5080 calories. The same animal put to fatten on the same diet without the production of mechanical labour yields a quantity of fat not less than 1 kilo. daily. We have then the equivalence of two numbers which express respectively the value of mechanical work and the production of fat. But 1 kilo. of good fat in normal combustion liberates 8 to

9 thousand calories; or about double the thermic equivalent of the mechanical work of the animal.

Dynamometric Machine of M. Lotin.—This paper cannot be intelligibly reproduced without the accompanying illustrations.

No. 6, February 5, 1880.

Light emitted by Gases under the Influence of Electric Discharges.—E. Wiedeman.—The author seeks to explain the formation of the different kinds of spectra by means of the kinetic theory of gases.

Extraction of Perfumes with Methyl Chloride.—Already noticed.

Aluminium Wires for the Telegraph.—The conductivity of aluminium is double that of iron; it is also lighter and a thinner wire can be used. As its high price is a practical difficulty an alloy of aluminium and iron has been suggested.

Application of the Microphone to Physiological Researches.—G. Trové and H. de Boyer.—The authors have modified the microphone for this purpose, and have utilised it in the study of muscular contraction.

MISCELLANEOUS.

Society of Arts.—The following are the papers to be read at the meetings of the Society of Arts after Easter, so far as the arrangements are yet complete:—April 2, "The Best Route for a Line of Railway to India," by B. Haughton, C.E. April 6, "Art in Japan," by C. Pfoundes, April 7, "Buildings for Secondary Educational Purposes," by E. C. Robins, F.S.A., F.R.I.B.A. April 8, "Recent Improvements in Benzine Colours," by F. J. Friswell, F.C.S. April 14, "The History of the Art of Bookbinding," by Henry B. Wheatley, F.S.A. April 16, "Russia's Influence over the Inhabitants of Central Asia during the last Ten Years," by Prof. Vambéry. April 21, "The Present System of Obtaining Materials in use by Artist Painters, as compared with that of the Old Masters," by W. Holman Hunt. April 22, "On some Recent Advances in the Science of Photography," by Captain Abney, R.E., F.R.S. April 27, "Iceland and its Resources," by C. G. W. Lock. April 28, "Recent Improvements in Gas Furnaces for Domestic and Laboratory Purposes," by Thomas Fletcher. May 5, "The last Forty Years of Agricultural Experience," by John C. Morton. May 7, "The Present Condition and Prospects of Agriculture in South India," by W. Robertson, M.R.C.A. May 13, "The Optical Properties of Crystals, and some of their Practical Applications," by Prof. W. G. Adams, F.R.S. The course of Cantor Lectures, which will be delivered during the same period, will be the third for the present session. It will consist of six lectures, by Mr. R. W. Edis, F.S.A., on "Art Decoration and Furniture," to be given on the following dates:—April 5, 12, 19, 26; May 3, 10.

MEETINGS FOR THE WEEK.

MONDAY, 22th.—Medical, 8.30.
— Royal Geographical, 8.30.
TUESDAY, 23rd.—Civil Engineers, 8.
— Anthropological Institute, 8.
WEDNESDAY, 24th.—Geological, 8.

NOTICE TO ADVERTISERS.

In consequence of GOOD FRIDAY occurring in the ensuing week, the CHEMICAL NEWS will be published on Thursday next, March the 25th. Advertisements must therefore be forwarded to the Office not later than 10 a.m. on Wednesday, the 24th instant.

THE CHEMICAL NEWS.

VOL. XLI. No. 1061.

A CONTRIBUTION TO THE KNOWLEDGE OF CHLORINE.

By V. MEYER and H. ZÜBLIN.

THE researches of C. Meyer and of one of the present writers on the behaviour of chlorine and iodine at high temperatures prove the occurrence of a decrease of density to the extent of one-third of the normal value as quite universal in the case of iodine, whilst as regards chlorine, the like phenomenon is demonstrated only in one special case, that is when the chlorine is applied in the nascent state. The experiments on chlorine were not, like those on iodine, performed with the free element, but with one of its compounds, platinous chloride, which evolves chlorine gas during the experiment, so that the latter is always in the nascent state. It appeared, therefore, important to examine how preformed free chlorine would behave at the same temperatures; or, in other words, to determine the vapour-density of free chlorine gas at a red heat.

A porcelain vessel, maintained at a yellow heat in a Perrot furnace, was completely filled with pure dry chlorine, which had been evolved from hydrochloric acid and manganese peroxide, and successively purified by water, sulphuric acid, and phosphoric anhydride. After the chlorine had remained for some time in the apparatus, and had assumed the temperature of the furnace, it was expelled by means of a current of carbonic acid, led into a solution of potassium iodide, and its quantity was determined by titrating the liberated iodine with a decinormal arsenite solution. The porcelain vessel, still at the same heat, was filled with dry air, and this also, after having assumed the temperature of the furnace, was expelled from the apparatus by means of a stream of carbonic acid, collected in a graduated tube over potassa lye, and its volume was determined in the usual manner. Thus were ascertained, *a*, the quantity of chlorine, *b*, the quantity of dry air contained in one and the same porcelain vessel at the same temperature, forming the needful data for calculating the density of the chlorine. The relatively small quantities of air or of chlorine contained in the handle of the apparatus which projects out of the furnace and in the short and narrow entrance- and exit-tubes, were determined separately and deducted from the total quantities of air (respectively chlorine), so that only such portions of air or chlorine as were in the ignited parts of the apparatus were taken into account. The apparatus at a yellow heat contained:—

Chlorine, 0.06529 grm., 0.06706 grm., 0.06670 grm.

Air, 0.02539 grm. (mean of three determinations.)

Hence the density of chlorine was found as 2.57, 2.63, 2.64, or in the mean 2.61. This number agrees satisfactorily with the value calculated for $\text{Cl}_2 = 2.45$, whilst when operating with nascent chlorine from platinous chloride at low temperatures, a similar value (2.42 and 2.46) was obtained, but at a yellow heat the numbers 1.65, 1.66, 1.66, 1.67, 1.60, 1.62, corresponding to 1.63, the value calculated for $\frac{1}{3}\text{Cl}_2$.

From these experiments there follows the interesting result that the dissociation of chlorine to molecules of the magnitude $\frac{1}{3}\text{Cl}_2$, which takes place on the employment of nascent chlorine at a yellow heat, does not appear at the same temperature when ready-formed free chlorine is used; whilst, on the other hand, iodine, even when applied as a free halogen, undergoes the conversion into $\frac{1}{3}\text{I}_2$, and at a decidedly lower temperature.

What may be the cause of the different behaviour of nascent and of free chlorine cannot for the present be ascertained. It may perhaps be sought in the less stable state, which bodies are apt to show during the nascent state. But it is also possible that other and more mechanical causes may occur, upon which the authors will enter in a future memoir.

They wish to point out for the present that the existence of chlorine Cl_2 , and of chlorine $\frac{1}{3}\text{Cl}_2$, at one and the same temperature, as these experiments prove, is distinctly analogous to the fact that oxygen and ozone, which in their respective densities show the same relation as the two modifications of chlorine, can exist at the like temperature and even side by side.

The above-mentioned experiments were completed when the authors received information of the researches of M. Crafts (*Comptes Rendus*, xc., p. 183), on the density of the halogens at ignition temperatures. His results agree with theirs most perfectly. M. Crafts also found for iodine the density $\frac{1}{3}\text{I}_2$, but for chlorine, which he introduced into the apparatus in the free state, Cl_2 .—*Berichte der Deutschen Chem. Gesellschaft*.

BALMAIN'S LUMINOUS PAINT.*

By C. W. HEATON, F.C.S.

I HAVE been invited to give this evening an account of one of those practical applications of science which have always had peculiar interest for members of this Society. In doing so I shall be compelled to allude briefly to some scientific truths, familiar to many among my audience, and particularly to those who attended Mr. Preece's recent and interesting lectures.

Everyone knows that a ray of white light consists of minute waves, or ripples, analogous to those of water, and of various lengths. The mean length of these waves, measured from the crest of one to the crest of the next, may be taken as the fifty-thousandth of an inch. They all travel in straight lines, and all with the same velocity, a velocity which is called the velocity of light. In passing through a prism, these lines of waves are refracted or bent, at different angles, and when afterwards allowed to fall on the screen they are reflected back to our eyes, and a portion of them, acting on the optic nerve, produce the sensation of colour. These rays are, therefore, called the visible rays. Here on the screen you see the spectrum of the white electric light.

The eye is, however, a very imperfect optical instrument, and is only sensitive to waves between certain limits of length. The real spectrum on the screen is much larger than the visible one. There is a portion beyond the red which is called the ultra-red portion. The waves of this portion are too long to affect the eye, but are powerful in producing the phenomena of heat. The rays are therefore sometimes called the invisible or obscure heat-rays. There is also a portion beyond the violet, produced by waves too short to affect the eye, but active in producing chemical and other effects. These rays are called the ultra-violet, actinic, or chemical rays.

Many years ago Sir John Herschell observed the peculiar blue opalescence which a solution of quinine presents when held in certain lights. Brewster made further observations of the same kind; but it was not until 1852 that Prof. Stokes, of Cambridge, published the memorable research on the subject which would alone have been sufficient to render his name immortal in the history of science. Stokes suggested the name fluorescence for the phenomenon, and showed that it was due to a power which some bodies possess of absorbing light waves and emitting them again as longer waves. Quinine is peculiarly sensitive to violet and ultra-violet rays. If I pass a solution of quinine through the spectrum, beginning at the red end, you will

* A Lecture delivered before the Society of Arts, March 11, 1880.

see that the fluorescence does not appear until we reach the blue portion, but continues to be visible far beyond the violet. In the ultra-violet portion we have a distinct conversion of invisible actinic into visible light rays. Here is a solution of the new Magdala red made from naphthalin. It appears black in all parts of the spectrum except the region of the yellow, in which it shows a magnificent red fluorescence. This experiment proves that fluorescence is not always caused by the conversion of invisible into visible waves. We now throw a beam of electric light, filtered to some extent from heat-rays by passing through ammonio-sulphate of copper, on two jars of water, and throw into one a solution of quinine, and into the other an alkaline solution of the curious compound called fluoresceine. You perceive in the one case a blue, and in the other an intense emerald-green fluorescence, and it is really difficult to believe that in each case the solution remains transparent. In fluorescence, then, we have generally, if not always, an increase of wave-length due to the molecular action of certain substances. The waves are never shortened, and are sometimes described as suffering "degradation." The luminosity ceases as soon as the incident light is withdrawn, or, to speak more cautiously, it lasts only for a minute period of time afterwards. In the case of quinine the work of converting invisible into visible rays seems to take about the 5000th part of a second.

We come now to a closely allied, if not identical, phenomenon. Many substances are known which, having been exposed to light, continue to emit light for measurable periods of time after the luminous source is removed. This phenomenon is called phosphorescence, and it is with this that we are chiefly concerned to-night. The term phosphorescence has been applied to light emitted under various conditions. Thus we have the light emitted during the slow oxidation of phosphorus; the light of living animals, such as the glowworm and the noctiluca; the light attendant upon incipient putrefaction, animal and vegetable; the light produced by the heating of phosphorus and many other bodies; by percussion, as when sugar is struck with a hammer or rubber; and the beautiful phenomena developed in high vacua in that strange condition of matter with which Mr. Crookes's remarkable discoveries have lately made us familiar; and, lastly, we have the light which follows insolation. All these are grouped together under the general term phosphorescence, but it is with the last one that we have at present to deal.

The history of discovery in regard to this kind of phosphorescence is a somewhat lengthy one. Obscure hints of the phosphorescent power of gems are contained in some of the works of the ancients, but nothing definite was discovered until about the year 1602, when a shoemaker of Bologna, named Vincenzo Casciorolo, picked up a heavy stone, and being an enthusiastic alchemist, took it home and treated it in his furnace, in the hope of obtaining gold from it. Instead of getting gold he obtained a mass which shone in the dark, and which became celebrated as the Bologna stone, or Bologna phosphorus. The stone was the now well-known heavy spar, and acted on by carbon it yielded barium sulphide, a compound of considerable phosphorescent power. In 1663 Boyle published some observations on the phosphorescence of the diamond, and in 1675 Baudouin obtained another phosphorus by the action of heat on calcium nitrate. Some important additions to our knowledge were made by Beccaria in 1744, and then came the discovery by Canton in 1761, of the phosphorus or pyrophosphorus which bears his name. It is made by calcining oyster shells with sulphur, and consists, of course, essentially of calcium sulphide, which is the most remarkable of phosphorescent substances. Thanks to the kindness of my friend, Professor Tuson, I am able to show you a sealed tube containing a specimen of this substance prepared by Canton himself, with the date 1764 engraved on the glass. It still retains its power, a remarkable proof that the luminosity is not due to any chemical action.

In 1775 Wilson published some valuable discoveries. He extended the list of phosphori, showed that light of various colours could be obtained from them, and made some interesting observations on the effects of heat and cold, to which I shall refer again. I have here a sheet of cardboard painted with various phosphorescent compounds, and a butterfly painted on glass in the same manner. On exposing them to light you will observe the different shades of colour, varying from red to blue, which are developed.

Passing by, in this imperfect sketch, several important names, we come to the greatest name of all, Edmond Becquerel, who for more than thirty years has enriched science by his discoveries. It is impossible for me to give even an abstract of these discoveries. He invented an instrument called the phosphoscope, by which he was able to detect the phosphorescent power in bodies which remain luminous for very short periods of time. He showed that the power was a very common one in natural as well as in artificially prepared substances, and that it was possessed by organic as well as inorganic compounds, and particularly by all fluorescent substances. He analysed the light emitted in each case by the spectrum, and determined its duration within certain limits of accuracy. These limits are singularly wide, extending from less than the 5000th part of a second to hours or even days. Of all phosphorescent bodies, the sulphides of calcium, barium, and strontium are the most remarkable in regard to the duration of their light; and Becquerel has studied the modes of preparing these compounds with the greatest care, and has revealed many strange and even startling facts in regard to them. They can be prepared by the action of carbon on the sulphates, by the action of sulphur on the oxides and carbonates, and by the action of alkaline sulphides, and even of antimony sulphide, on the last-named compounds. The phosphori prepared by these various processes differ widely in regard to the colour and duration of the light which they emit, and it is even possible to obtain shades varying from orange-red and indigo-blue from calcium only. But this is not all. The calcium sulphide obtained from calcite is different from that from arragonite, though these minerals have the same chemical composition; and the differences remain even after each mineral has been dissolved in acid, re-precipitated by sodium carbonate, and then ignited with sulphur. This affords a strange and, for the present, inexplicable example of molecular persistency. Becquerel holds that fluorescence and phosphorescence are one and the same phenomenon, the apparent differences being due solely to the different periods during which the absorbed light is emitted. The point is one which merits attentive study.

We now come to the immediate subject of my lecture. The late Mr. Balmain, formerly of University College, and an excellent chemist, had for many years made a special study of phosphorescent substances. He enjoyed the friendship of the Becquerels, father and son, and, aided possibly by them, he succeeded in producing a constant and very powerful phosphorescent substance. Here is some of the substance. You see how strong a light it emits after exposure to the magnesium light. It occurred to him to patent the use of this and other phosphorescent materials, mixed with water or oil, as paints capable of being applied in the ordinary way to any surface. This patent for luminous paint is now in the hands of Messrs. Ihlee and Horne, of Aldermanbury, and, thanks to those gentlemen, I am able to show you a great many specimens of articles thus painted, and so to enable you to judge for yourselves of its proposed applications. I have, in the first place, a number of sheets of cardboard so painted, some of them framed and glazed, which you see emit, after insolation, an amount of light which is remarkable. With these sheets we can try a few preliminary experiments, which will assist in illustrating the nature of the phenomena with which we are dealing. Let us, in the first place, test the sensitiveness of the paint. Exposing a sheet of painted card, previously kept in darkness, to Leyden-jar sparks from an induction-coil, you see that each spark impresses

its image on the card and makes it luminous. It is easy in this way to write luminous words on an otherwise dark card. You know how very short a time each spark lasts, and can therefore judge of the short time required for the illumination. The same fact is more roughly shown by striking a lucifer-match in front of a dark pane. The momentary flash of the tip is, you see, enough to produce a very visible effect on the paint.

We may now try the very obvious experiment of applying light of various colours to the painted surface. Here is a card partially covered with sheets of blue, green, yellow, and red glass. Burning a piece of magnesium in front of it, you see that no effect at all is produced by the red and yellow light, a considerable effect by the green, and most of all by the violet light. The same effect may be produced in another and less ambiguous manner. We will throw the spectrum on a white screen, which consists of five horizontal layers. The upper one is white paper, the next, sensitive photographic paper*; the next, paper saturated with quinine; the next, luminous paint, which at present is dark, and the lowest luminous paint previously insulated. The spectrum covers all these layers. A long exposure is required to give a very satisfactory result, but we are then able to make out that there is great general similarity between the rays which affect the silver, the quinine, and the luminous paint. The affected portions are nearly, but not quite, coincident. If the experiment is carried on for a long time with a very intense spectrum, another effect which we could not have anticipated is observed in the lowest layer, which has previously been made luminous. The violet and ultra-violet rays increases the luminosity, as we might have expected, but the yellow and red rays not only do not add to the luminosity, but actually take away the light previously present. Another experiment will show this effect more quickly. Here is a sheet, at present luminous; I cover half of it with opaque cardboard, and the other half with yellow glass, and then burn a piece of magnesium in front of it. See the result. The rays which passed through the yellow glass, have taken nearly all the light out of that half of the card, while the light on the other remains unimpaired.

For an explanation of this curious phenomenon, we must go back 105 years, to Wilson, who first observed it, though in a different manner. We must remember that red and yellow rays are also heat rays; and Wilson showed that heat would stimulate the light for a time³ only to cause it more rapidly to disappear. On a faintly luminous pane I place a can of hot water. Removing it after a minute, we find that the light in that place is much brighter than before. Cold diminishes the light, which, however, returns when the original temperature is restored. This I can easily show by applying a block of ice for a minute to the luminous surface. Thus we see the importance, in all experiments on the duration of phosphorescent light, of paying close attention to temperature.

We may now inquire how far this luminous paint is suited for actual use, and what kind of applications can be found for it. We have seen that it is very sensitive to artificial light. To ordinary daylight it is at least equally sensitive, and I have seen it tried, with a success which seemed to me wonderful, in a thick yellow London fog. A very short exposure produces the maximum of illumination, though, of course, the amount of this illumination depends on the quantity and quality of the light. The duration of the light is likewise dependent, to some extent, on conditions. When the paint has been exposed to the intense light of the sun, or of burning magnesium, a good deal of the brilliancy disappears quickly, but after that the fading is very slow; and it may be said that a more or less useful light will remain through the length of an ordinary winter's night. In an extreme case, I was just able to see the dial of a watch by the light emitted from a card which had been in total darkness for twenty-six hours. In this case the card had been exposed to daylight of moderate intensity for two hours.

The next point to be considered is the durability of the paint. The oil seems to protect it wonderfully, even from the acid air and rain of London. I have seen a wooden bowl, which was painted by Mr. Balmain more than a year ago, and has for several months been exposed to the open air in London, in very bad weather. It still preserves its power. Equally effectual is the oil as a protection from water. Here you see the painted model of a life-buoy immersed in water. Its luminosity is very evident. In several respects, therefore, namely, as regards intensity of light, lasting power, and durability under fair condition, these painted surfaces seem likely to be useful as carriers of daylight into dark places and through dark hours.

Let us now turn to the suggested applications of the paint. They may be divided roughly into the ornamental and the useful, and I need only mention a few out of the many that occur to the mind. In the former class, we may include these painted india-rubber balls, which are very pretty, and suggest nocturnal lawn tennis. Statuary, grotto-work, and ornamental tiles, also give striking and beautiful effects. Here is a large bust of the Prince Consort which has been painted and illuminated. You see how fine an effect it produces. Clock faces, so prepared, show the time all night, and watch-boxes enable us to do as much with our watches. The ends of lucifer match boxes have been painted with the same material. Here are a number of them. There would obviously be no difficulty in finding one of these boxes in the dark.

To pass to more important applications, the roofs of railway carriages have already been treated experimentally with the paint, with results that appear satisfactory. The light would, of course, only be employed for daylight trains which run through tunnels. Then, again, it is proposed to paint the names of streets, the directions at post-offices, sign-posts, and advertisements in the same way. Here are some framed notices, "Lodgings," "To Let," and such like, which would easily be seen through, at any rate, a great portion of the night. Still more important is the use of this luminous power in gunpowder magazines, spirit stores, and for purposes of exploration in coal-mines during seasons of danger, and many such uses may be thought of.

But of all the proposed uses of the luminous paint, none seem to me so important, or so certain of practical success, as the marine applications. Several of these have already been realised in a satisfactory manner. For signal and mooring buoys, for example, its utility is obvious. It is of the utmost importance that the buoys employed for marking channels should be seen easily, the safe entrance to a harbour or river frequently depending upon the successful passage through a line of buoys. The entrances to Poole Harbour and to the mouths of the Thames and Medway are marked in this way, and great difficulty and danger are often experienced on dark nights in making out the buoys. Now, there can be no doubt that a buoy thickly painted with luminous paint would remain visible through the whole, or nearly the whole, of a night. A few evenings ago, I assisted at the trial of a small mooring-buoy at Erith. It was not placed in the water till nine at night, having, of course, been for some hours before in darkness. We found that it was easily visible at distances of over a hundred yards, although the night was not a clear one, and the light to which the buoy had been exposed had not been very good. Yachtsmen, returning to moorings late at night, would find such buoys a great advantage, and many collisions might in this way be avoided. Equally important is the application of the paint to life-buoys. Life-buoys are, or should always be, kept on deck in convenient situations. A life-buoy thrown to a man overboard will often save his life, particularly if the man can swim. But what is the use of throwing out a common life-buoy on a dark night? It is impossible for the man to see it, and equally impossible for those on board. Here is a life-buoy painted with the luminous paint. It has been on the roof of this house all day, and has, consequently, been now for several hours in

* Kindly prepared for me by Mr. J. M. Thomson, of King's College.

darkness. Yet you see it emits a light that could easily be seen at a very considerable distance. Any decently good swimmer could easily reach it, while the ships' boats would know at once the point to which they must row.

Finally, there seems no doubt of the usefulness of the paint in diving and other submarine operations. We have ready a diver in full dress, painted all over with the luminous paint. He is being illuminated in the next room. Now he enters, and you see what a brilliant and ghost-like appearance he presents. At present one of the greatest difficulties in submarine operations arises from the intense darkness. With this new arrangement every diver will be his own lantern, and will carry down with him a stock of daylight that will assist his labours materially. This dress was actually so used at Southampton a day or two ago. Mr. Phillip Hedger, the Superintendent of the Southampton Dock Company, has sent this letter to Messrs. Ihlee and Horne, in which he says that the diver descended on a somewhat murky day into 27 feet of not very clear water, and found that he was able to distinguish the bolt-heads and mussels on a ship's bottom with ease. Mr. Hedger seems to entertain no doubt of the success of the new application.

Let me, in conclusion, draw your attention to the splendid inscription, "Balmains Luminous Paint," which stretches in letters of fire the whole length of the hall. It has been in darkness for two hours, and yet illustrates very simply the way in which the paint may be made useful for advertising purposes.

I may now conclude this brief and very imperfect sketch. I heartily wish the exposition had fallen into better hands, and particularly that our Chairman, whose well-known researches on actinic light have added so much to our knowledge, would have taken the matter in hand himself. However, I have done my best, and can only hope that you have acquired some new ideas on the subject, and will in future watch with interest for further useful application of the luminous paint.

I must here express my sincere thanks to my friend Mr. Bolas, himself so excellent a lecturer, for his invaluable help in the experimental portion of my lecture.

THE LINES OF DISCOVERY IN THE HISTORY OF OZONE.

By ALBERT R. LEEDS, Ph.D.

I. ITS ORIGINAL DISCOVERY, SOURCES, AND PROPERTIES.

THE history of ozone begins with the clear apprehension, in the year 1840, by Schönbein, that in the odour given off in the electrolysis of water, and accompanying discharges of frictional electricity in air, he had to deal with a distinct and important phenomenon. Schönbein's discovery did not consist in noting the odour—that had been done by Van Marum more than half a century before—but in first appreciating the importance and true meaning of the phenomenon. For while Van Marum, Cavallo, and others who followed them, connected the odour with the electricity, calling it the "electrical odour" or "aura eléctrica," and thus made it the property of an imponderable agent, Schönbein ascribed it to the peculiar form of matter operated upon. The hypothesis of Van Marum necessarily remained barren of fruits; that of Schönbein speedily enriched chemical science with a host of acquisitions. Clinging tenaciously to the doctrine that there could not be a variety of origin for one and the same odour, and that the kind of matter producing it in every case must be identical, Schönbein fixed his discovery by giving to that one and certain kind of matter the name of Ozone. By adhering to this guiding clue, he added, as a third source of ozone, the action of moist phosphorus upon air (1840 to 1843), and since that time, besides electrolysis, electrical influence, and the action of air upon moist phosphorus, no

other sources of ozone of practical utility have been discovered.

The fact that Schönbein so stoutly insisted on, and eventually so triumphantly established, the *identity* of the ozone from whatever source derived, must not be lost sight of in any estimate of his merits as a discoverer. The earliest attack came from De la Rive, who attributed the odour to metallic oxides set free from the metals used as electrodes, or as terminals in electric discharges. But Schönbein pointed out that besides the improbability of an odour arising from solid bodies, this hypothesis required that solid bodies should have the property of indefinite suspension in the atmosphere instead of being deposited or washed down by water (1840 to 1843).

The next attacks came from Fischer, who regarded Schönbein's ozone as probably peroxide of hydrogen, and from Williamson, who thought there were two kinds of ozone,—one the ozone given off in electrolysis, and which he regarded as a higher oxide of hydrogen, differing from the previously well-known peroxide, and the other formed by the action of phosphorus on moist air. But Schönbein disposed of both objections: the first, by showing that the chemical and physical properties of ozone are not the properties of peroxide of hydrogen; the second, by demonstrating that whatever might be the true nature of ozone, the gaseous matter obtained in the electrolysis of water was in all respects identical with that formed by the action upon air of moist phosphorus (1844 to 1845).

During these first five years Schönbein was busily engaged in ascertaining the properties of ozone. Since no peculiar methods were employed in the furtherance of these discoveries, they need not detain us here further than briefly to summarise them, and to point out what corrections have been rendered necessary by the labours of subsequent investigators. They are—1st. Its eminent oxidising powers, as shown by its ability to transform most metals into their higher oxides, and to raise the lower oxides into the condition of peroxides. Certain of the non-metals—phosphorus, chlorine, bromine, and iodine—are similarly oxidised. Schönbein's statement that it does not unite with nitrogen under ordinary circumstances, but enters into combination when alkali is present, has been abundantly disproved—among others, by Berthelot (1878), who has shown that no combination occurs even when alkali is present. It oxidises sulphites and nitrites into sulphates and nitrates, and many sulphides into their corresponding sulphates. It destroys (as has since been more elaborately demonstrated by Houzeau, 1872) many gaseous compounds of hydrogen, like those with sulphur, selenium, phosphorus, iodine, arsenic, and antimony. It discharges vegetable colours, and powerfully attacks many organic bodies. The nature of its action in the latter case has been more extensively studied by Gorup-Besanez (1863), and he has described the products of the reactions which occur when ozone is allowed to act upon organic substances, alone or in presence of alkali. 2nd. According to Schönbein ozone is insoluble in water. The observations of subsequent experimenters conflict on this point, but there appears to be much evidence to show that it is soluble in water, though only in small degree. 3rd. Schönbein pointed out that atmospheric air strongly charged with ozone acts powerfully on the mucous membranes and produces symptoms of catarrh. This and his analogous discovery that ozone is present in the atmosphere, and plays there a very important part, attracted to the subject not only great popular attention, but enlisted as observers a multitude of students of medicine the world over, who hailed the newly-discovered body as an invaluable therapeutic agent, and rushed forward to establish by sufficiently numerous observations the relations between its presence or absence in the atmosphere, and the kind and prevalence of disease. Thirty years have passed away, and neither anticipation has been realised. Indeed, at the present hour, the possible value of ozone as a therapeutic agent is obscured by its having fallen into the hands of empirics; and the multiplication of inexact observations

and the crude and hasty generalisations therefrom, have covered with a sort of scientific opprobrium the whole subject of Atmospheric Ozone.

What causes have led to these lamentable results in the past; what prospects are there that both subjects can be reinstated in good scientific standing in the future?

And first with regard to ozone as a therapeutic agent. Without considering at present the unsettled questions of a medical character, as to the proper mode or amount or propriety of application, we apprehend that there have been hitherto three grave instrumental difficulties—1st. To obtain ozonised air or oxygen of known strength and of adequate purity. 2nd. It is doubtful whether in one form in which the attempt has been made to employ ozone in medicine, that of "ozonised water," any ozone whatever has been present. Such was the case with the "ozone-water" of Krebs, Kroll, and Co., in which Rammelsberg found *chlorine*. Since ozone is so slightly soluble in water at common temperatures that it is extremely difficult to demonstrate the fact of solution, the proposition to employ "ozonised water" as a remedial agent opens a wide door to quackery. 3rd. It is certain that from the mixture of potassium permanganate and sulphuric acid, which has been and is recommended as a convenient source of ozone for medical use, no ozone, but merely chlorine and oxides of chlorine (due to impurities in the permanganate) are derived.

These errors have been exposed and the difficulties overcome. There is no obstacle to having in the office of the physician, the sick-room of the patient, or the wards of the hospital, ozonisers suitable to each place, and adequate to supply ozonised air or oxygen of known strength and purity. This being the case it remains for the therapist to do his part of the work, and to discover when and how ozone is to be employed in legitimate practice.

Second, to detect the amount of ozone present at any time or place in the atmosphere, and the rôle this atmospheric ozone plays as a disease excitant or prophylactic. The objections which vitiate the observations hitherto made are two in number:—1st. The ozonoscopes hitherto employed, Houzeau's and the thallium-test included, are all affected by some one of the gaseous bodies possibly present in the atmosphere, as well as by ozone. 2nd. The method of conducting the observations is in its nature inexact, and variations in wind, temperature, humidity, &c., are allowed to increase the resultant errors.

Advance in this direction is to be looked for only when the methods at present in use are abandoned in favour of others more in harmony with those pursued in other branches of gas analysis, and when reagents are employed which will assign true values to the amounts of ozone determined.

II. THE NATURE OF THE CONSTITUENT MATTER OF OZONE.

In his speculations upon the nature of ozone, Schönbein was less fortunate than in his multiplied inquiries into its sources, properties, and applications. The difficulty at that time of procuring air or oxygen containing more than a minute percentage of ozone, and of manipulating it when obtained, was very great, so that precise quantitative investigations were attended with formidable obstacles, and probably for that reason were rarely instituted by Schönbein. He brought forth a variety of hypotheses, thus introducing great uncertainty into a confessedly difficult subject, and necessitating the labours of chemists during nearly a quarter of a century for their complete overthrow.

His earliest hypothesis was that ozone is a compound consisting of hydrogen and oxygen. This, in 1844, he abandoned in favour of the theory that ozone itself is elementary, and along with hydrogen enters into the composition of nitrogen, which is a compound substance. The following year he reverted to his original hypothesis, and, while maintaining strenuously that ozone is not peroxide of hydrogen, he nevertheless upheld the view that it is composed in certain unknown proportions of hydrogen and oxygen.

The second hypothesis was overthrown by the experiments of Marignac and De la Rive, who showed that ozone could not be derived from the decomposition of nitrogen, inasmuch as they obtained it by passing electric sparks through perfectly pure and dry oxygen. They proved the resultant body to be ozone, by causing it to react on moist silver and potassium iodide with the formation of argentic peroxide and iodate of potassium. They explained these reactions by supposing that under the influence of the electric discharge the oxygen had acquired an electrified condition, with exalted chemical properties: in other words, that ozone is oxygen, and oxygen only, but oxygen in an electrified state. Plausible as was this explanation, there was nothing in the experiments—water having been present in the reaction upon silver and potassium iodide—to confute the different interpretation brought forward by Schönbein, that ozone was oxygen to which in some way was added the elements of water. Nor was this point settled by a more elaborate experiment of the same nature, instituted by Fremy and Becquerel in 1853, who demonstrated that when a certain volume of oxygen is confined over an aqueous solution of potassium iodide, moist silver, or mercury, *all* of the oxygen undergoes absorption by the reagent under the influence of a sufficiently prolonged series of electric sparks.

The first to abandon the theory that hydrogen is a constituent of ozone was Schönbein himself (1849). He employed air, ozonised as strongly as possible by moist phosphorus, and afterwards dried by passage through a sulphuric acid drying tube. That water was employed in the generation of the ozone was not from Schönbein's point of view an essential element in the problem; it was whether this ozone after drying still contained the elements of water or hydrogen.

Three hundred litres of the desiccated air were passed through a narrow glass tube heated to redness, in order to decompose the ozone, and then through a second sulphuric acid drying tube. Since the latter in repeated experiments showed no increase of weight, Schönbein regarded the absence of hydrogen in ozone as conclusively proven. At the same time he did not accept the views of Marignac and De la Rive, declaring that to him the existence of an allotropic modification of a gaseous body was inconceivable. For a long time, however, the theory that ozone was a compound of hydrogen and oxygen prevailed. It derived great weight from the experiments which had been made by Williamson in 1845. He prepared ozone by electrolysis, and to avoid obtaining any hydrogen along with the electrolytic oxygen, used oxide of copper dissolved in sulphuric acid as the electrolyte. The gas was dried over calcium chloride, and then passed over ignited copper turnings into a second drying tube: this uniformly showed an increase of weight. The copper previous to ignition had been reduced by carbonic oxide, and not by hydrogen, in order to prevent the possibility of any occluded hydrogen being given up, on ignition, to the stream of ozonised oxygen.

These views were apparently confirmed by Baumert's experiments (1853). He passed the electrolytic oxygen evolved in such a manner as to exclude the presence of hydrogen, through a very long sulphuric acid drying tube, and thence into an absorption apparatus containing potassium iodide and provided with a sulphuric acid bulb-apparatus, to condense evaporated water. In case the matter of ozone and oxygen were identical, the weight of oxygen equivalent to the weight of iodine set free by the ozone should have been equivalent to the total gain in weight by the absorption apparatus. But, according to the experiments, this weight was less, and the numbers found apparently assigned to electrolytic ozone the formula H_2O_3 . And since Baumert found that ozone prepared by the electric discharge could not be made to yield up the elements of water on strong heating, while that prepared by electrolysis could, he regarded the two as different bodies—the former as allotropic oxygen, the latter peroxide of hydrogen.

Thus the old hypothesis, against which Schönbein had so long striven, that there were two (and possibly more) bodies of the nature of ozone, was rehabilitated. It was finally overthrown by Andrews (1856), who showed that the preceding experiments on electrolytic ozone had been vitiated by the presence of a small but appreciable quantity of carbonic acid, which, unless very great precautions be taken, is always present in the evolved gas. In very numerous experiments he showed that the weight of active oxygen equivalent to the weight of the iodine set free in the absorption apparatus was equal to the entire gain in weight of the apparatus, and therefore no hydrogen as well could have been present; also, that the properties of electrolytic ozone, and that obtained by the action of the electrical spark on pure and dry oxygen, were identical. More especially, it was shown that both were converted into ordinary oxygen at a temperature of about 237°C .; and from the whole investigation the author drew the conclusion, which was confirmed by the still more elaborate experiments of Soret in 1863, and is now universally adopted, "that ozone, from whatever source derived, is one and the same substance, and is not a compound body, but oxygen in an altered or allotropic condition."

III. THE EXACT NATURE OF THE RELATIONS EXISTING BETWEEN OZONE AND ORDINARY OXYGEN.

We have seen that Marignac and De la Rive, as the result of their experiments performed in 1845, had enunciated the view that ozone was oxygen in a peculiar electric state. They proposed to abandon the name "ozone," which assumed an independent chemical existence for this body, and to call it merely "electricised oxygen." This view of the constitution of ozone was one not readily susceptible of investigation by usual chemical methods. But the case was different with the hypothesis which was shortly afterwards advanced by Dr. T. Sterry Hunt, in 1848. Since his intuition of a truth, not fully demonstrated until twenty years later, is of a very striking character, it will be interesting to quote it as originally announced. In a paper on the anomalies presented in the atomic volume of sulphur and nitrogen, Dr. Hunt says—"In considering such combinations as SO_2 and SeO_2 , which contain three equivalents of the elements of the oxygen group, it was necessary to admit a normal species which should be a polymer of oxygen, and be represented by $\text{O}_3=(\text{OO})$. The replacement of one equivalent of oxygen by one of sulphur would yield sulphurous acid gas (OOS), and a complete metalepsis would give rise to (SSS). The first compound is probably the *ozone* of Schönbein, which the late researches of Marignac and De la Rive have shown to be in reality only oxygen in a peculiarly modified form, &c."

The hypothesis herein stated, that ozone is triatomic oxygen, necessarily involved the assumption of such a corresponding difference in density, and other physical properties,—differences admitting of exact quantitative proof or disproof. Such were the experimental difficulties in the way, however, that it was not until 1860 that an investigation was made into the volumetric relations of ozone to oxygen. The experiments of Professors Andrews and Tait then resulted in establishing that when perfectly pure and dry oxygen is converted into ozone, under the influence of the silent electric discharge, it becomes more dense, the amount of contraction being proportional to the quantity of ozone produced; also that when ozone thus condensed is exposed for a short time to a temperature of 270° to 300° it expands to its original volume. That the increase in density was exactly proportional to the amount of ozone formed was proven by an analysis of the contracted gas by means of potassium iodide. The amount of iodine in every case set free was precisely equivalent to the weight of a volume of oxygen equivalent to the volume of the contraction which the oxygen had experienced in the process of ozonation. The same laws were demonstrated to hold good with regard to electrolytic ozone, not

only by these authors (1860), but also by Von Babo and Claus and by Soret (1863).

Andrews and Tait found great difficulty in reconciling the theory of the allotropism of ozone with their experiments, inasmuch as the oxidation of a body like mercury, potassium iodide, &c., was effected without any diminution in the volume of the contracted gas. In other words, the density of the allotropic oxygen concerned in this oxidation was apparently infinite. They sought therefore to explain the origin of ozone by the assumption of a decomposition of the oxygen.

But in 1861 Odling put forth the interpretation that ozone was a compound of oxygen with oxygen, the combination being attended by a contraction. Hence if one portion of the combined or contracted oxygen were absorbed by an oxidisable body, the other portion would be set free, and by its liberation might expand to the initial volume. He likewise suggested that this contraction might consist in the condensation of three volumes of oxygen into two volumes, not because this ratio was the only one which would explain the volume and density relations, so far as then known, but because, on the hypothesis of the dual nature of oxygen, this was their simplest possible explanation. Four years later Soret discovered that a very remarkable reaction occurs when electrolytic ozone is allowed to act upon oil of turpentine. Its volume is diminished by a volume equivalent to twice that of the oxygen, corresponding to the iodine set free on passing the ozonised oxygen into a solution of iodide of potassium. The latter, it will be remembered, is the same as the diminution in volume which the oxygen undergoes in ozonation, and may be called the contraction-volume. Hence the two volumes of ozonised oxygen absorbed in Soret's experiments contained not only their own volume of oxygen, but that contained in the contraction-volume, or in all three volumes of ordinary oxygen. The density of ozone, therefore, was to the density of oxygen as three to two, or 1.6584 , the density of ordinary oxygen being 1.1056 .

Soret inferred rather than demonstrated these relations, inasmuch as in his first set of five experiments the ratio of the total volume of ozonised oxygen absorbed by the turpentine to the contraction-volume was 2.4 , and in his second set of seven experiments 1.81 , both of these results being far from 2, the theoretical number.

However, in 1872, Sir Benjamin Brodie, by the introduction of methods of exact volumetric character, supplied a rigorous experimental demonstration. He obtained in a set of eight concordant experiments made with oil of turpentine, for the ratio between the whole diminution in the volume of the original oxygen, to the diminution in the volume of the ozonised oxygen, as a mean result, 3.02 to 2.02 . Operating in the same manner with a neutral or slightly alkaline solution of sodium hyposulphite, he obtained, as a mean result of twenty-seven concordant experiments, the same ratio of 3.02 to 2.02 . In these experiments the actual weight of the oxygen absorbed could not be determined otherwise than by calculation from the alteration in volume. But by the oxidation of stannous chloride, under proper conditions, he effected a direct determination, and found that the weight of the oxygen absorbed from the ozonised oxygen by the stannous chloride was almost exactly three times the weight absorbed from the same gas by potassium iodide. At the same time, the volume in the first case was almost exactly twice the contraction-volume, as determined by the latter reagent.—*The Journal of Science*.

Rapid Alcoholic Fermentation.—Before determining the glycerin in certain wines it has been found necessary to destroy the sugar by dilution with water and the repeated addition of yeast. M. Jos. Boussingault states, however, that by means of a process not here described, the sugar cannot be converted into alcohol within five or six hours.—*Les Mondes*.

ON THE COMPOSITION AND ANALYSIS OF WELDON MUD.

By Dr. LUNGE,

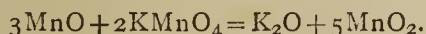
Professor at the Federal Polytechnicum, Zurich.

(Concluded from page 130.)

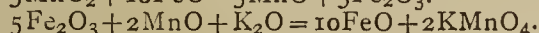
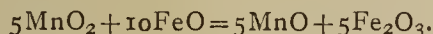
A SECOND question is the *estimation of total manganese*. The last year or two have brought us quite a number of new processes for estimating Mn, principally called into life by the importance of determining the proportion of metal in iron ores, spiegeleisen, ferro-manganese, &c. Among these I will only quote the processes of Guyard as modified by Stingl and Morawski, and later on by Volhard; of Beilstein and Jawein, and of your fellow townsman, Mr. John Pattinson—leaving out a number of others which I did not think it worth my while to try. It was the question, whether the plan proposed by Mr. Weldon himself was sufficiently exact, or whether it could be replaced by any of the others with advantage?

First of all I tried Volhard's process (of which a sufficiently detailed description is given in the CHEMICAL NEWS, vol. xl., p. 207). 5 c.c. mud was dissolved in HCl; the solution evaporated several times with nitric acid, neutralised with soda, just acidulated with nitric acid, and titrated in the boiling state with potassium permanganate, after having added 2 grms. of pure zinc sulphate. There was used 21.3 c.c. of semi-normal permanganate, from which we find the proportion of Mn = 0.1757 grm.

A second sample of 5 c.c. treated in the same way, but leaving out the ZnSO₄, required 21.4 c.c. permanganate = 0.1765 Mn. That the precipitate really contained, as required by theory, pure MnO₂, without any MnO, was proved by dissolving it in ferrous sulphate solution and re-titrating; the ferrous sulphate destroyed was found to correspond again exactly to 21.4 c.c. permanganate. The first titration (according to Volhard) is represented by the equation:—



The second by the following two equations:—



It is no matter of surprise that the process yields the same results with or without zinc sulphate, since the large quantity of calcium salt present plays the same part as the zinc salt, viz., supplying the "base," which seems required to secure the precipitation of all Mn as MnO₂. That this is possible appears from Volhard's original paper (*Annalen der Chemie*, vol. cxcviii., p. 318), and is in any case proved by the experiments just quoted.

Volhard's process is undoubtedly quite accurate, and, if the Mn is present as nitrate or sulphate, is not at all so troublesome as appears at first. The precipitate, which at first is very voluminous, later on settles down very quickly, and admits of pretty rapid titration. Still that operation (which has to be performed at nearly boiling heat) is not so easy or convenient as the iron method, and the worst feature for our purpose is the absolute necessity of destroying all chlorides, which takes a little time and trouble. I should not therefore recommend this process so much as the bleaching-powder, which is equally accurate and less troublesome.

I further tried Beilstein and Jawein's process (*Berliner Berichte*, vol. xii., p. 1528), which is principally recommended for separating Mn from other metals, and which I find to be most suitable for this purpose, when alkaline salts need not be looked for, and when there is but little iron present, both of which conditions exist in Weldon mud. Here also the chlorides must be destroyed by heating with strong nitric acid, of which there must be a large excess; then chlorate of potash is added little by little, always boiling, by which all Mn is precipitated as MnO₂, quite free from lime, &c. A portion of the clear liquid is poured off and boiled with more nitric acid and

potassium chlorate, which should not produce any further precipitate. The whole is diluted and filtered. The washed precipitate, if there be much iron present, contains iron, and must be again subjected to the former treatment; but, in case of very little iron being present, is practically pure MnO₂. Beilstein estimates the Mn by titrating the washed precipitate according to Bunsen's iodometrical method; but this would be very inconvenient, as it is rather troublesome to get the precipitate from the filter into the dissolving flask, and as some portions of it obstinately adhere to the beaker in which the precipitation has taken place. I have therefore modified this method in this way:—I wash the precipitating beaker as well as possible, leaving the adhering MnO₂, and put it aside. The precipitate is washed on the filter with boiling water, which is done with great ease and rapidity, as it is very dense, and occupies only a small portion of the space which the same quantity of MnO₂ requires when produced by Volhard's or the bleaching-powder method. It is, moreover, distinguished from these latter precipitates by its intensely black colour, and, more essentially, by its freedom from lime, &c., which I proved by direct analysis. The washed filter is bodily transferred into the precipitating beaker, and ferrous sulphate is added, which dissolves the precipitate, also that adhering to the glass, quite as quickly as any other precipitated MnO₂. The operation is ended by re-titrating with potassium permanganate.

Although the results of this process are quite accurate, and agree both among each other and with those of the other processes, I would recommend it only for the case (which does not occur in ordinary factory work) where CaO and MgO have to be estimated. In this case the bleaching-powder method is of course not applicable, neither is Volhard's method, which pre-supposes the precipitation of calcium or zinc oxide along with MnO₂. But for ordinary estimations of Mn, at any rate in Weldon mud, Beilstein's method is still too troublesome, as it involves as much boiling with nitric acid as Volhard's, and as it happens sometimes, if the boiling has not lasted long enough, that the liquid is after all blackened again by potassium chlorate, thus spoiling the test.

We thus come back to the bleaching-powder test, which was originally prescribed by Mr. Weldon. Mr. John Pattinson has shown quite lately, as you all know, that from a solution of a pure manganese salt there is precipitated by bleaching-powder, not pure MnO₂, but an oxide containing only 98 to 99 per cent of MnO₂, with a little MnO. He therefore prescribes the addition of ferric or zinc chloride, when pure MnO₂ falls down. This observation agrees completely with that made by Volhard, viz., that some other base must be present to satisfy the acid properties of MnO₂, failing which this is done by some MnO. But in our Weldon mud it is not even necessary to add the salts mentioned by Mr. Pattinson, for there is always quite a sufficient quantity of calcium chloride present to satisfy the requirements of the case; and we may assume from the outset that Mr. Weldon's prescription *as it stands* will give perfectly correct results. I have therefore directed my experiments to this line: to find out how far the process would be simplified without taking away from its accuracy.

5 c.c. of some fresh Weldon mud was dissolved over the lamp in as little HCl as possible, cooled down a little, a quantity of strong clear bleaching-powder solution was added, and then so much hot water that the volume amounted to 100 c.c. This mixture was heated a little longer (if filtered at once, it was sometimes found that the filtrate contained manganese); the precipitate was then filtered, thoroughly washed till KI gave no reaction, the filter put back into the first beaker, iron solution added, and titrated with iron and permanganate. Two experiments indicated the Mn to be equivalent to 8.4–8.4 c.c. semi-normal permanganate.

A third experiment was made with mud *without dis-*

solving it in acid. The mud was boiled a very short time with bleach solution as it was. The liquid turned strongly pink, and was therefore boiled for a minute with a few drops of nitric acid and alcohol, which decolourised it. The washed precipitate was found equal to 8.4 c.c. permanganate, like the first two.

A fourth experiment was made, adding to the mud some HCl, but not sufficient to dissolve it completely; undoubtedly the "base" would be saturated. The solution, boiled with bleach liquor, turned strongly red, and was decolourised by boiling with alcohol; but the first time the experiment miscarried, since the filtrate turned brown on adding more bleach, which proves that even a strong pink colour does not secure complete peroxidation unless the liquid is tried with more bleach, to see whether all MnO_2 is precipitated. On repeating the experiment with that precaution, the same result was obtained as before, viz., 8.4 c.c. permanganate.

Lastly, 5 c.c. was supersaturated with HCl, bleach liquor, water, and calcium carbonate added, just as prescribed by Mr. Pattinson. Result of two testings again: 8.4 c.c.

The conclusion is that in any case the same result is obtained, so that the mud need not be dissolved in acid at all, but may be boiled directly with bleach liquor. If it is proposed to dissolve it in acid, the excess of this is certainly best taken away by CaCO_3 , to avoid a great waste of bleach and evolution of chlorine. In any case the bleach process is so simple, rapid, and accurate, that I cannot hesitate to recommend it whenever it is applicable; certainly always for Weldon mud.

We now come to the estimation of the "base." Dr. Post, in the papers quoted above, altogether denies the existence of that base, and manganites generally, in Weldon mud, from the result of a number of analyses of the mud. But that part of his paper is very open to criticism indeed. He unfortunately prepared the material for his analyses by washing the mud with an enormous quantity of water, viz., by more than 100 washings with forty times its weight of water. Even then he found the washings to contain some calcium, sulphuric acid, and very little chlorine; and he concludes from this that the calcium cannot be in chemical combination with MnO_2 , whilst I for my part would just draw the opposite conclusion, viz., that there is a combination, which is gradually destroyed by the enormous mass of water. It is very probable that the water contained enough carbonic acid to convert some of the calcium into bicarbonate, and remove it as such. He found, indeed, 2.3 per cent of CO_2 in the partly-dried residue, and assumes that the CaO and MgO found by him are simply carbonates, produced by the carbonic acid present in the air blown through the oxidiser. But even according to his own analyses (which do not agree very closely among each other), the CO_2 present would not account for much more than one-third of the CaO and MgO, for which he adduces no valid reason whatever; and I cannot in any case acknowledge that he was dealing with real Weldon mud, but only with a product obtained from this, but modified by treatment with more than 4000 times its weight of water, whose gaseous and other impurities, let them be ever so small, would be very efficient owing to its enormous quantity. It is not very wonderful, accordingly, that the compound or mixture which he ultimately subjected to analysis, after having given up the attempt to reduce it to a definite state of dryness, &c., should yield somewhat less than the base required by Weldon's formula, RO_2MnO_2 . It would be infinitely more wonderful if the thousands and thousands of testings made at the works, which always yield more than the above base, were altogether wrong. The only, although very remote, possibility of this would exist, if the testing method for base, generally employed at alkali works from Weldon's prescription, were entirely faulty, and to investigate this has been the last portion of my present task.

The Weldon mud which I employed for my analysis

came from a small factory where they used a good deal of native manganese as well, and where they had had some trouble with the "blows," so that the base was rather high, and the proportion of MnO_2 to total Mn not very favourable, as appears from the analysis. This would not, however, influence the testing method. In order to see whether the usual test for base is correct or not, there was nothing left but to make a complete analysis of the mud. Warned by Dr. Post's want of success I preferred (as I should have done even without that stimulus) to analyse the mud *in toto* as it came to me from the oxidiser. By estimating all its constituents, leaving the water aside as immaterial, we must arrive at the *proportion* between MnO_2 and bases, and that is all we require in this case.

The mud was the same as used for the last estimations of total manganese; tested in the usual way it showed 0.0797 Mn to be present as MnO_2 , and 0.1155 Mn altogether. The testing for base was performed on principle according to Mr. Weldon's instructions, but with a few modifications in detail. The mud, after warming it with oxalic acid, was not at once re-titrated with caustic potash or soda, because the final reaction with litmus is made somewhat uncertain by the precipitate suspended in the liquid, but the latter was filtered, the precipitate washed with hot water (which is done very quickly), and the filtrate titrated with semi-normal ammonia, which seems to answer better than potash; the latter sometimes causes a coloured precipitate, and gives uncertain results. Three tests gave exactly concordant results, from which the base could be calculated = 0.828.

The analysis of the whole mud yielded the following results for 5 c.c. :—

$\text{MnO}_2 = 0.1261$; corresponds to 2×0.0232 oxygen.

$\text{MnO} = 0.0462 = 0.0104$ basic oxygen.

$\text{CaO} = 0.4326 = 0.1236$ " "

$\text{MgO} = 0.0307 = 0.0123$ " "

$\text{Fe}_2\text{O}_3 = 0.0025 = 0.0007$ " "

0.1470

$\text{Cl} = 0.5412 = 0.1245$ " "

$\text{CO}_2 = 0.0070 = 0.0025$ " "

$\text{SO}_3 = 0.0022 = 0.0004$ " "

0.1274 " "

This leaves a surplus of 0.0196 basic oxygen, and comparing this with the above figure of 0.0232, we find the ratio in Weldon's parlance = 0.845 base. This agrees so far with the figure found by Weldon's own process, viz., 0.828, that we may assume the latter to give quite near enough results for practical requirements, although hardly absolutely correct for real chemical analysis. We must not forget that Weldon's process neglects the presence of calcium carbonate, whose proportion in Weldon mud is certainly very slight. In no case can Dr. Post's conclusion be confirmed, for whether the oxides forming the base "are present in chemical combination with MnO_2 as manganites," or not, there is at any rate enough of them present for that purpose, which is precisely what Dr. Post denies, but without sufficient reason.

Whether "manganites" exist or not is quite another question, which cannot be solved by mere analysis, and which presents great difficulties. Some weighty reasons speak for the assumption that MnO_2 itself is a compound of MnO with manganic or permanganic acids, viz., either MnO, MnO_3 , or $3\text{MnO}, \text{Mn}_2\text{O}_7$. In that case the "manganites" would be basic manganates or permanganates; Weldon's "acid calcium manganates," $\text{CaO}, 2\text{MnO}_2$, would be, say, $\text{CaO}, \text{MnO}, \text{MnO}_3$, &c. I must refrain from entering into this speculative domain at further length.

I would rather conclude with a practical remark which may be new to some of the members of the Society. It has been found at the Aussig Chemical Works (Mr. Schaffner's) that it is far from indifferent what proportion of calcium chloride is present in a manganese liquor intended to be treated by the Weldon process. Unless

there are at least $2\frac{1}{2}$ equivalents of CaCl_2 present to each equivalent of MnCl_2 , the blows are apt to miscarry, and especially thick batches occur more easily. Accordingly at those works the liquors are regularly tested for Mn as well as for Cl, by which means the above proportion is easily found. A short time ago I was consulted by the chemist at an alkali works, where they could not get up proper batches, all of them turning stiff. At first I advised better settling, more final liquor, less heat, more blowing, &c., but nothing did any good, till I thought of that Aussig observation, which I communicated to my correspondent. A few days later I got a most grateful letter, to say that they had taken my advice and that all their troubles were over now. This may be useful to some of our friends on Tyneside as well.

P.S.—I have just received a copy of a more extended publication by Dr. Post on the above subject. From this it appears that he employed a plan for separating manganese and lime, which has long since been shown to be faulty, viz., precipitating the former by bromine in an acetic acid solution. He now quotes one analysis of unwashed Weldon mud; but this (although he does not mention it) does not confirm his own conclusions, since the base can be calculated from it to amount to 0.61.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 18, 1880.

Mr. WARREN DE LA RUE, President, in the Chair.

THE following certificates were read for the first time:—J. M. Dennis, R. Grimwood, W. F. Haydon, F. B. Last, H. Liepmann, A. J. Smith, J. W. Stanley.

Prof. TIDY was then called upon to read his paper on "*River Water*." The author discusses the subject under three heads:—1. Analytical Details of River Waters. 2. The Various Sources of Impurity to which River Water is Subject, and the Means whereby Purity is maintained by Nature, or may be effected by Art. 3. The extent to which Statistics warrant us in condemning or approving the Supply of River Water for Drinking Purposes.

1. *Analytical Details of River Waters*.—River Thames:—The author gives a table showing the composition of the Thames water as supplied to the metropolis from January, 1876, to December, 1879. The details given are—Total solid matter; oxygen required to organise the organic matter; organic carbon and nitrogen, as determined by Dr. Frankland; nitrogen as nitrates and nitrites; lime, magnesia, chlorine, sulphuric anhydride; hardness before and after boiling. The solid matter is at its highest in February, 21.63 grains per gallon, descending regularly to August, 18.24 grains per gallon, whence it again rises. Boiling reduces the hardness to less than one-third. From September to January the chlorine is under 1 grain per gallon; from February to August it is over 1 grain per gallon. The quantity of solid matter is no gauge as to its organic purity, as judged by the oxygen required. The testimony of the results obtained with the oxygen process agree remarkably with those obtained by the combustion process, as has already been pointed out by the author, the same story being thus told by two absolutely independent experimental witnesses. The author regrets the frequency with which cases occur where conclusions, of great importance sanitarily, are based on experimental determinations, as incapable of arrangement or of interpretation as they are outside the sphere of scientific accuracy. The nitrates or nitrites do not accord with the results indicating the organic matter; it may be that the organic nitrogen of one month becomes the organic nitrogen of the next. The author has estimated the gases dissolved in filtered

Thames water on twenty-seven different occasions. The oxygen in solution during the winter months (2.19 cubic inches per gallon) was found to be very nearly double the amount held in solution during the summer months (1.19). The author intends to turn his attention to a further examination of this part of the subject. Some analyses of unfiltered Thames water from Hampton are then given. River Lea:—Monthly analyses (1877 to 1879 inclusive) of the water from this river are given. The total solid matter is again highest in February, 23.34 grains per gallon, sinking to a minimum in September, 17.99 grains per gallon. The chlorine is slightly higher than in the Thames. As far as organic matter is concerned this river is slightly purer than the Thames. The results of the oxygen and combustion processes again exhibit a remarkable correspondence. River Nile:—Monthly analyses of this river, taken during one year at Cairo, are given. The total solid matter ranged from 9.5 to 14.3 grains per gallon; the chlorine from 0.145 to 1.395; the oxygen required from 0.08 to 0.27. All the samples but that of May were thick. During April, May, and June, when the Nile is lowest, the water is worst. As regards total solid matter, 12.73 to 14.33; chlorine, 0.64 to 1.39, &c.; the month of July is the transition month, the river rapidly rising; during September and October the condition of maximum flood persists; during the fall the river attains its maximum purity. The Severn:—Analyses of samples of water collected from the Severn at various spots, in January, 1878, from Worcester above the sewage outfall down to Tewkesbury, a distance of 16 miles, together with analyses of the River Teme, are given. The Severn water contains about 16 grains of solid matter per gallon, the chlorine being double the amount of that present in the Thames. The quantity of chlorine varies remarkably at different spots, probably from the influx of brine springs from the New Red Sandstone. The Shannon:—The samples were collected in October, 1879, the river being about 2 feet above summer level. This river was selected because it drains a larger area than any single river of the British islands; because, though uncontaminated with animal impurity, it contains a large quantity of dissolved peaty matter; and because it is a type of the Irish rivers which so often pass through lakes in their course to the sea. The analyses include samples taken at the extreme ends of one of the lakes, Loch Derg. The solid matter varied from 15.3 to 20.2 grains per gallon; there was but a trace of nitrogen as nitric acid; the chlorine was remarkably constant, about 1.0 grain per gallon.

2. *The Various Sources of Impurity to which River Water is Subject, and the Means whereby Purity is maintained by Nature or may be effected by Art*.—The pollution produced by trade refuse is not considered in the present paper. Flood Water:—During the first few days of a flood the drainage called by the author "primary flood water" materially affects the river for the worse, as all the soluble and some of the insoluble matters accumulated in the soil during the period when the rainfall was insignificant pass into the river. The river becomes heavy in total solid matter, excessive in dissolved organic matter, and turbid. The velocity of the river is increased, and carries down accumulations, organic and mineral, from the higher reaches of the river. The author then gives analyses of the Thames water before and after two heavy floods (November 18th, 1875, and January 11th, 1877); the rise in the total solid matter is not so great as was expected; the rise in the organic matter was well marked, the high flood water requiring 0.173 grain of oxygen per gallon against 0.064 grain ten days before; further analyses of ordinary floods are given, which show similar results. Primary flood water is therefore inferior to normal river water as regards organic purity and general clearness. By "secondary flood water" the author implies the water that comes down after some considerable period of flood, when the drainage area has been washed clean, the river passing slowly from a state of maximum impurity to one of maximum purity; the total solid matter of a secondary

flood water being only 17·28 grains against 22·84 grains in primary flood water, the oxygen required being reduced to about a sixth, the hardness by 3 degrees, whilst the suspended matters are too small to be capable of estimation. Although the Thames when at or below summer level is brighter and clearer than at any other time, the water is not of such good quality as during the period succeeding a flood. The author then discusses the influence of floods as exhibited in the River Nile, and shown in the analyses already referred to; the influence of the geological nature of the drainage area; of the time of year at which the floods occur; of the interval between one flood and the next; and of the exact time occupied by a given rainfall on the flood water carried into the river. He then takes up the subject of peat. Peat exists for the water analyst as old peat or recent peat, the latter giving a yellowish green, the former a coffee-brown tint, the colour passing through all gradations according to the age and quantity of peat present. The author then insisted on the value of the "two-foot tube," *i.e.*, a tube 2 feet long, 2 inches in diameter, with white glass ends, as a most valuable adjunct to analysis: thus 0·4 per 100,000 of organic carbon would have a very different significance to the chemist if the tint exhibited by a large bulk of the water was brown, indicating vegetable matter, to what it would if the water was colourless. Prof. Tidy has never known in practice a case where the water of a river became colourless simply through the bleaching of peat, and thinks that it is quite possible to compare the depths of colour in two waters, though the tints are different. The oxidation of peat has been carefully studied by the author; in the Shannon he has analysed the water at various places, the most notable instance of oxidation being at the falls of Castle Connell; a short distance above, the organic carbon being 0·84 part per 100,000; a short distance below, 0·593 per 100,000. Another instance of this diminution of carbon was noticed in the North of England, where the water is peaty, and is delivered from a storage reservoir to the supply reservoir, 6820 yards distant, by two channels,—one a closed iron pipe, the other an open brick conduit. The water delivered by the iron pipe has distinctly a peaty tint; the same water delivered from the conduit is clear and nearly colourless. The water in the storage reservoir contained 0·46 organic carbon per 100,000; that from the iron pipe, 0·49; that from the open conduit, 0·23. This, says the author, is a perfect experimental proof of the oxidation of peat. The quantity of peat can also be considerably reduced by admixture with water containing much suspended mineral matter, which causes a precipitation of the peat. Thus the entrance of the turbid River Mulcaire brings down the organic carbon in the Shannon from 0·914 to 0·61. This action was also confirmed by shaking up a peaty water with turbid Mulcaire water in the laboratory. Thus the quantity of peat in running water is kept in check—(1) by the inherent power which water possesses of self-purification, owing to the oxidation of the peat by the oxygen held in solution in the water, the process being enormously helped by certain natural and physical conditions, whereby the more complete aëration of the water and the intimate contact between the oxygen and peat is effected; (2) by mechanical precipitation, by admixture with coarse mineral suspended matter. From peat the author passes to animal impurity: he agrees entirely with Dr. Frankland as to the vital importance of this subject. No one, says Prof. Tidy, should be required to drink his own excreta, much less the excreta of his neighbours. The two important questions are—Does the water of a river, having been once polluted by admixture with sewage, again, after a moderate flow, assume its original state of purity? And, secondly, If such a natural process of self-purification exists, what are the conditions requisite for its fulfilment, and what are the circumstances affecting these conditions? The author answers the first question in the affirmative, and considers that this self-purification depends upon three things:—(a) the subsidence of the

coarser suspended impurities; (b) the presence of animal life after the water has assumed a certain degree of purity; (c) the oxidation of the organic matter, the dissolved oxygen being derived partly from the air and partly from plant life. The rapidity of this oxidation depends on the degree of dilution of the sewage, the distance of run, the velocity of the current, the temperature, and certain natural or artificial physical conditions, such as the mixing of the water and air at locks, weirs, &c. The author then proceeds to enumerate the proofs of this oxidation. From analogy we should expect water to purify itself, as the air, which is constantly receiving masses of organic *débris*, eliminates, by oxidation, &c., its sewage matter. Are men never to drink water because it has once been contaminated by sewage? As well might we say that air which has once received scales from a person suffering from scarlet fever, or the infected breath of a measly patient, must never be breathed again. If oxidation purifies in the one case, may not oxidation be expected to act in the other? The proofs of this oxidation are threefold:—(A.) The naked-eye inspection of rivers receiving sewage. The sewage at the outfall is probably offensive; as it passes down the river it blackens from the formation of sulphide of iron; farther on this black colour decreases, disappears, and the river at last exhibits no signs of odour, colour, or turbidity. Again, soon after the sewage has been turned into the river, the sewage fungus appears, all other vegetable life being absent, no fish are seen, the banks are black; a short distance farther on the fungus vanishes, vegetation is luxuriant, fish abound, the river clears, and no trace of black deposit can be seen. Thus the River Soar, at Leicester, is black with sewage; at Loughborough, 13 miles distant, the river is perfectly clear, and fish are abundant; the Irwell, at Manchester, is polluted with every form of filth—in 9 miles the offensive character of the stream has entirely disappeared; and so on in many more cases. Such examples prove the power of self-purification possessed by rivers. (B.) Analysis. The Thames at Lechlade may be taken as pure river water; after flowing a distance of 110 miles, and receiving the sewage of many towns, &c., the water at Hampton contains rather less organic carbon and nitrogen than at Lechlade. What has become of this sewage if it has not been oxidised? The same is true of many other rivers—the Severn, the Wear, &c. The author then refers to the diametrically opposite conclusions in the Sixth Report of the Rivers' Commission, in which Dr. Frankland states that organic matter is not appreciably oxidised by the flow of a few miles. Prof. Tidy does not for one moment question the accuracy of the results, but strongly dissents from the conclusions, and submits that analyses indicating a *reduction* of organic carbon and nitrogen between two spots are of much greater value than any analyses indicating the reverse; because Dr. Frankland cannot say that the organic matter found at B, where he collected his second sample, is the same as that found at A, where the first sample was taken, and between the two spots, A and B, a fresh quantity of organic matter may have been introduced. Thus, to take an illustration—Six passengers enter a carriage at station No. 1; at station No. 3, six passengers are still in the carriage, but unless there is evidence to show that they are the *same* six passengers there is nothing to prove that the whole of the six passengers who got in at station 1 did not get out at station 2 and six fresh passengers get in. But if, at station 3, only two passengers are found, it is absolutely certain that at least four must have got out at station 2. The author then criticises experiments in which Dr. Frankland shook up a mixture of water with 10 per cent of sewage, and thus imitated the flow of a polluted stream for 96 and 192 miles. The author contends that these experiments did not imitate at all accurately the flow of a stream, and the results obtained are therefore valueless. The author set up an apparatus which did to some extent represent the action of a running stream. It consisted essentially of a series of about 20 V-shaped wooden

troughs lined with glass: the troughs were placed one over the other, and were inclined alternately in opposite directions, so that the water flowed from a cistern into the upper trough, and thence gradually down the 20 troughs into a cistern beneath, from which it was pumped up into the higher cistern. After many difficulties, from the smoky atmosphere in London, the author, with the assistance of Mr. Hart, obtained results with various mixtures of New River water with sewage from Abbey Mills, which proved satisfactorily a decided decrease of organic matter after a run of 1 mile. After 2 or 3 runs the offensive sewage smell disappeared; the last traces of sewage matter seem to be the most difficult to oxidise. From these considerations the author concludes by thus paraphrasing a well-known passage in the "Sixth Report of the Rivers' Commission," p. 138. "I am led to the inevitable conclusion that the oxidation of the organic matter in sewage, when mixed with unpolluted water and allowed a certain flow, proceeds with extreme rapidity, and that it is impossible to say how short a distance such a mixture need flow under favourable conditions before the sewage matter becomes thoroughly oxidised. It is certain to my mind that there is no river in the United Kingdom but what is many times longer than is required to effect the destruction of sewage by oxidation." Some of the methods for purifying water are then considered. A water which is impure from the presence of sewage should be unhesitatingly rejected. In urging the claims of rivers as one of the sources for water supply, the author means always rivers free from sewage, *i.e.*, rivers, the purity of which at the suggested intake can be proved by chemical analysis. Having such a water, however, much may be done by art to improve it by storage, subsidence, and filtration: these methods are considered in detail. Pure sand of uniform medium fineness is recommended as the best filtering medium on the large scale; the rate of filtration should be 2 gallons per hour per square foot; the filter bed should be changed about every two years.

3. *How far do Statistics warrant us in Condemning or in Approving the Supply of River Water for Drinking Purposes?*—In the opinion of the author, peaty waters have not been proved to produce diarrhoea. As regards animal pollution the author admits that disease has been produced by drinking impure and polluted water; but the argument against the use of a river water is based (1) on the existence of germs; (2) on the supposition that the oxidation process which might affect organic matter would probably leave organised bodies untouched. Germs would probably be destroyed by oxidation and endomose after a run of 10 or 12 miles in a river. If they are not destroyed as organic matter they ought, as organised bodies, to increase; and as we pass down the river each town should exhibit an increasing death rate from the multiplication of the disease germs. Statistics, however, show a very different state of things. A disease usually spreads up, not down a river. From an examination of numerous statistics it is seen that the death rate of towns in which the water is obtained from wells is practically identical with that in towns supplied by rivers, and that in London, as regards mortality, there is very little to choose between districts supplied by well water and those supplied by river water; so that it is clear that the *materies morbi* is subject to the same laws of destruction as ordinary organic matter. As a matter of sentiment the author would prefer well water. If all the mischief be laid to an undiscoverable germ a terrible doubt is cast on the value of all chemical analysis. There is no well-authenticated case where river-water, having received sewage, the dilution being considerable, and circumstances favourable for oxidation, has, after a flow of 10 to 12 miles, been manifestly the cause of an outbreak of disease. In all well-proved cases of outbreaks of disease resulting from the use of drinking-water, such water would have been unhesitatingly condemned on analysis by the chemist. The author protests most strongly against the startling statement in the

"Report of the Rivers' Commission," "That the supply of deep well water would confer on the metropolis an *absolute immunity* from epidemics of cholera." Regarding all the above facts, the author submits the two conclusions:—

- (1.) That when sewage is discharged into running water, provided that the dilution of that sewage with pure water be sufficient, the whole of the organic impurity will be got rid of after the run of a few miles.
- (2.) That facts indicate that, whatever may be the actual cause of certain diseases, the *materies morbi* that finds its way into the river at the sewage outfall is destroyed along with the organic impurity after a certain flow.

The above paper (of about 130 pages) contains a large number of tables of analyses, &c.: it was listened to with great interest by a crowded meeting of the Society.

The PRESIDENT said that the large assemblage that evening evinced the great interest taken by chemists in this vital question of a suitable water supply. This interest had been increased by the forcible yet courteous way in which Prof. Tidy had supported his views. He would call on Dr. Frankland for any remarks he would like to make.

Dr. FRANKLAND said that at that late hour it was quite impossible to do justice to the discussion of this important paper. He did not know any subject of greater moment to the health of towns. He had listened to many statements founded on opinions, surmises, and casual observations, but they had made but little impression, as they lacked the quantitative element: the present paper differed entirely from such statements. Prof. Tidy had furnished us with a large number of facts which deserved serious consideration. As far as he could see there was no fundamental difference between the facts of Prof. Tidy and previous results of his own, but considerable explanation was necessary to understand how such diverse opinions could be drawn from identical facts. He would therefore propose that the discussion be deferred until after the paper had been printed.

The discussion was then adjourned *sine die*.

The cordial thanks of the meeting were given to Prof. Tidy for his paper, and the Society adjourned to April 1st, when the following papers will be read:—"On β -Orcinol," by Dr. Stenhouse and Mr. Groves; "Preliminary Note on the Action of the New Diastase, Eurotin, on Starch," by R. W. Atkinson; "Note on the Products of the Combustion of Coal-gas," by L. T. Wright; "On Polysulphide of Sodium," by H. C. Jones; "On the Reflection from Copper, and on the Colorimetric Estimation of Copper," by T. Bayley; "On Pyren," by Watson Smith and G. W. Davies; "Analyses of the Ash of the Wood of Two Varieties of the Eucalyptus," by Watson Smith.

The Anniversary Meeting takes place on Tuesday, March 30.

CORRESPONDENCE.

THE DIRECT METHOD FOR TESTING VITRIOL CHAMBER EXITS.

To the Editor of the Chemical News.

SIR,—I wish to say that on making enquiries I have satisfied myself that Mr. G. E. Davis had originated and used the method of testing the N compounds in vitriol chamber exits by the use of the nitrometer as early as September, 1878, so that I was mistaken in thinking that I used it for the first time in May, 1879.—I am, &c.,

T. E. VASEY.

Church Street Chemical Works,
Leeds, March 9, 1880.

THE DIRECT METHOD FOR TESTING
VITRIOL CHAMBER EXITS.*To the Editor of the Chemical News.*

SIR,—In my letter (CHEMICAL NEWS, vol. xli., p. 69) I did not wish to imply that Mr. Vasey had *intentionally* appropriated the method of testing vitriol chamber exit gases for nitrogen compounds by the use of my nitrometer. All I wished to do was to inform your readers that I had used and recommended it as a correct and ready method long previously.—I am, &c.,

GEORGE E. DAVIS.

PS.—In my letter (*ibid.*) for “after acidifying and evaporating,” read “after evaporating and acidifying.”

CHEMICAL NOTICES FROM FOREIGN
SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 18.

The Occurrence of Para-leucanilin in the Magenta Melt.—C. Graebe.—The author suggests that para-leucanilin is a direct product of the action of arsenic acid upon a mixture of aniline and toluidin, and that the colouring-matters may perhaps be formed by its oxidation.

Synthesis of Phenyl-naphthyl-carbazol.—C. Graebe and W. Knecht.—The authors had previously suggested that a body found in crude anthracen was to be regarded as a carbazol of the composition $C_{16}H_{11}N$, possibly an imido-phenyl-naphthalin. Their supposition has been synthetically confirmed.

Relative Replaceability of Bromine in the Monobrom-benzyl-bromides.—C. Loring Jackson.—The bromine is most readily replaced in the para compounds, least in the ortho, the meta compounds taking an intermediate position.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.

No. 72, December, 1879.

This issue contains no chemical matter, save a paper by M. Peligot on the properties of glucose. It has been noticed under the *Comptes Rendus*.

Verhandlungen des Vereins zur Beforderung des Gewerbflusses. No. 1, January, 1880.

This issue is exclusively taken up with mechanical subjects.

Annales de la Société des Sciences Industrielles de Lyon.
No. 3, 1879.

This issue, again, consists exclusively of mechanical matter.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 7, February 12, 1880.

The chemical matter in this number has been already noticed.

No. 8, February 19, 1880.

The Phenomena of Vision.—Prof. H. H. Dor.—The author combats the theory that our ancestors were colour blind.

The Thermo-electric Battery of M. C. Clamond.—This paper cannot be reproduced without the accompanying illustrations.

No. 9, February 26, 1880.

Carbonic Oxide in the Products of the Combustion of Coal-gas.—M. Gréhaut.—The small quantity of carbonic oxide evolved can scarcely be detected by its effects on animals compelled to inhale the products of combustion.

Temperature of the Sun.—M. Rossetti.—The author contends that the temperature of the sun does not fall below 20,000°.

Summary of Recent Progress in Chemistry.—M. Gerber.—A series of notes from papers regularly abstracted in the CHEMICAL NEWS.

No. 10, March 4, 1880.

Experimental Researches on Accidental Double Refraction.—M. Macé de Lépinay.—The author's researches have led to the observation of certain new facts, but they have neither supported nor destroyed the theoretical ideas by means of which Naumann has attempted to explain accidental double refraction.

Palm Oil.—An account of the source, the supply, and the uses of this oil.

Ericin, a Colour extracted from Poplar Wood.—This dye is said to be obtained by treating with a hot solution of alum the stems of common heath and the wood of different varieties of the poplar.

Reimann's Färber Zeitung,
Nos. 5, 6, and 7, 1880.

These numbers contain nothing of general interest.

NOTES AND QUERIES.

Reactions of Soluble Glass.—Would you kindly inform me what reaction takes place when pounded dolomite is mixed up with silicate of potash? I chiefly want to know whether the magnesia forms a chemical compound with the potash, which, I presume, is liberated by the carbonate of lime, or is the potash left free to come out as an efflorescence? Can you refer me to any work giving a clear account of the action of water-glass when combined with such materials as are usually mixed with it? I am told that magnesia forms an insoluble compound with water-glass, but does it do so in the same way as chloride of calcium, which, I understand, forms silicate of lime, but by so doing leaves the potash free?—L.

MEETINGS FOR THE WEEK.

TUESDAY, 30th.—Civil Engineers, 8.
Chemical, 8. (Anniversary.)

THURSDAY, April 1st.—Chemical, 8. Dr. Stenhouse and C. E. Groves
“On β -Orcinol.” Mr. R. W. Atkinson,
“Preliminary Note on the Action of the
New Diastase, Eurotin, on Starch.” Mr.
Lewis T. Wright, “Note on the Products of
Combustion of Coal Gas.” Mr. H. Chapman
Jones, “On Polysulphide of Sodium.” Mr.
Thomas Bayley “On the Reflection from
Copper and on the Colorimetric Estimation
of Copper.” Mr. Watson Smith and George
W. Davies, “On Pyrene.” Mr. Watson
Smith, “Analysis of the Ash of the Wood of
two varieties of the Eucalyptus.”

FRIDAY, 2nd.—Geologists' Association, 8.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1062.

ON THE DENSITY OF BROMINE VAPOUR AT A YELLOW HEAT.

By V. MEYER and H. ZÜBLIN.

THE density of bromine vapour at a yellow heat has been examined according to our air-expulsion process, both for the nascent halogen and for its free state. The experiments with nascent bromine were readily successful, but with the free halogen, its great volatility occasioned difficulty in its introduction into the apparatus, which, in spite of many attempts, have not been overcome.

For the experiments with nascent bromine chemically pure platinum bromide was used, and the experiments were conducted in the manner already described. The temperature was the highest which can be produced with the Perrot furnace, about 1570°. The densities found were 3.78, 3.64, the value calculated for $\frac{3}{2}\text{Br}_2$ being 3.64. The theoretical density for Br_2 is 5.52. Hence, under these conditions, bromine displays exactly the same phenomenon of dissociation as have been observed with chlorine and iodine; its vapour-density being reduced one-third at a yellow heat. For the experiments with free bromine the authors employed a preparation purified by being rectified over potassium bromide. The determination of its vapour-density, conducted in watery vapour by the air-expulsion process, gave 5.38 as against the calculated value for Br_2 , 5.52.

On attempting to repeat the determination at a yellow heat a peculiar difficulty was encountered. The bromine was weighed in thin glass globules, which were closed before the blowpipe and then thrown into the ignited apparatus. The experiment failed each time, because the globule exploded on entering the apparatus with such speed that the bromine was projected out of the vessel. This evil could not be overcome even by the use of stronger globules, and the authors only succeeded in avoiding the loss of the bromine by inclosing it in short strong capillary tubes of very infusible glass. Even so the evaporation took an explosive form, and there was no certainty that a part of the contents was not flung into the colder neck of the apparatus. The numbers obtained agree very imperfectly—4.30, 4.44, 3.94, 3.99, 3.78, 4.20, 4.14, and lie between the value calculated for Br_2 (5.52) and that for $\frac{3}{2}\text{Br}_2$ (3.64). M. Crafts, who experimented upon free bromine (not nascent), arrived at a similar result. At the highest temperature of the Perrot furnace, he found the vapour-density of bromine 4.39 and 4.48.

The inconvenience above mentioned has made it hitherto impossible to determine the vapour-densities of very volatile bodies, such as free bromine, water, &c., at a yellow heat. It may be probably overcome by the use of much larger receptacles than stood at the disposal of the authors who will continue their experiments upon bromine with platinum bromide only. The results are still imperfect, as bromine has been hitherto examined at very high temperatures only.—*Berichte der Deutschen Chemischen Gesellschaft.*

Certain Chemical Constants.—J. L. Janovsky.—The author contends that the valence of an atom or molecule is its greatest possible capacity of saturation, when it of course becomes a constant. He seeks to prove electro-chemically, that the equivalent is a constant.—*Chemisches Central-blatt.*

ON THE BEHAVIOUR OF IODINE AT ELEVATED TEMPERATURES.

By V. MEYER.

In the earlier memoir of V. and C. Meyer, on the decrease of the density of chlorine at elevated temperatures to two-thirds of its normal value, it was mentioned that the behaviour of iodine was analogous. The author reports on the experiments which lead to this conclusion. The iodine employed was prepared from the purest commercial iodine by sublimation over iodide. It was kept in a closed bottle over sulphuric acid. The author assured himself of its perfect purity by titration with the purest arsenious acid, repeatedly sublimed.

With this preparation determinations were carried out at different degrees of heat. In order to know approximately the temperatures they were ascertained calorimetrically, as in the former experiments. The experiments took place (A) in a glass vessel (253°), in the vapour of amyl benzoate. B, in a glass vessel in the lead-bath, about the melting-point of sulphur. C, D, E, F, in porcelain vessels in a Perrot furnace, the temperatures being respectively about 586°, 842°, 1027°, and 1567°.

The experiments gave a result similar to that obtained with chlorine. Up to about 600° the density of the iodine vapour agreed exactly with the formula I_2 . At 800° a strong decrease was perceptible, and from 1027° to about 1567°—that is, through a range of more than 500°—it remained unchangeably and exactly equal to the value calculated for $\frac{3}{2}\text{I}_2$. Thus in experiment A the experimental values obtained were 8.89 and 8.83 as against the calculated value 8.78.

In B, 8.84 and 8.85.

In C, 8.73, 8.71, and 8.71.

In D, where the temperature was 842°, the experimental values fell to 6.68, 6.80, 6.80.

In E, at 1027°, the results were 5.75, 5.74, as against the value 5.83 calculated for $\frac{3}{2}\text{I}_2$; and

In F, at 1567°, the experimental values were 5.67, 5.60, 5.71, 5.81.

These data give the following view of the behaviour of iodine at high temperatures:—

Temperature.	Vapour-Density Found.	Theory.
253°	8.89 .. 8.83	For I_2 : 8.78
450	8.84 .. 8.85	
586	8.73; 8.71; 8.71	
842	6.68; 6.80; 6.80	
1027	5.75; 5.74	
1570	5.67; 5.60; 5.71; 5.81	For $\frac{3}{2}\text{I}_2$, 5.83

The density curve of iodine vapour differs from that of chlorine by undergoing its decrease of density much earlier than the latter. Whilst chlorine was only fully converted to $\frac{3}{2}\text{Cl}_2$ about 1200°, in iodine the results obtained at 1000° agree closely with the value calculated for $\frac{3}{2}\text{I}_2$, and preserve this agreement unaltered as far as the highest temperature hitherto found applicable.

The results just communicated deviate in some points from those arrived at by MM. Deville and Troost in their admirable researches on vapour densities. This applies especially to the observations made about 1000°. Whilst at this temperature the author obtained closely the figures calculated for $\frac{3}{2}\text{I}_2$, the result of a determination of the boiling-point of zinc made by MM. Deville and Troost with an iodine thermometer, and therefore assuming the unchanging density of iodine vapour, leads to the number 1040°, i.e., almost exactly to the same result as they obtained by means of an air-thermometer. How this contradiction is to be explained the author is not yet able to say, but will return to it in a subsequent memoir. He felt, however, bound to use the greatest precautions against any error made in the experiments about 1000°. Half a year therefore, after the experiments mentioned above a further

determination was made at about the same temperature, with completely new apparatus, and with iodine freshly prepared and purified. The result agreed exactly with that formerly obtained, the experimental density found being 5.88. He also repeated the determination of the vapour-density of mercury as a check on the applicability of his process at such temperatures. The result was 6.98 as against 6.91 by calculation.

Determination of the Vapour-Density of Iodine in Platinum Vessels.—The previous experiments for determining the vapour-density of the halogens were made in vessels of porcelain, the fitness of which for the purpose had been ascertained by special experiments. It seemed, however, desirable, in order to obviate possible objections, to repeat the experiments in vessels of a quite different material. The author selected, therefore, the platinum apparatus and the process as modified accordingly, described *Berichte*, xii., p. 2204. It was not possible to operate with chlorine, as according to the observations of Troost, Hautefeuille, Seelheim, and the author, it attacks platinum at a yellow heat: but with iodine the experiment could be conducted without difficulty. The results arrived at were 5.71, 5.81 (calculation for $\frac{1}{2}I_2 = 5.83$), and agree completely with those obtained in porcelain vessels. In these experiments the iodine was weighed in a small platinum jug, and its vapour only came in contact with platinum and nitrogen, as it was not necessary to cover the bottom with sand.—*Berichte der Deutschen Chemischen Gesellschaft*.

THE ESTIMATION OF CARBON IN STEEL.

By JAMES W. WESTMORELAND,
Associate of the Royal School of Mines, London.

In the *CHEMICAL NEWS*, vol. xl., p. 225, M. Sergius Kern, in a short note, impugns the accuracy of the colour test for the estimation of combined carbon in steel, and at page 263 of the same volume Mr. W. D. Hermann, in a letter advocating the direct combustion process, states the general opinion of chemists that the colour test is not accurate.

The objections to the direct combustion process are—that the steel is required in a fine state of division, and the uncertainty as to the completeness of the reaction, while the time required by any combustion process would prevent its regular use in steel works, where it is not unusual to test from thirty to fifty samples daily.

Having used the colour test extensively for some years, I find that in most cases the results obtained by it agree well with those obtained by other processes,—that concordant results are obtained by analysts working independently on the same sample, and that discrepancies which arise are due more to errors or inexperience than to any defect in the method.

In order to compare the processes I was requested to examine by colour test a series of samples of steel in which the carbon had been estimated by combustion (between sixty and seventy samples were examined) the greater number of the experiments being made in the year 1876, with others at intervals up to the present time.

The following are examples of the results obtained in 1876:—

Carbon Per Cent.			
	A.	B.	C.
1.	0.285	0.280	0.30
2.	0.349	0.340	0.36
3.	0.158	0.152	0.18
4.	0.565	0.532	0.48
5.	0.147	0.122	0.16

The results in column A were obtained by combustion, the steel being dissolved in a solution of chloride of copper, &c., the separated carbon being finally burnt with oxide of

copper in a stream of oxygen gas: this process, although tedious, is very accurate. The results in column B were obtained by myself, using the colour test, in the Bowling laboratory. The results in column C were obtained by the colour test in the laboratory of a South Wales steel works.

The following experiments were made in May, 1878:—

Carbon Per Cent.		
	Combustion.	Colour Test.
6.	0.355	0.35
7.	0.515	0.50
8.	0.189	0.19
9.	0.29	0.27
10.	{ 0.415 0.41	0.42
11.	0.05	under 0.10
12.	0.622	0.67 0.647

The combustions were made as before, and the colour estimations by myself, then engaged as chemist to the Rhymney Iron and Steel Company.

The sample No. 11 shows one of the defects of the colour test—that it cannot be used for estimating very low percentages of carbon in steel, and, on the other hand, its indications with high percentages are not very exact. For steel works' purposes, however, where estimations ranging from 0.10 to 1.00 per cent combined carbon are required in large numbers and quickly, it is invaluable, taking but little time, and giving results agreeing well with those obtained by other methods.

Bowling Iron and Steel Works,
February 28, 1880.

BORACIC ACID AS A PRESERVATIVE.

By H. ENDEMANN.

BORACIC acid has, for the last ten years, held a place amongst our antiseptics, and has frequently been recommended for the preservation of meats and vegetable substances. The original discoverer, Gahn, sold in Europe two mixtures.

The one was a mixture of one part of boracic acid with one part of alum; the other, one part of boracic acid with two parts of alum. These were called respectively "aseptine" and "double aseptine."

Provisions in part preserved by boracic acid are generally within the time of from one to two months covered with a black crust, provided they are kept in the ordinary oak provision barrels. Alum prevents this.

This observation, long ago published, I found verified in my own investigations. It was, at first, my intention not to preserve without salt, but to limit the quantity of the salt to be used by the addition of certain antiseptics. In the following, without going into the details of the numerous experiments, I shall give the results of those which are directly of interest to the question discussed in this paper.

It was found that fresh beef, packed with 1 per cent of boracic acid and a salt pickle of 50 per cent, remained sweet and wholesome for several months, even if kept at an average temperature above 80° F. It was likewise found that previously salted beef could not be preserved by the addition of boracic acid. From this it was evident that the process of salting removed from the beef certain substances, in the absence of which preservation became impossible. These substances proved, on further investigation, to be the phosphates. It was, therefore, not the boracic acid which had been the cause of the preservation, but, rather, substances which are produced by the action of the boracic acid—the acid phosphates.

I could cite a long list of materials which cannot be preserved with boracic acid, owing to the want of phosphates. In all these cases, however, preservation would

be possible if, with the boracic acid, a phosphate were to be added: a somewhat roundabout way, which can be improved, as I shall explain further on.

During my investigations regarding the disinfecting properties of various substances I had found that it is very difficult to develop bacteria in a fluid containing acid phosphates, and also that many acids are powerful disinfectants, destroying the life of bacteria completely, even if present only in small quantities. One part of HCl in 64 parts of Cohn's fluid, well stocked with bacteria, destroyed these completely.

I was led, therefore, to make experiments in which boracic acid was replaced by equivalent quantities of other inorganic acids, and thus ascertained that exactly the same results could be reached as with the use of boracic acid.

The best results were reached by the use of phosphoric acid, and mixtures of phosphoric acid and hydrochloric acid.

Phosphoric acid, even in dilute solution, acts powerfully on fresh meat, covering it with a white layer of coagulated albumen, which, however, on standing gradually disappears. Mixtures, however, where the PO_5 is partly replaced by HCl, do not act in a like manner; and, even if some precipitation should take place, will soon allow the meat to recover its original appearance.

Meats thus treated keep exceedingly well, and at least fully as well as when preserved, under similar circumstances, with an equivalent quantity of boracic acid.

Less favourable results were obtained by the use of sulphuric, nitric, and acetic acids, which is easily explained by the instability of these acids.

The preservation by means of boracic acid cannot, therefore, be considered as involving a new principle; it is merely a variation of, but by no means an improvement on, the time-honoured vinegar pickling. The insipid taste of free boracic acid and the acid phosphates prevents its easy detection, and brings consumers to the belief that the meat is fresh.

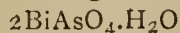
This peculiarity is the only one recommending the use of boracic acid.

A French commission, appointed to investigate the influence of boracic acid on the human system, found that it could be taken for a considerable time without producing any injurious effects. Yet it is certainly neither a regular constituent of the body nor is it contained in our food, and it is, therefore, doubtful whether the results reached by the commission must be considered as conclusive, as, in the course of time, constitutional difficulties may supervene, if such preserved provisions are taken for a considerable period.—*Journal of the American Chemical Society.*

A METHOD FOR ESTIMATING BISMUTH VOLUMETRICALLY.

By M. KUHARA,
Fellow in Chemistry, Johns Hopkins University.

BISMUTH may be estimated gravimetrically as—



(dried at 100° to 120°), by precipitating its nitric acid solution with arsenic acid or sodic arseniate, collecting the precipitate on a weighed filter after washing it by decantation, and weighing the filter and precipitate together.* In carrying out this process of estimation, however, various difficulties are met with.

It occurred to me that by taking advantage of Bøedeker's method for the estimation of arsenic acid,† the gravimetric method above referred to might be converted into a volumetric. My hopes were completely realised, as the new method gives very accurate results.

The method was based upon the precipitation of bismuth, as BiAsO_4 , from its nitric acid solution, by adding a measured quantity of standard solution of disodic arseniate in slight excess, and upon the determination of the amount of residual arseniate by means of standard uranic nitrate. In carrying out the estimation, the nitric acid solution of bismuth under examination must be free from hydrochloric acid, as well as substances precipitable by arseniate. Before entering upon the process of actual analysis, the amount of bismuth in a solution to be examined was approximately ascertained by the same method as that employed in the actual analysis.

Now a convenient quantity of the solution under examination was taken, a slight excess of disodic arseniate solution whose exact strength had been previously ascertained by a known weight of bismuth, was added to the solution, which was then agitated well without heating, and the mixture was set aside for a time to complete the reaction. The mixture containing the precipitate was rendered slightly alkaline with ammonia and then slightly acid with acetic acid. Now uranium solution, the relative strength of which as compared with that of arseniate had been previously ascertained, was allowed to run in from a burette very slowly, the mixture having been constantly well agitated to complete the reaction. The end of the reaction was recognised in the ordinary manner by means of ferrocyanide of potassium. From the amount of uranium solution used the excess of arseniate can be calculated; and from the actual amount of arseniate found by subtracting the excess from the whole amount taken, the quantity of bismuth can be easily calculated.

The following results were obtained:—

For the standard solution of arseniate about 20.87 grs. of disodic arseniate were dissolved in 1 litre of water, and for the uranium solution about 35.6 grs. of uranic nitrate.

Relative strengths of arseniate and uranium solution:—

	C.c. of Arseniate taken.	C.c. of Uranium Solution used.
1.	10	10.0
2.	10	9.9
3.	10	10.1
4.	10	10.2

Mean = 10.06

1 c.c. of arseniate = 1.005 c.c. of uranium solution.

Standardisation of arseniate in terms of bismuth:—

	Grms. of Bismuth taken.	C.c. of Arseniate taken.	C.c. of Uranium Solution used.
1.	0.10	13	6.0
2.	0.10	13	5.9
3.	0.10	13	5.7
4.	0.10	13	5.7
5.	0.10	13	5.7

Mean = 5.8

1 c.c. of arseniate = 0.01369 gr. of bismuth.

The following results were obtained by using a known weight of bismuth:—

	C.c. of Bi. taken.	C.c. of Arseniate taken.	C.c. of Uran. Solution used.	C.c. of Arseniate used.	Bismuth found.
1.	0.125	12	3.3	8.7	0.119103
2.	0.125	12	3.2	8.8	0.120472
3.	0.125	12	3.3	8.7	0.119103
4.	0.125	12	3.3	8.7	0.119103
5.	0.125	12	3.1	8.9	0.121841
6.	0.125	12	3.0	9.0	0.123210
7.	0.125	12	2.9	9.1	0.124579
8.	0.125	12	2.9	9.1	0.124579
9.	0.125	12	2.9	9.1	0.124579
10.	0.125	12	3.0	8.7	0.119130

These results were not quite satisfactory, so other experiments were made.

* H. Salkowski, *Journ. Prakt. Chem.*, 104, 170. *Zeitschrift für Anal. Chem.*, 8, 205.

† "Fresenius Quant. Analysis," p. 256.

Uranic nitrate solutions of different strengths were prepared, and that containing 43.2 grs. of crystallised uranic nitrate in 1 litre of water was found to give the best results. The strength of the arseniate solution remained the same.

Relative strength of sodic arseniate and uranic nitrate :—

	C.c. of Arseniate taken.	C.c. of Uranium Solution used.
1.	10	8.95
2.	10	8.90
3.	10	8.80

Mean = 8.88

1 c.c. of uranic solution = 1.126 c.c. of arseniate.

Standardisation of arseniate in terms of bismuth by using uranic nitrate solutions of different strengths :—

	Grms. Bismuth taken.	C.c. of Arsen. taken.	C.c. of Uran. used.	C.c. of Arsen. used.
1.	0.1	13	4.95	7.427
2.	0.1	13	5.10	7.258
3.	0.1	13	5.15	7.201

Mean = 7.295

1 c.c. of arseniate = 0.0137 gr. of bismuth.

The following results were obtained by using known weights of bismuth :—

	Grms. Bismuth taken.	C.c. of A sen. taken.	C.c. of Uran. used.	Excess of Arsen. found.	C.c. of Arsen. used.	Bismuth found.	Difference. Gain or Loss.
1.	0.125	12	2.57	2.893	9.107	0.124766	+0.000240
2.	0.125	12	2.60	2.927	9.073	0.124300	+0.000700
3.	0.125	12	2.60	2.927	9.073	0.124300	+0.000700
4.	0.125	12	2.55	2.871	9.129	0.125067	-0.000067
5.	0.125	12	2.56	2.882	9.118	0.124916	+0.000084
6.	0.125	12	2.60	2.927	9.073	0.124300	+0.000700

—American Chemical Journal.

A NEW ANÆSTHETIC.

At a recent meeting of the Edinburgh Odonto-Chirurgical Society in Edinburgh, Mr. W. Bowman Macleive brought under the notice of the members a new combination anæsthetic, which he had been using for some time past in his private studies with very successful results. It consisted in combining the administration or the exhibition of ethylen-dichloride with nitrous oxide gas. The manner of administration was simple. It consisted solely in placing a small piece of sponge, retained in position by a clip within the way-tube or supplementary bag of the nitrous oxide inhaler, leaving sufficient space on each side of the sponge for the free passage of the nitrous oxide into and out of the bag. Only a very small quantity of ethylen-dichloride was required, viz., about half a drachm. The time of inhalation to produce anæsthesia measured from 60 to 90 secs., and the time of complete anæsthesia—during which time the sensation was sure to be more profound and agreeable than when induced by nitrous oxide gas alone—was from 1½ to 2½ minutes, which embraced a period of time sufficient to enable most of the operations required in the dental surgery to be comfortably performed. He further mentioned that in all the cases in which he had used this anæsthesia, upwards of sixteen in number, there had been a complete absence of sickness, and only one case in which there might be said to have been the slightest approach to stertorous breathing. The pulse was slightly more accelerated than normal, but was full and strong, and there was that absence of lividity which renders administration of nitrous oxide so disagreeable, and in some cases

repulsive, to the onlooker, the lips alone, on the removal of the face-piece, presenting in any case, and that only occasionally, the faintest appearance of lividity. It had also this decided benefit over the single administration, that it produced a relaxation of the muscles so contrary to the almost spasmodic rigidity induced by nitrous oxide. The Society resolved to experiment with this anæsthetic.

A NEW FORM OF INSTRUMENT FOR THE DETERMINATION OF SPECIFIC GRAVITY.

By F. P. DUNNINGTON.

THE following easily portable apparatus, of which I have not seen any mention, affords a means of quickly obtaining the approximate specific gravity of bodies, and may be used under circumstances in which neither the balance, Nicholson's hydrometer, nor the spring balance of Prof. Jolly could be so conveniently employed.

This instrument consists of an hydrometer-shaped float, A, with the lower portion of the stem (B) graduated to one-tenth cubic centimetres, or other equal subdivisions of volume, and the upper portion (C) separated from it by a water-tight partition, the latter being left open above, and also graduated so that each subdivision shall mark off a volume contained in this tube C equal to the volume of the lower portion of the stem marked off by each of its divisions. Or by attaching the tube C to B, having the inner diameter of C equal to the outer diameter of B, portions of the same paper scale may be attached inside of B and outside of C, and the latter then covered with varnish.

This instrument may be most conveniently transported in the stout glass jar in which it is to float. It is used as



follows :—Partly fill the jar D with water of the temperature of the surrounding air; put a little water in C (enough to cover the substance of which the sp. gr. is to be taken), and read off its level; then float the instrument, and press it down—say five divisions—below the level at which it will rest, allow to come to rest, and take a reading of the depth at which it floats. Having broken from the substance, of which the sp. gr. is to be taken, a single lump which would nearly fill the upper tube, breathe on the piece, and with forceps carefully let it fall into the water contained in C (if a number of fragments or powder must be employed, after putting into the water in C, stir with a needle to get rid of air-bubbles); then, as before, press down the stem B the additional five divisions, allow to come to rest, and again read off the depth at which it floats, and the level of the water in C. The difference be-

ween the readings on B gives the volume of water equal to the weight of the substance added, and the difference between the readings on C gives the volume of the substance; then, dividing the former by the latter, we obtain the specific gravity.

The additional pressing down of the stem seems necessary to give uniformity to the meniscus formed on B. I find a little glycerin added to the water used in C serves well to perfect this meniscus: in making a reading of the level of the latter special care must be taken, since the appearance of the curves is altered when the substance comes near the surface of the liquid.

By using any liquid in the tube C, its sp. gr. may be determined with but 3 or 4 c.c., if it be not too volatile; and also the sp. gr. of solids soluble in water, but insoluble in some other liquid.

The instrument employed in making the following determinations has the following dimensions:—Cylinder (A), 50 m.m. long, 30 m.m. diam.; stem, 100 m.m. total length, 12 m.m. diam. The attached scale is of paper 60 divisions to the inch, the short tube C being attached by cement.

It would be preferable to have the tubes B and C united by fusion, and the scales etched upon the glass.

Uniform pieces of calcite, marble, and wolfram were selected, and the sp. gr. of each, for the temperature of the laboratory carefully taken, giving for these respectively

2.716 2.719 6.844

Of single pieces of each of these, weighing from 2 to 3 grms., the sp. gr. was thrice determined, giving—

(1)	2.72	2.77	6.58
(2)	2.77	2.83	6.85
(3)	2.72	2.78	6.95

Of single pieces weighing less than 1 grm. the sp. gr. was thrice determined, giving—

(1)	2.73	2.87	7.20
(2)	2.66	2.75	6.67
(3)	2.70	2.66	6.5

These eighteen determinations and their calculations required seventy-six minutes, and the longest time required for any one was five minutes.

It will be seen that there is considerable variation in these results, yet I would judge that they are sufficiently accurate for the ordinary use in determinative mineralogy.

University of Virginia.

AMMONIA FROM THE NITROGEN OF THE ATMOSPHERE AND THE HYDROGEN OF WATER.

On Wednesday, the 17th ult., was published a patent for the manufacture of ammonia from the nitrogen of the air and the hydrogen of water. The process is patented by Messrs. Rickman and Thompson.

Within the last twenty years the manufacture of ammonia synthetically has been several times attempted, and though in every attempt it is probable that ammonia has been made, yet never on a commercial scale. In all these attempts the process has been to combine the nitrogen and hydrogen directly at a low heat and receive the ammonia in solution in water, or by substitution, first forming a cyanide at a higher heat, and then indirectly producing ammonia by the decomposition of the cyanide, the result in both cases being ammonia in solution in water. Rickman and Thompson's procedure is altogether different; they produce ammonium chloride direct, and either in dry powder or in solution, and this by the simplest means and by the use of simple apparatus and inexpensive material. They do not use retorts, as has been done in all other

cases, but simply a closed brick furnace having the ash pit closed to regulate the supply of air, and they cause the vapour of water to be produced by the waste heat of the furnace itself. The deoxidising material actually used is the dust of steam coal, which costs only 1s. 6d. a ton at the pits; to this is added from 5 to 8 per cent of common salt; and this coal dust is the only fuel used, except at the commencement, when a bushel or so of coke is used for lighting. The great difficulty in making ammonia from the nascent hydrogen of water and the nitrogen of the air is the restricted limits of temperature, between generation and decomposition; it being necessary that carbon, in whatever form used, should be at a full red heat to decompose the vapour of water; but at a bright red heat, ammonia is decomposed. Now ammonium chloride under the same conditions is simply volatilised and not decomposed. As chloride of sodium or of calcium is decomposed at a full red heat in presence of nascent ammonia, therefore one of these chlorides is mixed with the coal, that ammonium chloride may be formed; so that if by chance the heat should be raised to a bright red no loss is sustained; the ammonium chloride is simply volatilised. By these means a greater range of working temperature is obtained. At the present time, with the consumption of from 20 to 28 lbs. of the mixture of coal dust and salt per hour, from 2 to 3 lbs. of ammonium chloride is formed.

If the process be worked in the vicinity of the coal pits, which undoubtedly it will be, this coal dust and salt mixture will cost under 1d. This being so, the revolution in agriculture spoken of by M. Georges Ville in his Treatise on Chemical Manures, recently published here, cannot be far distant, if it is to be produced, as he predicts it will be, when ammonium sulphate is sold at 1d. per lb.

THE PURIFICATION OF GAS.*

By HARRY EDWARD JONES, M. Inst. C.E.

In dealing with the general subject, the author placed the purification of bisulphide of carbon in the foreground. The late Dr. Letheby, in 1860, was responsible for the introduction into an Act of Parliament of a limitation to 20 grains of this impurity per 100 cubic feet of gas. Until the year 1866, no such limitation had been put upon companies outside the Metropolis, and since then it had only been imposed in important towns. Such limitations were nowhere enforced or respected. Dr. Letheby had an opinion that it was only necessary to resort to the exclusive use of lime as a purifying material; nevertheless it was unsuccessful when adopted in the works of the late Ratcliffe Gas Company, an average for two years (1875-6) giving 28 grains per 100 cubic feet. In January, 1872, the gas referees reported the impossibility of fixing a limit. In 1877, the failure of the simple line process led to the adoption of a different plan; the carbonic acid was carefully eliminated. Success was, however, only occasionally attained, and the condition of the gas at the outlet of each purifier in the series was in consequence carefully tested and recorded day by day. These tests would have been a formidable business, but for the introduction at that time of Mr. A. Vernon-Harcourt's colour tests. It was found that though carbonic acid was never allowed to pass the first purifier of a set of three vessels worked consecutively, it was only possible to keep the sulphur low when the third purifier was receiving a considerable quantity of sulphuretted hydrogen. Further, that so soon as the first vessel was taken off for the necessary replenishing, the sulphur rose many grains, and did not fall until the replenished purifier had been put to work again as the third vessel, and its contents had received more sulphuretted hydrogen. This action was illustrated

* A paper read before the Institution of Civil Engineers, March 9th, 1880.

by a table and diagram, as was also the action of a series of four lime purifiers. The advantage of further carbonating the lime, on works which were closely surrounded by dwelling houses, led to the adoption of the Beckton system. As it became unmistakable that sulphuretted hydrogen was mischievous when in excess, arrangements were made for abstracting it as it left the carbonic acid system of purifiers, by interposing oxide of iron. This was carried out with marked improvement, and the effective life of the sulphide of lime was much prolonged. In practice, both for the relief of back pressure, and for facility in renewing a sulphide-of-lime purifier, the carbonic acid set of purifiers should yield the following results:—

	Crude Gas, A B C D					
Sulphur.	40	52	45	30	28	} Grains per 100 cubic ft.
Carbonic Acid . . .	410	300	40	—	—	
Sulphuretted Hydrogen	800	900	400	—	—	

In this state of things the sulphur was generally 25 per cent. below that in the crude gas.

There was great advantage in exposing the material in the sulphide of lime purifiers to a current of air. Two purifiers at the Wapping works of the Commercial Gas Company, which had become ineffective, were opened, the lime examined, stirred up, and put back, and they afterwards worked for nine and eight months respectively, and did tenfold more work than previously. The prejudicial effect of cold in the reduction of sulphur in the carbonic acid system of purifiers, and the beneficial effect of the application of heat from the exhaust steam of an engine, were then referred to. No advance had been made in the direction of purifying in closed vessels with liquid agents. Mr. G. T. Livesey had succeeded in eliminating the carbonic acid to 0.5 per cent. in volume, by the use of rough caustic ammonia-liquor. The tar should be separated from the gas immediately on leaving the hydraulic main. The author allowed the gas to leave the condensers in summer at no higher temperature than that of the atmosphere, and, in winter, than 40° F.; in the former case the scrubbers usually reduced the gas to the temperature of 55°, and in the latter raised it to 45°. From the author's practice with water condensers he had found, in one type, that 1000 cubic feet per diem could be treated by 0.5 square foot of water. The result of condensation on the quality of the gas was then stated.

The purification of crude gas from ammonia proper was next referred to. The process was easy. The essentials were contact with extended superficies of water, or such concentrated intimacy as involved frictional resistance to the gas and the ultimate exposure of the gas to pure water. No sooner was lime substituted for oxide of iron in the scrubbers than ammonia appeared in the clean gas; this had been lessened by the addition of oxide of iron catch purifiers, and removed by a small quantity of sulphuric acid and sawdust in the last catch purifier. The cost of ammonia purification was very small, the particulars of which were stated.

Sulphuretted hydrogen was easily removed by lime, oxide of iron, peroxide of manganese, and sulphate of iron. Of these purifiers lime was the best, and oxide of iron the cheapest and least offensive. Oxide of iron, sulphate of iron, and manganese converted the sulphuretted hydrogen on exposure to the air into free sulphur, becoming oxides again, and being, as it was called, revived.

Decomposition of Carbonic Acid by Plants under Artificial Illumination.—P. Dehérain and L. Maquenne. —Leaves plunged in water and exposed to a powerful light, such as that of a Drummond or a Bourbouze lamp, decompose carbonic acid and evolve oxygen. This phenomenon, however, occurs only so long as the luminous rays predominate. If the dark rays predominate, as when a Bourbouze light acts through a stratum of benzin or chloroform, oxygen is consumed and carbonic acid libera.ed.—*Ann. Agronom.*, v., 401.

PROCEEDINGS OF SOCIETIES.

INSTITUTE OF CHEMISTRY OF GREAT BRITAIN AND IRELAND.

Report of Council.

As at the preceding annual general meeting, the Council has again to congratulate the members on the increasing prosperity of the Institute. At the time of the last meeting there were on the register 341 Fellows and 53 Associates, whilst at the present time there are 370 Fellows and 54 Associates, besides 1 Fellow who has recently been elected, but not yet formally admitted, making in all 425 members. Three candidates have been admitted to the Associateship of the Institute, and two who were Associates have passed to the grade of Fellow, so that there is only an increase of one in the number of Associates at present on the register. During the past year we have lost one Fellow by death, Mr. T. Wills, the Chemical Secretary to the Society of Arts, and two Fellows have resigned. The increase in the number of members must be looked upon as decidedly satisfactory, considering that the regulations under which candidates are admitted are now so much more stringent than in the preceding year; the thorough and searching examination in practical chemistry which is demanded of candidates for the Associateship has, for the present, limited the number who have taken the necessary step to qualify for that grade.

One of the two prizes of £50 offered by the President for original investigations involving gas analysis has been awarded to Mr. Leonard Dobbin, for his research on "Some Reactions of Tertiary Isobutylic Iodide," which has been accepted by the Council as sufficient and satisfactory evidence of training in practical chemistry to entitle him to the Associateship.

The numerous applications with reference to the qualifications necessary to obtain admission to the Institute are evidence that the advantages to be derived from organisation amongst professional chemists are more and more appreciated, and this is especially the case with the younger members of the profession. The small number of candidates who have hitherto presented themselves for practical examination would seem to indicate that the chemical student does not usually devote sufficient time to the attainment of a sound knowledge of analytical chemistry; there can be no doubt, however, that the thorough preliminary training required to pass the practical examination now imposed by the Institute as a condition of obtaining admission as Associate, will induce students to prolong the period of study, and to pay more attention to the various branches of exact analysis.

A meeting was held in February last immediately after the annual general meeting for the purpose of considering the alterations in the articles proposed by the Council, which alterations were carried unanimously. The members will recollect that the greater number of the changes introduced were for the purpose of avoiding the confusion arising from the manner in which the terms "Member" and "Fellow" were used in the original Articles of Association. As the articles stand at present, every chemist who has been formally admitted is a Member of the Institute, and is entitled to vote at all meetings, but Associates are not eligible as officers or Members of Council.

Conferences have been held since the last Annual General Meeting, at which the subjects discussed were:—
"The Adulteration of Food."

"The Relations of the Chemical Profession to Public Sanitation," that on the Adulteration of Food occupying two evenings. These meetings have been so successful hitherto that the Council has decided to continue them. It is a matter of regret that the great body of the members of the Institute in the provinces cannot attend these meetings, and thus have an opportunity of taking an active part in the discussions; but, in order to obviate

this difficulty as far as possible, it has been decided to print the paper which forms the subject of discussion and circulate it amongst the members some time previously, so as to afford those who reside away from London and cannot attend the meetings an opportunity of forwarding to the secretary any remarks they may desire to make, which can then be read at the meeting. It was also thought that this step would give members an opportunity of carefully considering the subject before the meeting, and thus facilitate the discussion.

A Parliamentary Committee has been appointed, whose business it is to watch over any Bills which may be brought forward in Parliament which may concern the chemical profession, and to call the attention of the Council to any which they deem likely to affect the interests of the members of the Institute. Negotiations with the Pharmaceutical Society are pending for the alteration of the clauses in the Pharmacy Act which restrict the use of the title of "Chemist" to those registered under that Act, in the event of any amendment of the Act being brought into Parliament.

One of the two prizes of £50 each, offered by our retiring President, Prof. Frankland, for "the best original investigation involving gas analysis," still remains to be awarded. Dr. C. Meymott Tidy has also offered a prize of £25 for the best original investigation on "Special Reactions of the Alkaloids and their Separation from Organic Mixtures." These prizes are open not only to Associates, but to all persons, except Fellows of the Institute, who shall before the 31st December next have qualified themselves for the Associateship in all respects short of passing the prescribed practical examination, and the Council has decided to accept successful competition for these prizes in place of such practical examination.

The Institute is again indebted to the President and Council of the Chemical Society for the use of their rooms during the past year.

President's Address.

Dr. FRANKLAND said—You have heard the Report of your Council and the financial statement of our treasurer, and I think you will agree with me that they are both eminently satisfactory. We have, on the year, an increase of upwards of 30 Members, and our income has exceeded our expenditure by some £277. Three years have not yet elapsed since the draft scheme for this Institute was laid before the Organisation Committee, when a Sub-Committee was appointed to take all necessary steps for our incorporation, when the names of 48 chemists were added to those of the Committee, and when the first Officers and Council were elected. Many obstacles had still to be overcome before the incorporation of the Institute was accomplished seven months later; and at the first annual general meeting, two years ago, only 225 Fellows had joined the Institute. To-day we have an aggregate of 425 Members, consisting of 371 Fellows and 54 Associates, a number which the Chemical Society had not attained before its twenty-third anniversary.

The mere number of our Members, however, so far from being a subject for congratulation, might be one to be deplored if a strict investigation of the qualifications of candidates for admission had not been maintained. By lowering our standard we could, doubtless, have doubled our numbers, but this would have been to forego the essential object of the organisation—the guarantee that consulting and analytical chemists are duly qualified for the proper discharge of the duties they undertake. An inspection of our register will, I think, convince you that your Council has fairly effected a separation of the competent from the incompetent members of the chemical profession, and although there are some practising chemists of eminence whose names we should like to see enrolled in our list of Fellows, it is satisfactory to know that their number is very small. The period during which they can be admitted under the provisional regulations is now rapidly drawing to a close, for, after the 2nd of October next, no

one can, by the Articles, enter our body, either as a Fellow or Associate, without first passing the prescribed examinations. It is, therefore, desirable that our Members should use their influence to bring in those few competent chemists who may, from one cause or another, be still unattached to the Institute, for it is evident that the existence of obviously competent professional chemists outside our organisation will be a source of confusion to the public, and will tend to lengthen the time before Membership of this Institute will be considered to be as essential to the practice of chemistry, as are the corresponding badges of competency in the professions of law and medicine.

In my own individual experience as a teacher of chemistry, I find manifested amongst students, and especially amongst the better class, a continually increasing interest in the Institute; and I know, at the present time, many students who are undergoing special training for the Associateship. But the objects and advantages of the Institute are not patent to everyone; there are no popular developments of our functions, we have no journal, we have not hitherto enjoyed much notice from the Press, and we do not even hold any public meetings. Indeed, it would be contrary to our principles to push ourselves into notice with the object of adding to our numbers; but it is, therefore, all the more necessary that our *professorial* members should bring the Institute under the notice of their students, encouraging them to prepare for admission to the Associateship; for, by so doing, they would not only be contributing to the elevation of chemistry as a profession, but also to the better education of chemical students in general; since the training prescribed by us in theoretical and analytical chemistry, physics, and mathematics, with incentives to original investigation, is precisely that which is necessary for all competent chemists, whether professional or professorial.

It should never be forgotten that the paramount function of our Institute is the *registration* of trained chemists competent to undertake the investigation of the various problems which arise in ever increasing numbers in connection with the applications of our science to the arts, public health, agriculture, and technical industry; and the *publication* of this register in such a manner as to bring it to the notice of Government departments, boards of health, public sanitary Authorities, Manufacturers, and others who require the aid of chemical experts. But there are other subsidiary functions incumbent upon us of no small importance; amongst these are watchfulness of such legislation as is likely to affect our body, and, secondly, the cultivation of professional ethics and good fellowship by the holding of conferences for the discussion of questions arising out of the practice of our profession. Since incorporation, our parliamentary functions have not had many demands made upon them, but the Conferences on "Trade Certificates," "The Adulteration of Food," and "The Relations of the Chemical Profession to Public Sanitation," have excited much interest amongst our Members, and the reports of the discussions have no doubt been very useful as means of interchange of opinions on the subjects treated of.

Valuable suggestions for the utilisation of the Institute in other directions have been made from time to time, and probably some of these will ultimately be acted upon; but, as they involve the expenditure of considerable sums of money, they should be undertaken with deliberation and caution, for it behoves us, at the outset of our career, to practise strict economy. We may at any moment be involved in parliamentary or legal proceedings; these are, of course, very costly, and our power for good in connection with them will be very much in proportion to the amount of our investments and the balance at our bankers. Much of our ultimate success in winning due recognition for the Institute will depend upon our steady accumulation of capital, and I consider it, therefore, a very fortunate feature in our present position, that the current expenses are so moderate. Thanks to the

hospitality of the Chemical Society, we are rent free; most of the work is done without remuneration, and our chief outlay is incurred in the performance of that most important function, the printing, advertising, and distribution of our register.

Our present position may, therefore, be thus summarised. We have as many competent members as we could hope to extract out of the general mass of professional chemists, our finances are in a flourishing condition, our standards for admission are as high as the scientific training in our schools and universities will allow, the register of our Members is annually put into the hands of those who are in the habit of resorting to the aid of professional chemists, and there is every reason to hope that the influence and progress of the Institute will be no less satisfactory in the future.

CORRESPONDENCE.

"NEW" METHODS.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 114, Dr. J. Grossmann introduces "A New Method for the Alkali-metric Determination of Sulphates," which has during the last few years been several times introduced as "new." It has been proposed in 1877 by P. Haubst (*Ibid.*, vol. xxxvi., p. 227), but before that gentleman, and, as far as I can find, originally, by M. F. Jean (*Comptes Rendus*, 83, p. 973, in abstract in CHEMICAL NEWS, vol. xxxiv., p. 272, 1876). It was also described by Jean and Pellet in *Bul. Soc. Chim.*, in abstract in *Chem. Soc. Journ.*, 1877, vol. ii., p. 353.

It would surely be a simple matter to look over the indices of a few volumes of the CHEMICAL NEWS, or of any other chemical journal, before describing and publishing methods as "new." Much time might thus be saved to reading chemists, and confusion avoided.

A curious point will be noticed by attentive readers on comparing the abstract of Jean's paper in the CHEMICAL NEWS with Haubst's "original" paper in the same journal.

JEAN, 1876.

"... The solution . . . is mixed with a slight excess of baryta water and then with Seltz water . . . It is raised to a boil, and the whole is filtered. . . .

"The mixed precipitate having been washed with boiling water until the washing waters no longer present an alkaline reaction, the filtrate is mixed with tincture of litmus, raised to a boil, and titrated with a standard solution of sulphuric acid.

"The quantity of sulphuric acid necessary . . . is exactly the same as what was combined with the alkalies, potassa, or soda. . . ."

The similarity may be accidental, but if so is remarkable enough to deserve being put on record.—I am, &c.,

HAUBST, 1877.

"... Add a slight excess of baryta water and then pass in a current of CO₂ gas, or mix with it water highly charged with this gas; let it boil for a few minutes, and filter.

"The precipitate is washed with boiling water till the washings are neutral to test-paper, and the filtrate, which contains now the alkalies as carbonates, is titrated with . . . centinormal sulphuric acid.

"The amount of acid consumed is exactly the same as that originally combined with potash or soda."

OTTO HEHNER.

54, Holborn Viaduct, London
March 15, 1880.

"NEW METALLIC COMPOUND."

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS (vol. xli., p. 87) you reported Dr. Cole's lecture on a "New Metallic Compound," and I have endeavoured to avail myself of it, in taking a reverse or "matrix" from plaster medallions, the result of which, though dissatisfying to myself, may be of interest to some of your readers.

As the lecturer considered it immaterial what sulphide was used, I used the common black sulphide of antimony (Sb₂S₃) which I had by me, and the proportions which I found most fluid at the temperature mentioned in the lecture were four parts of sulphide and three parts of sulphur.

This mixture took a sharp impression from embossed paper without injuring it in the least; but with plaster (CaSO₄) it was quite different, the two surfaces adhered together entirely all over like a solid block; and after trying many expedients to separate them, I was obliged to break off the plaster piecemeal. The parts in contact with the "metallic compound" were quite brown, altered in texture, and impaired in firmness, and thus the medallion of Dante was quite spoiled; but the metallic matrix formed in the experiment was sharp and good except for numerous small air bubbles, caused probably by a slight formation of sulphurous acid. If it be suggested that the plaster should have been oiled, I need only say that sulphuretted oil would quite spoil the plaster.

If the sulphur in the "metallic compound" were in a state of chemical union or combustion, the effects described could not be attributed to its presence.

To ascertain this point I subjected a portion of the "new metal" to the action of bisulphide of carbon, and in a few minutes it dissolved out the whole of the sulphur quite unaltered, that is to say, if 4 grs. Sb₂S₃ are fused at 320° F. with 3 grs. sulphur, and the mass when cold is well washed with bisulphide of carbon, there will be left 4 grs. Sb₂S₃, and the solution on evaporation yields 3 grs. of crystallised sulphur.

Under these circumstances, we surely cannot regard it as anything more than a concrete or cement, in which the sulphur acts as a flux to promote fusion at a lower temperature.

And may I venture to suggest that this ready separation of sulphur from the mass by mere solvents impairs its utility for many purposes; there are volatile products in coal-gas, which have a strong solvent action on sulphur, and in their presence such caulking would soon become porous, to the great loss of the companies.

And in the laboratory it would be necessary to bear in mind, that all volatile products which dissolve or soften sulphur would render the use of this lute or cement quite inadmissible.—I am, &c.,

ALFRED PAYNE, F.C.S.

Ettingshall, Wolverhampton.

BLOWPIPE ANALYSIS.

To the Editor of the Chemical News.

SIR,—In expressing the gratification I feel at honest praise from a writer who understands the subject so well as "Triton" (CHEMICAL NEWS, vol. xli., p. 133) obviously does, I would venture to say, with regard to the "ellychnine pyrochrome" to which he objects, that the long name—derived from the Greek for a candlewick—is the worst part of it, but I could not get a better one.

I showed this test for lime, &c., to some gentlemen of the Royal School of Mines, in Jermyn Street, about five years ago, by touching the wick of a candle from which a blue pyrocone was being produced—with the causticised fragment of one of the old fossil bones kept so carefully in the Museum there—when I well remember one of the gentlemen present exclaiming "This is equal to strontia!"

the "this" referring, of course, to the intense red colouration thus produced. I firmly believe this red colour to be due to *burning hydrogen*, suddenly freed through the decomposition of part of the blue hydrocarbonous "flame" by the caustic lime, for three reasons:—

- (a.) Because if you direct this blue hydrocarbonous pyrocone upon clean shining aluminium plate there is no result; but if you insert a fragment of caustic lime carbon is soon deposited.
- (b.) The same fragment of lime which has afforded this fine orange-red colouration at the *base* of the blowpipe pyrocone, if inserted at its *apex*, or in what is called "O.F.," gives chiefly the *yellow* which is ascribed to sodium, but which I firmly believe to be due to "chemical water."
- (c.) The fumes of common salt, volatilised by this pyrocone in a room, give it after a time a red mantle, fainter, but of the same tint as that afforded by caustic lime, and we *know* that hydrochloric acid is produced by volatilising common salt in ordinarily moist atmosphere, and, also, that undecomposed hydrochloric acid burns with a yellow flame.

The vesiculation test for potash in presence of soda, also objected to by your correspondent, is not properly described in my work, "Pyrology;" but if a little pure silica is fused with the test, and not too much of that—as too much alkali, either potash or soda, spoils the reaction—added to the bead of boric acid which is vesiculated and breathed upon, I think every candid operator will find that the presence of potash enables the vesicle to cloud over, and that soda only prevents such clouding over, but the proportion of silica has yet to be determined.

Your correspondent will perhaps be glad to hear that the examination of boric acid beads in which various oxides and sublimates, &c., had been decomposed by high microscopic power has afforded the most surprising results, promising to throw light on the much-vexed question of the real constitution of metals, &c., and to make the chemistry of substances *in fusion*, so little studied hitherto, "the Chemistry of the future."—I am, &c.,

W. A. Ross.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 8, February 23, 1880.

Formation-heat of Persulphuric Acid.—M. Berthelot.—The formations of oxygenated water, of persulphuric acid, and of ozone, are endothermic, and form a graduated scale.

Ozone, $O_2 + O$, absorbs 14.8 cal.

Persulphuric acid, $S_2O_6 + O (= S_2O_7)$ 13.8 „

Oxygenated water, $HO + O (= HO_2)$ 10.8 „

These bodies are respectively transformable(?) All three contain active oxygen; that is to say, such as is ready to act upon oxidisable bodies with more facility than ordinary oxygen, because they contain an excess of energy derived from the thermic excesses pointed out here.

Decomposition of Oxygenated Water and the Derivatives of Barium Binoxide.—M. Berthelot.—The spontaneous decomposition of hydrated barium binoxide is explicable by the displacement of the second equivalent of oxygen by water, the compound being thus converted into hydrated baryta, with disengagement of heat.

Combination-heat of Chloral Hydrate.—A. Wurtz.—The author, having repeated his experiments with the

apparatus figured and described, finds that his former conclusions are confirmed, *i.e.*, that the vapours of water and of anhydrous chloral may be mixed without giving rise to a sensible disengagement of heat, a fact which supports the opinion that the vapour of chloral hydrate is a mixture and not a definite compound. M. H. Sainte-Claire Deville pointed out that in such experiments the temperature in the central receptacle is very much influenced by the proportion between the volumes of the respective vapours.

Production and Crystallisation of an Anhydrous Silicate in Presence of the Vapour of Water at Common Pressures.—S. Meunier.—The author has obtained crystals of enstatite by bringing into contact at a suitable temperature, vapours of water, of magnesium, and of silicon chloride.

Comparison between the Tension Curves of Saturated Vapours.—P. de Mondesir.—Not susceptible of useful abstraction.

A New Electro-magnet.—M. Chambrier.—The author enlarges the surfaces of the extremity of the nucleus and of the oscillating armature. He causes the nucleus to penetrate into a cavity formed in the armature, or he permits its circumference to penetrate into a circular groove hollowed in the armature.

Use of Tempered Glass for the Construction of Condensers.—M. Ducretet.—Vessels of tempered glass, used as Leyden jars, can receive very strong charges of electricity without being perforated, and consequently give condensed sparks of a power much superior to those commonly obtained. Plates of tempered glass may be employed for the same purpose.

Preparation of Acetylen.—E. Jungfleisch.—This memoir requires the accompanying illustrations.

Determination of the Combustion-heats of Glycerin and Ethylenic Glycol.—W. Louguine.—The quantity of heat disengaged in the transformation of glycerin is 392,455 cal. Ethylenic glycol yields 283,293 cal.

A Digestive Ferment Produced during Panification.—Scheurer Kestner.—During panification there is produced a peculiar ferment, having a strongly digestive action upon the flesh of animals, analogous to the digestion produced by the vegetable pepsin of the carnivorous plants.

No. 9, March 1, 1880.

This issue contains the account of the annual meeting of the Academy, with the presidential speech of M. Daubrée, and the awards of the various prizes given as rewards for scientific observations.

Chemisches Central-blatt.
No. 4, 1880.

Artificial Alkaloids.—A. Ladenburg.—The author proves the identity of the artificial atropin prepared from tropin and tropic acid with the natural base. By treating the salts of tropin with hydrochloric acid, he has obtained a class of bases which he names tropeins. Amongst these may be mentioned salicyl-tropein, phthalyl-tropein, and oxytoluyl-tropein. Hyoscyamin and duboisin may possibly belong to this class of alkaloids.—*Berlin Monatsber.*, 1879, p. 779.

Ichaboe Guano.—B. C. Niederstadt.—The analysis of a cargo after treatment with sulphuric acid.—*Landw. Vers. Stat.*, xxiv., 269.

Liquid for Preserving Vegetable Preparations.—J. Nessler.—Alcohol at 20 per cent. with the addition of a little calcium sulphite.—*Landw. Vers. Stat.*, xxiv., 275.

Contributions to the Analysis of Wine and Must.—R. Ulbricht.—For the last three years the author has used the method of Parkes for determining the cuprous oxide separated out in performing Fehling's test.—*Landw. Vers. Stat.*, xxiv., p. 253.

Improvements in the Iodine Industry and in the Determination of Iodine.—B. Wetzig.—The simplest and most certain method for the determination of iodine is that of Fresenius.

No. 5, 1880.

Use of Aluminium in the Laboratory.—F. Stolba.—The author recommends this metal for evaporating-dishes, spatulæ, clamps, &c.—*Listz. Chem.*, iv., 34.

Signification of Calcium Oxalate in Plants.—B. J. van der Ploeg.—Lime in leaves increases with their age, but there is not a proportionate increase in oxalic acid. Some leaves, as those of the chestnut and the elm, are free from this acid, whilst in others, such as rhubarb, it is found in excess as compared with the lime. During the growth of plants, lime accumulates in the leaves and the rind, but not in the seeds, the roots, or the wood. Oxalic acid probably serves to keep the albuminoids in solution.—*Naturforsch.*, xiii., 17.

Action of Diastase and Pytaline.—M. Kjeldahl.—The author examines the influence of temperature, time, and concentration upon the diastatic process. The action of pytaline upon crude starch is very trifling, but it is much stronger upon starch-paste.

Manufacture of Opal and Alabaster Glass.—The following mixture is now in use:—Felspar, 20 to 78 per cent; blue lime, 17 to 60 per cent; and heavy spar, 5 to 40 per cent.—*Moniteur de la Céramique*.

Detection of Copper in Olive Oil.—O. Cailletet.—The author dissolves 0.1 grm. pyrogalllic acid in 5 c.c. of ether, adds 10 c.c. of the oil and agitates. Copper, if present, is precipitated as a pyrogallate and the liquid takes a brown colour.

Examination of Alkaline Dicarbonates.—M. Lalieu.—1 grm. pure NaHCO_3 , if treated with an excess of acid, gives off 0.524 grm. or 264 c.c. carbonic acid. The commercial salt, a mixture of the mono- and di-compound, gives, with an excess of acid, a quantity of acid, which is called V, and is divided into two unequal parts. Let one part, a , be unchangeable = $\frac{264}{2} = 132$ c.c., which are required to combine with the total soda so as to form a neutral carbonate. The other portion, b , or $(V - 132)$ c.c., forms the second molecule of carbonic acid. If, e.g., $b = 9$ -10ths, 8-10ths, 7-10ths, &c., of a , then the salt under examination consists of 9-10ths, 8-10ths, &c., dicarbonate, the residue being neutral carbonate. It is therefore sufficient to divide b by a , in order to find the proportion of dicarbonate. A more precise method is to determine the residue left on ignition as well as the volume of gas evolved.—*Journ. de Pharm. d'Anvers*.

Preparation of Water free from Ammonia.—The pipe conveying the steam from the still is not connected directly with the condenser, but opens into the bottom of a large iron drum containing about 10 gallons, whilst the head of the drum is connected with the condenser in the ordinary manner. Part of the steam condenses in the drum falls to the bottom, and is kept continually boiling by the inrush of steam. If the water is drawn off here by a suitable arrangement it is found perfectly free from ammonia, which is met with in abundance in the water running from the condenser. In this manner a gallon of water, perfectly free from ammonia, may be obtained per hour.—*Arch. Pharm.*, xv., 458.

Separation of Quinine from Strychnine.—M. Dwars.—The mixed solution is supersaturated with ammonia and shaken up with chloroform. After the latter is evaporated the residue is dried at 110° , dissolved in warm water with the addition of sulphuric acid, neutralised with ammonia, and mixed with an excess of ammonium oxalate. After the lapse of twenty-four hours the quinine is collected as oxalate, and weighed.—*Arch. Pharm.*, xv., 463.

Bulletin de la Société Chimique de Paris,
No. 12, Dec. 25, 1879.

Fermentations Produced on Extracting the Saccharine Juice of Beet-root by Diffusion.—MM. A. Millot and Maquenne.—Hydrogen is given off in consequence of the butyric fermentation, but the acetic acid which is formed may also, at the temperature of the operation, react upon the sheet-iron of the apparatus and produce a further quantity of hydrogen. The destruction of sugar amounts to 1 per cent.

Transformation of Bromised Styrolen into Methylbenzoyl.—C. Friedel and M. Balsohn.—The authors heated bromised styrolen, with an excess of water, to 180° , in sealed tubes, for twelve hours. The products were hydrobromic acid and an oily matter containing methylbenzoyl.

Limited Oxidation of Ethylbenzin.—C. Friedel and M. Balsohn.—The authors conclude that in the oxidation of aromatic hydrocarbons, at least of those in which a group CH_2 is attached to the benzenic nucleus, there is first formed an acetone, which is then split up in the known manner.

Synthesis of Ethylbenzin by means of Ether and Benzin.—M. Balsohn.—This synthesis is effected by heating together 1 part of ether, 2 parts zinc chloride, and 4 parts of benzin, in a sealed tube, for twelve hours, at the temperature of 180° .

Russian Chemical Society, Session Feb. 1/13, 1879.—MM. Beilstein and Jawein communicate an exact method for the determination of zinc, consisting in the electrolysis of the solution of the double cyanide of zinc and potassium.

MM. Beilstein and Kourbatoff find that the principal product of the oxidation of bromo-naphthalin is phthalic acid. There is likewise formed a resinous body containing bromine and bromo-naphthalic acid.

M. Schalfieff has ascertained that the melissic acid of Brodie is not a homogeneous substance.

M. Potililtzine described researches on the reciprocal displacement of the elements. At 20° chlorine acts upon the bromides of sodium, potassium, and silver, but the decomposition is never complete. In the sulphide of silver the percentage of sulphur displaced by selenium is 68.80; in copper sulphide, 48.01; and in lead sulphide, 65.72.

No. 1, Jan. 5, 1880.

Direct Combination of Cyanogen with Hydrogen and the Metals.—M. Berthelot.—Already noticed.

On Epichlorhydrin.—M. Berthelot.—Already noticed.

No. 2, Jan. 20, 1880.

Explosion of a Platinum Retort used for the Concentration of Sulphuric Acid.—F. Kuhlmann (fils.).—Already noticed.

Action of Acetic Anhydride upon Certain Aldehyd Phenols.—P. Barbier.—The author has obtained acetylparoxybenzoic and acetylated oxytoluic aldehyds, the latter compound both in a liquid and solid state.

On Iso-phthalophenon.—E. Ador.—The author establishes the symmetric constitution of this body.

No. 3, Feb. 5, 1880.

Researches on the Reversion of the Phosphates in the Superphosphates used in Agriculture.—A. Millot.—The new reversion discovered by means of ammonium citrate is due exclusively to the action of phosphoric acid and acid phosphate of lime upon ferric oxide, forming bicalcic phosphate. On a pasty mass containing iron oxide, iron phosphates with an excess of base are formed, even if the mass is strongly acid. These basic compounds are not soluble in ammoniac citrate.

Composition of Slate.—E. J. Maumené.—Many varieties of slate contain a large proportion of calcium car-

bonate, and are in consequence readily attacked by atmospheric agents, acids, &c.

Determination of Urea by means of Alkaline Hypochlorites and Hypobromites.—A. Fauconnier.—The author has observed that the presence of saccharose does not increase the proportion of nitrogen given off by urea under the influence of sodium hypobromite, but that in presence of glucose the theoretical quantity is obtained.

Influence of Sugars on the Determination of Urea in Urine.—M. Jay.—The conclusions arrived at are that neither glucose nor cane-sugar should be employed in the determination of urea, the former being alone attacked by the hypobromite, and the second yielding a volume of gas which increases with the strength of the solution.

On Active Methyl-propyl-carbinol, and on Certain Fungi effecting Complete Combustion.—J. A. Le Bel.—The Fungi in question are *Penicillium glaucum* and *bicolor*.

The Positions α and β in Naphthalin.—F. Reverdin and Nölting.—Already noticed.

Chemiker Zeitung.
No. 4, 1880.

Influence of Manufacturing Refuse on Animal Life.—Dr. Auerbach.—The author mentions a chemical manufactory in England where large quantities of solution of sodium sulphate accumulated, and in order to recover the salt were allowed to evaporate spontaneously in large tanks about 30 yards square. During an entire summer he observed in these tanks numerous water-beetles, *Gyrinus natator*, which on the approach of danger hid themselves among the crystals of Glauber's salt, and did not appear in the least incommoded by the nature of the fluid they inhabited. One of these tanks having leaked into a ditch and thus contaminated a river at the distance of about two miles, thousands of dead fish were seen floating on the surface.

No. 5.

A New Method of Determining the Value of Zinc-powder.—V. Drewsen.—If zinc-powder is brought in contact with sulphuric acid and acid potassium dichromate, using of the latter more than twice the quantity theoretically needful, the nascent hydrogen reduces the chromic acid to chromic oxide. To 1 grm. zinc-powder is added 100 c.c. of a solution of pure potassium dichromate (40 grms. to 1 litre), and two quantities of dilute sulphuric acid, each of 10 c.c., are introduced successively, stirring well all the time. As soon as the zinc-powder is dissolved, with the exception of a small residue of insoluble matter which is almost invariably present, an excess of sulphuric acid is added, and 50 c.c. of a strong solution of iron sulphate (200 grms. to 1 litre), the action of which upon the solution of the dichromate has been determined previously. A slight excess of the same liquid is then cautiously added, titrating afterwards back with the solution of acid potassium dichromate until a drop of the liquid is no longer turned blue by potassium ferricyanide. The quantity of potassium dichromate used, if multiplied by 0.66113, gives the metallic zinc present in the sample of zinc powder. The results obtained agree well with those furnished by the method of Fresenius, *i. e.*, combustion of the hydrogen by means of copper oxide, and weighing the water formed. For pure zinc the process is not applicable.—*Zeitschrift Anal. Chemie.*

NOTES AND QUERIES.

Valuation of Soda-Ash.—Being a subscriber to the *CHEMICAL NEWS* of some twelve or fourteen years' standing, I beg to ask the following question:—Having a quantity of soda-ash containing 52 per cent of alkali, and another quantity of weak soda-ash only containing 12 per cent of alkali, will you kindly tell me by what calculation I may find the proportion of each to add to bring the whole to the uniform strength of 34 per cent?—K.

MEETINGS FOR THE WEEK.

- MONDAY, 5th.—Medical, 8.30.
— Royal Institution, 5. General Monthly Meeting.
— Society of Arts, 8. Robert W. Edis, F.S.A., "The Decoration and Furniture of Town Houses."
TUESDAY, 6th.—Civil Engineers, 8.
— Zoological, 8.30.
— Pathological, 8.30.
— Royal Institution, 3. Prof. Huxley, "Dogs and the Problem Connected with them."
— Society of Arts, 8. Mr. C. Pfounds, "Art in Japan."
WEDNESDAY, 7th.—Society of Arts, 8. E. C. Robins, F.S.A., F.R.I.B.A., "Buildings for Secondary Educational Purposes."
THURSDAY, 8th.—Royal Institution, 3. Prof. Tyndall, "Light as a Mode of Motion."
— Royal Society Club, 6.30.
— Society of Arts, 8. F. J. Friswell, F.C.S., "Recent Improvement in Benzine Colours."
FRIDAY, 9th.—Royal Institution, 8. Prof. Huxley, "The Coming of Age of the Origin of Species," 9.
— Astronomical, 8.
— Quekett, 8.
SATURDAY, 10th.—Physical, 3.
— Royal Institution, 3. Mr. James Sully, "Art and Vision."

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THE CHEMICAL NEWS.

VOL. XLI. No. 1063.

PEROXIDE OF HYDROGEN AND OZONE.

By ALBERT R. LEEDS, Ph.D.

IN the CHEMICAL NEWS (vol. xxxviii., pp. 224, 235, 243, 249, and 257) I published a series of articles, the chief object of which was to prove that when the so-called atmospheric ozonoscopes were prepared with the utmost precautions, and subjected in the laboratory under known conditions to the operation of vapours and gases other than ozone, such as nitrous acid, hydrogen peroxide, &c. (bodies existant to a greater or less extent in the earth's atmosphere), they all underwent change. Hydrogen peroxide affected all the ozonoscopes, thallous hydrate excepted. The results obtained with this were entered as doubtful. In one series of trials, operating with a very dilute solution of peroxide, the thallium paper was yellowed: in a second series, the yellow stain was upon the edge only of the paper, the centre remaining white. These results were there explained by the supposition of the "thallium peroxide undergoing reduction in contact with hydrogen peroxide, water and oxygen being liberated." In other words, while the effects of hydrogen peroxide in changing the thallium ozonoscope were visible, yet, accepting as true the generally received statement of Schönbein concerning the mutual decomposition of hydrogen and thallium peroxides, I attributed the final slight result to the reduction of any thallium peroxide first formed. This explanation was very unsatisfactory to me at the time, inasmuch as it necessitated the supposition of an initial oxidation, followed by a reduction effected by the continued operation of one and the same reagent, and I was engaged in its further study, when the publication of the admirable paper by E. Schöne on this subject, definitely settled the question.* He there demonstrates the fallacy of Schönbein's statement, and shows that hydrogen peroxide can convert thallous into thallic oxide, not only when the peroxide exists in solution, but also when it is in the form of vapour.

It may be regarded as established, therefore, that none of the so-called ozonoscopes necessarily indicate the presence of ozone in the atmosphere.

While my paper, written with the object of demonstrating the proposition just enunciated, was in course of publication, Mr. C. T. Kingzett made the unwarranted charge (CHEMICAL NEWS, vol. xxxviii., p. 243) that I seemed "to have fallen into an error committed by many other observers, namely, that of estimating as ozone everything in the atmosphere capable of colouring so-called ozonoscopic papers." Moreover, he based this charge on a misquotation, for in the paragraph he quotes, I speak not of the ozonic reaction, but of the "so-called" ozonic reaction, an epithet which he borrows in his own article.

In a later communication (CHEMICAL NEWS, vol. xl., p. 96), Mr. Kingzett says, "There is no known process of slow oxidation which has been established to produce ozone." Couched as it is in such positive terms, this assertion contravenes the statements of those who have experimentally investigated the subject during the past forty years. It might, therefore, not unreasonably be anticipated that Mr. Kingzett should bring forward some new and crucial experiments of his own. He adduces none. At the same time he fails to recognise certain distinguishing properties of ozone and hydrogen peroxide, and apparently is not conversant with many phenomena exhibited in the oxidation of phosphorus in moist air.

Now, the property of ozone, most strikingly characteristic, is its smell. This smell, so far as long-continued familiarity with it enables me to judge, and whether the ozone is derived from the oxidation of phosphorus, or results from the action of the silent discharge on pure and dry oxygen, or accompanies the electrolysis of acidulated water (and the smell in the three cases is identical), is possessed by ozone only. It is not the odour of phosphorus, nor of chlorine, nor of the oxides of nitrogen, although it is sometimes compared with these very dissimilar bodies. Concerning the odour of hydrogen peroxide, there appears to be wide diversity of opinion. Most of the works on chemistry, to which I have referred, state that it is odourless; one authority mentions that its odour is peculiar; another, that it resembles chlorine. The solutions which I have prepared at different times myself, carbonic acid being employed to decompose the barium peroxide, have not evolved any odour that I was able to recognise or perceive. But when air is forced for a few minutes through a phosphorus ozonator, such as is described in CHEMICAL NEWS, vol. xl., p. 246, the atmosphere of a large lecture room becomes insupportable from the diffusion throughout it of an invisible gas (not a white smoke, like the so-called antozone), and there is everywhere present the penetrating and irritating smell of ozone.

In the next place, ozone is very slightly soluble, and water, through which hydrogen has been passed at or below the common temperature, may be made to indicate the existence of dissolved ozone by appropriate tests.* It is certainly not too much to assume, that when water, through which ozone has been passed, is evaporated to one-tenth of its original bulk, its dissolved ozone is expelled. But under these circumstances 30 c.c. of a solution which originally contained 2.7 m.grms. of hydrogen peroxide, when evaporated down to 3 c.c., yielded on titration a result equivalent to 2.4 m.grms. of the peroxide. The loss was due either to the decomposition of 0.3 m.grm. of hydrogen peroxide, or to the expulsion of 0.44 m.grm. of dissolved ozone. The former is by far the more probable supposition, and in either case the body principally contained in the water was hydrogen peroxide. This water was contained in the first wash-bottle used in washing the ozone, generated by the action of phosphorus partly immersed in *aqua purissima*, upon air which had been previously purified from all pre-existent nitrogen compounds (ammonia, nitrous and nitric acids), organic particles, &c.† No nitrous acid was present in the water, as was shown by examination with Griess's test (meta-diamido-benzol), but it contained 0.49 m.grm. of nitric acid, and 0.066 m.grm. of ammonia. This determination was made upon 481 litres of air, the amount of ozone formed being 0.924 grm. The percentage of hydrogen peroxide in the first bottle of wash-water, as compared with the total weight of the air ozonised, was 0.00038 per cent.

In another trial, in which 420 litres of air were drawn over and 0.805 m.grm. ozone produced, the amount of hydrogen peroxide contained in the wash-water was 2.01 m.grms., or 0.00037 per cent of the total weight of the air aspirated. It contained also 0.075 m.grm. ammonia and 0.88 m.grm. nitric acid, but of nitrous acid yielded not a trace to meta-diamido-benzol. In a third experiment, in which 396.4 m.grms. of ozone were produced, the wash-water contained 0.37 m.grm. of nitric acid, no nitrous acid, and 1.3 m.grm. of hydrogen peroxide, corresponding to 0.00039 per cent of the total weight of the air ozonised. One word as to the close agreement in the percentages of hydrogen peroxide found in the wash-water employed in the various trials. It was an agreement not anticipated at the outset, and became evident only when the actual amounts of hydrogen peroxide, found in the course of experiments continued during the course of more than three

* Ber. der Deutsch. Chem. Gesell., xii., 1831.

† "Upon Ammonium Nitrite, and upon the By-products obtained in the Ozonation of Air by Moist Phosphorus," CHEMICAL NEWS, vol. xl., p. 70, by the Author.

* Ann. der Chem., vol. cxcvi., p. 58.

months, at their conclusion came to be calculated up as percentages of the total weights of the air used. The same agreement was found to exist between the amounts of *nitric acid* present in the wash-water, when their percentages were calculated in like manner. In the first trial, 27 litres of air being aspirated, the nitric acid was 0.000318 per cent of the total weight of the air; in the second trial, 108 litres of air aspirated, it was 0.000318 per cent; in the fourth, 85 litres aspirated, it was 0.000303 per cent; in the sixth, 262 litres aspirated, 0.000328 per cent. In these four experiments, water was used in the jars of the ozonator. In the third trial, 45 litres of air being drawn over, the nitric acid present in the wash-water was 0.00054 per cent; in the fifth trial, 90 litres aspirated, it was 0.00054 per cent. In these two trials, the bichromate mixture was used in the jars.

In another place (CHEMICAL NEWS, vol. xxxix., p. 157), I have shown that the amount of ozone produced by the oxidation of phosphorus, under given circumstances, is constant for any particular temperature, and consequently the generation of ozone may be graphically represented by a curve, the production at 6° C. being *nil*, rising to a maximum at 24° to 25° C., and then rapidly diminishing as the temperature approaches 44°, the melting-point of the phosphorus. The results above given warrant, it appears to me, the formulation of another proposition:—

2. In the oxidation of phosphorus in moist air there is formed both *ozone and hydrogen peroxide*.* The amounts of the two bodies evolved, under given circumstances, bear a constant relation to one another and to the air passed over, the percentage of hydrogen peroxide being very small as compared with that of the ozone. In the experiments quoted above, the temperature being 18° to 21° C., the percentage of ozone, as referred to the weight of air aspirated, was 0.15 per cent; that of hydrogen peroxide was 0.00038 per cent, or but 1-400th part of the ozone.

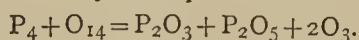
The amount of phosphoric acid in the wash-water, in the experiment in which 420 litres of air were aspirated, was 0.252 grm.; of phosphorous acid, 0.013 grm. It was determined by trial that the presence of phosphorous acid in the solution did not affect the titration of hydrogen peroxide. In this connection, it is likewise important to formulate the three following propositions:—

3. No nitrous acid is present among the final products. The presence of hydrogen peroxide is in itself a sufficient reason.

4. Both ammonia and nitric acid are present in the final products, the amounts found standing in a constant ratio to the quantity of air aspirated. In one trial the results agreed with those corresponding to the formula of ammonium nitrate.

5. The generation of ammonium nitrate and hydrogen peroxide is as invariable a feature of the phenomena observed during the course of the oxidation as is the production of phosphoric and phosphorous acids. For this reason any explanation of the phenomena which neglected the contemporaneous generation of ammonium nitrate and hydrogen peroxide would be imperfect.

The results and the above conclusions were arrived at and published in the early part of last year. The explanation brought forward at that time, and which as yet I have seen no reason to modify, was as follows†:—"As to the reason of the formation of ozone itself under these circumstances, it may be conjectured along with Lamont‡ and others, that it is connected with the uneven quantivalencies of the elements taking part in the reaction, which may be represented by the equation—



If this hypothesis be true, then we should anticipate the development of ozone whenever oxidation of a perissad

occurred at temperatures compatible with the stability of the ozone molecule. Even at the temperature of combustion of hydrogen, this is supposed by C. Than* to be the case. He explains in this manner the presence of the ozone, which he states he has detected in the combustion of hydrogenous substances generally, and its absence in the combustion of carbon."

"In entering into new combinations the oxygen molecules must undergo temporary resolution into their constituent atoms. These, while *en route* to take up new positions in other combinations, and animated by their atomic energy, or energy of the nascent state, may either oxidise the oxygen molecule, or the nitrogen molecule, or the molecule of water. In the first place, ozone would be produced; in the second, regarding water as the basic body and NNO as the nitryl, there might be formed, as Hunt as indicated,‡ ammonium nitrate; in the third, hydrogen peroxide."

Subsequent to the publication of these results, Prof. McLeod took up the subject, and arrived at the conclusion that‡ "the gas obtained during the slow oxidation of phosphorus, possesses the properties of ozone, and not those of hydroxyl, the only known peroxide of hydrogen." He likewise states that it is extremely improbable that ozone and hydrogen peroxide are both formed, as these substances destroy each other. The principal quantitative experiment mentioned was that of passing the gas, first through a tube heated to various temperatures, then into a weighed sulphuric acid tube, and finally into a solution of potassic iodide and starch acidified with sulphuric acid.

In connection with these experiments, it is to be noted in the first place, that Prof. McLeod regards the question in the light of an alternative, the presence of ozone excluding that of hydrogen peroxide, and *vice versa*. This view, though supported by the statements of Schönbein,|| that the two bodies mutually decompose one another, is much weakened by the recent elaborate experiments of Schöne,§ who shows that when strongly ozonised oxygen containing 5.2 volumes per cent of ozone, is agitated with an hydrogen peroxide solution, containing 0.4 per cent of the peroxide, or three to four times as much of the peroxide as is adequate to destroy all the ozone, it is only after the lapse of half an hour that as much as half of the ozone is destroyed.

Why, then, if hydrogen peroxide and ozone were both present, did not a gradual increase in weight in the sulphuric acid dryer, as the gas was heated at various temperatures up to 200°, indicate the progressive decomposition of the former? The failure to obtain such a serial progression is not explained. Neither is there any explanation of how an increase in weight in the cases where heat was applied is necessarily to indicate the presence of hydrogen peroxide, when an increase (and that a much larger one) took place when no heat was used. Moreover, ozone itself undergoes decomposition at a temperature of 200°. But at this temperature Prof. McLeod obtains by titration a result corresponding to 3.4 m.grms., which is relatively greater than the amount obtained when the ozone was not heated at all.

I shall be glad if Prof. McLeod will convict me of error if I am mistaken in stating that the decomposition of his potassium iodide solution under these circumstances was due not to ozone (remaining after the air had been heated to 200°), but to the presence of free acid. In the early part of my own studies upon the subject, I investigated as an essential question whether a current of ozonised air could be accurately titrated by an acid solution of potassium iodide, and determined that it could not.¶ Unless the presence of oxygen be rigorously excluded from the

* This statement is not new. Hydrogen peroxide was recognised by Schönbein among the by-products of the oxidation of phosphorus.

† *Journ. Amer. Chem. Soc.*, i., 156.

‡ CHEMICAL NEWS, vol. xxviii.,

* *Journ. Chem. Soc.* [2], vol. ix., 483; *J. Prakt. Chem.*, [2], i., 415.
† "Chemical and Geological Essays," T. S. Hunt, Second edition, p. 472.

‡ CHEMICAL NEWS, vol. xl., p. 307.

|| *Journ. für Prakt. Chem.*, vol. lxxvii., p. 130.

§ *Ann. der Chem.*, vol. cxcvii., p. 240.

¶ *Phil. Mag.*, April, 1879; *Journ. Amer. Chem. Soc.*, i., p. 18.

solution, excessively dilute acidified solutions of potassium iodide change even in the dark. In the light, the strength of the solution remaining the same, the change increases in the same ratio as the relative increase of the surface of exposure of the solution to the light's influence. But when oxygen is excluded, strong solutions containing excess of acid may be exposed to the *direct light of the sun for days* without undergoing decomposition. Hence, when a current of air is allowed to pass through an acidified solution of potassium iodide, it is placed under conditions most favourable to its decomposition, and the amounts of iodine liberated will depend, first, upon the relative amounts of acid and iodide present, and, second, upon the intensity of the light and the relative dimensions of the surface of exposure. I have been most careful *not* to acidify the potassium iodide solution during the course of the experiment, but at its close, and then, for the reasons above mentioned, to add only sufficient dilute acid to decompose the iodate which has been formed, and titrate at once.

In conclusion, I wish briefly to summarise the arguments above stated. The gaseous body given off in the course of the reaction between moist phosphorus and air, has a very powerful and peculiar odour, which is identical with that produced in the electrolysis of acidulated water. This is not the case with hydrogen peroxide. Ozone is very slightly soluble in water, and is readily expelled from the solution on heating. Hydrogen peroxide is miscible in all proportions with water, and solutions containing as much as 1 per cent H_2O_2 may be concentrated by evaporation on the water-bath, until a higher degree of concentration is reached, without great loss of peroxide. (See also Schöne, *Ann. der Chem.*, 196, p. 60, and Davis, *CHEMICAL NEWS*, vol. xxxix., p. 221.) Wash-water, through which air ozonised by phosphorus has been passed, after concentration yields a reaction due to hydrogen peroxide. The absence of nitrous acid in such wash-water can be readily demonstrated. No statement has been made as to the relative amounts of ozone and hydrogen peroxide *generated*, but only as to the relative amounts *evolved*. The latter amounts are those which remain after the highly dilute atmospheres of hydrogen peroxide and ozone have operated upon each other in the ozonising chamber. The absolute amounts depend upon the temperature, the period during which the ozone and hydrogen remain in contact with one another, the rate of flow of gas, &c., but under given circumstances the ratio of the hydrogen peroxide in the *evolved* gas to the ozone, is a definite quantity. In the experiments detailed, as determined upon the first portion of wash-water, it was approximately the 1-400th part.

It must be borne in mind that this number does not represent the total amount of hydrogen peroxide, but only that dissolved in the first wash-bottle. This is the larger amount, but some is likewise present in the second wash-bottle, still less in the third, and so on. A portion escapes solution in the water, even when a number of wash-bottles are used, and finally passes into the potassium iodide employed to titrate the ozone. During the whole of this interval, the hydrogen peroxide and ozone are slowly decomposing one another, the results obtained, on titrating with potassium iodide solution, depending on the point in the series where the titration is effected. When the current of ozonised air issues again from the potassium iodide solution, it is laden with a white cloud. This white cloud is the antozone of Schönbein and other authors. It is, in fact, hydrogen peroxide, held in a state of aqueous suspension, and *is the product of the action of the ozone upon the potassium iodide solution*. As such, it is to be discriminated from the white cloud, which is to be seen in the first two or three wash-bottles placed directly in connection with the ozonising chambers of the phosphorus ozonator. This latter white cloud is likewise suspended hydrogen peroxide, but its origin is to be sought for in the same series of changes as that resulting in the production of the ozone itself.

It follows from the above, that the numbers obtained as

the results of titrating the stream of ozone and hydrogen peroxide with potassium iodide, do not represent ozone only. They represent also the relatively minute amounts of hydrogen peroxide, which, as I have shown in the paper published in the early part of the preceding year and previously alluded to, always accompany the ozone generated by the action of moist air upon phosphorus.

Finally, I desire to mention in explanation of certain statements made in the latter part of the article, that they are founded upon experiments as not yet published, and that I have only been induced to make this preliminary notice of them, in order to complete an argument against what have appeared to me to be erroneous views.

NOTES ON SOLUBILITY.*

By D. B. DOTT.

BEFORE proceeding to the matters more particularly pertaining to this paper, it seems desirable to say a few words on the general subject of solution.

Dr. Watts, in his "Dictionary of Chemistry," defines solution as "the liquefaction of a solid or gaseous body by contact with a liquid, the solid or gas being diffused uniformly through the liquid and not separating when left at rest." In the same article we read—"The product formed by the union of liquids one with the other is usually called a *mixture* if the two liquids are capable of uniting in any proportions whatever, and a solution when each of the liquids is capable of taking up only a limited quantity of the other." The definition is certainly incomplete without taking the last quotation as a qualification, for a liquid manifestly dissolves in another liquid in the same manner as does a solid or a gas, showing that there is no preliminary or accompanying liquefaction. I cannot leave these quotations without condemning what has always appeared to me a somewhat meaningless distinction between *solubility* and *miscibility*. It is customary to say, for instance, that ether dissolves a small proportion of water, but that ether and alcohol are miscible in all proportions. Is the difference here anything more than one of degree? When it is said that two liquids are perfectly miscible, is it not just another way of saying that each is capable of dissolving its own volume of the other? Moreover, the term *miscibility* is occasionally applied to that intimate mixture of liquids which is sometimes observed—as, for example, chloroform with glycerin, where the two liquids mix thoroughly so as to simulate solution, but after some time slowly separate. Indeed, this appears the more appropriate use of the expression.

I have read with interest in the *CHEMICAL NEWS* (vol. xxxvii., p. 47) a paper by William Durham "On Suspension and Solution." After describing a number of experiments founded on such facts as the precipitation by sulphuric acid of clay suspended in water, which phenomenon is explained by the greater attraction of water for sulphuric acid than for clay, he concludes as follows:—"There seems to be a regular gradation of chemical attraction from that exhibited by the suspension of clay in water up to that exhibited in the attraction of sulphuric acid for water, which we call chemical affinity. Chemical combination, solution, and suspension differ only in degree, and are manifestations of the same force." Although, doubtless, these conclusions would not generally be admitted, I believe they are fundamentally true. Just as we have a gradation from a perfect gas through intermediate stages to a liquid, and from a liquid through various degrees of viscosity to a solid, so we may say that there is a gradual increase of force from mere adhesion up to the most unmistakable instance of chemical affinity. For the sake of convenience, however, there is no harm in distinguishing

* A condensed report of a paper read before the Edinburgh University Chemical Society, February 11, 1880.

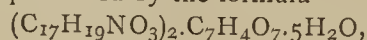
two classes of material phenomena, the physical and the chemical, and there should be no hesitation in referring solution to the former. It is true there are one or two facts which give some support to the chemical view of solution; such as the precipitation by water of a resin dissolved in alcohol, as likewise the instance already referred to in Mr. Durham's experiments. From the same point of view the solution of a salt in water may be regarded as a combination of the salt with water to form a fluid hydrate, which then diffuses through the liquid. On the other hand, combination in definite and multiple proportion, and other characteristics of chemical action which it would be superfluous to recite, have no place in solution, so that we are compelled to place solution among the purely physical phenomena.

Perhaps the best definition of solution I have seen given is that by Dr. Isidor Walz (*Pharm. Journ.*, [3], vi., p. 8), who gives the following expression:—"Solution is the penetration of the molecules of one or more substances into the inter-molecular spaces of another substance, and the consequent formation of a homogeneous resultant." My only objection to this definition is that it includes among solutions alloys and hydrates, as well as mixtures of gases, while it is unusual and confusing to speak of solution except in reference to liquids. The definition I have devised, although no doubt open to objection, better expresses, I think, the nature of the process than the definitions sometimes given:—

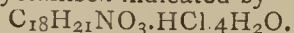
Solution consists in the Molecular Mixing of Distinct Liquids.

1. The degree of solubility extends from zero to infinity, and depends upon the nature of the liquids and upon the temperature and pressure.
2. When a solution under given conditions is incapable of dissolving more of one of its constituents it is said to be saturated as regards that constituent.
3. In the case of solids and gases, solution is preceded or accompanied by liquefaction, which is brought about by contact of the liquid with the solid or gas, the resulting solution not being capable of liquefying a substance after it has attained its point of saturation for the same.

We come now to those instances of solubility which suggested to me the writing of this paper. So far as I am aware it is not generally known that many alkaloidal salts are much more soluble when in the amorphous anhydrous state than when in the crystalline hydrated form. The salts to which my attention has been more particularly directed are the meconate of morphia and the hydrochloride of beberia. The neutral meconate of morphia crystallises from water in prismatic needles, having, when air-dry, the composition represented by the formula—



and when dried in the water-bath the five molecules of water are eliminated. Beberia hydrochloride has the composition when crystallised indicated by—



In dry air three of these molecules are given off, but the last is not lost under a temperature of about 120°C . As regards the solubility of these salts I have had some difficulty in coming to an exact conclusion. Storer distinguishes the two recognised methods of determining solubilities as the "method by digestion" and the "method of cooling." In the former of these the solvent has added to it an excess of the substance whose solubility is to be determined, the temperature being kept as nearly constant as possible, and the solution is agitated from time to time. If at two of these intervals the proportion of dissolved substance is found to be the same the solution may be considered saturated, and the results taken as correct. In the "method of cooling" a saturated solution is prepared at a certain temperature, and the solution is then allowed to cool to the temperature at which it is desired to determine the solubility. After remaining a sufficient time at

that temperature, the amount of dissolved substance is estimated in the usual way. The question arises, What is a sufficient time to allow the solution to stand at the temperature finally attained before making the determination? Gay-Lussac in his experiments found that two hours were enough, and I observe that more recently (*Chem. Soc. Journ.*, 1876) Victor Meyer recommends the same time. Storer, is of opinion that wherever practicable the "method by digestion" ought to be followed, and that the employment of the "method of cooling" is principally responsible for the striking discrepancies in nearly all accounts of this class of constants, the source of error being the tendency of many substances to an indeterminate supersaturation when cooled from a higher to a lower temperature. If a sufficient length of time were allowed the results obtained by both methods would probably ultimately agree, but no adequate allowance has been made for this tendency to "supersaturation." Two hours may be a sufficient interval for some solutions, but three days are not sufficient for others, as the following figures will show:—A saturated solution of morphia meconate was prepared by "method of cooling." After remaining for two or three hours at 10° 278.96 grs. H_2O contained 28.95 grs. of the crystallised salt, or 1 part is soluble in 9.6 parts H_2O . After three days, 178.63 grs. H_2O contained 5.94 grs. salt, or 1 part is soluble in 30.0 parts H_2O at 10° . Of the same solution after ten days ($t = 11^\circ$) 108.57 grs. H_2O contained 3.13 grs. of the salt, or 1 part is soluble in 34.6 parts H_2O . Similarly with a solution of morphia neutral tartrate. On the first day the proportion dissolved was 1 in 5.0; on the second day, 1 in 6.2; on the fourth day, 1 in 11.0; the temperature varying slightly on these occasions, but not in such a way as to account for the divergence in the results. What I wish particularly to call attention to, however, is the great difference in the solubility of these salts when crystalline and hydrated and when amorphous and anhydrous, as I have no doubt the tendency to "supersaturation" depends on the same cause. The solubility of morphia meconate (5-hydrate) was determined by "method by digestion" ($t = 12^\circ$). 54.93 grs. H_2O dissolved 2.098 grs. of the crystallised salt, or 1 part is soluble in 26.1 parts H_2O . 94.45 grs. H_2O dissolved 3.592 grs. of the salt, or 1 part is soluble in 26.2 parts H_2O . We may therefore take the solubility of di-morphia meconate penta-hydrate as 1 in 26 of water at 12° . Of the anhydrous salt, 100 grs. of H_2O dissolved 45 grs. at 11° , or 1 part is soluble in 2.2 parts water. This result must only be taken as giving the minimum solubility, as it is impossible to digest a solution with excess of the hydrate for any length of time without crystals of the 5-hydrate rapidly forming. I have not so carefully ascertained the solubility of beberia hydrochloride, but 1 part of the crystallised salt is soluble in about 30 parts of H_2O at 18° . On the other hand, the amorphous salt, whether dried in the water-bath or by exposure to the air (in which latter case it contains very nearly the proportion of water for the 4-hydrate), is soluble in water to almost any extent.

The question I have put to myself is this—What is the explanation of these salts being so much more soluble in the anhydrous amorphous state than when in the hydrated crystalline state, and what is the cause of their tendency to form "supersaturated" solutions? Perhaps the explanation which most readily suggests itself is that the anhydrate and hydrate being distinct compounds, the former is more soluble than the latter, and that when a hot solution of the hydrate is prepared the hydrate decomposes into the anhydrous salt and water, while again, on the solution cooling, the anhydrate slowly re-combines with water to form the less soluble hydrate. This theory, if it agreed with all the facts, would be quite satisfactory: but there is at least one phenomenon which tends to disprove it. I refer to the avidity with which moisture is absorbed by some of these amorphous salts, notably by morphia meconate. Portions of this salt, prepared by evaporating a solution to dryness on the water-bath, were placed under bell-glass with a beaker of water. Moisture was rapidly

absorbed, the substance becoming sensibly wet to appearance and to the touch. On weighing these portions at intervals of twelve hours the maximum gain observed was 29.6 per cent. The weight then decreases pretty rapidly until the absorbed water is = 13.6, and then the salt gradually dries up to the 5-hydrate. The percentage 13.6 agrees pretty well with that required for a 6-hydrate, 14.02, 11.68 being the increase required for the known 5-hydrate. These results rather militate against the theory of such salts dissolving as anhydrides. One would naturally expect that when a salt has a great affinity for water it would combine with the water when thrown into it previous to dissolving. Another view which may be taken of the phenomena under discussion is the assumed existence in solution of different hydrates of varying degrees of solubility. As is well known, this is the explanation usually given of the formation of supersaturated solutions of sodic sulphate, &c. We can imagine that when the anhydrous salt is added to water it immediately combines to form its highest hydrate, but that the least soluble hydrate gradually separates according to the same law by which the least soluble salt separates in a case of double decomposition. This explanation is still open to objection. If the least soluble hydrate has a strong tendency to form out, why does it not do so at once? Now it is evident that the conclusive point to determine is whether the high degree of solubility adverted to is a function of the anhydrous state or of the amorphous state. It has often been noticed that alkaloids when recently precipitated and in the amorphous condition are more soluble than when after standing some time they have become crystalline. It is easy to prepare amorphous hydrates of morphia meconate and beberia hydrochloride, having apparently the same composition as the described crystalline hydrates, and their non-crystalline salts are extremely soluble. More decisive than any of these is the experiment next described. If morphia meconate 5-hydrate be dried in the water-bath it still retains its crystalline appearance, unlike the anhydrate prepared as formerly described. Let it now be digested with water, and it is found to be no more soluble than the ordinary crystallised salt.

It must often have been observed that the presence of small quantities of colouring matters and the like greatly interferes with the separation of organic salts and crystalline principles from their solutions. That is to say, that a hot solution of such a strength that on cooling it becomes filled with crystals, will cool down to the same temperature without yielding any crystals if only certain impurities are present, the substance slowly crystallising out after some time, and this with compounds which are not known to form hydrates. It is sometimes remarked of such instances that the substance is more soluble in a solvent containing colouring matters than in the pure menstruum: but this is misleading if not quite incorrect, seeing that the colouring matters are present after the crystals have appeared, just as they were at first. In some cases mere traces of such colouring matters almost prevent a salt crystallising.

Having given a considerable amount of attention to this class of phenomena, I have come to the conclusion that these are genuine instances of "supersaturation": that is to say, that these compounds are not rendered more soluble by the presence of colouring matters, in the same way as calcic sulphate is rendered more soluble in water by the addition of hydrochloric acid; but that the colouring matters in some manner interfere with the formation of crystals, and favour the condition of "supersaturation." This depends, I believe, on the capacity of dissolved substances for existing in two distinct states, what we might perhaps call the *potentially amorphous* and the *potentially crystalline*, corresponding to the amorphous and crystalline conditions of solid matter. Assuming the possibility of this, we can then readily believe that colloidal colouring matters, being themselves incapable of crystallising, will tend to retain other dissolved substances in the same state as themselves, and thus retard the formation of crystals.

This theory of a twofold liquid state may seem a very large conclusion from meagre premises, but I feel convinced that some such theory is requisite, and that other phenomena besides those referred to will be found to favour this view of the matter. If it be true, for instance, as has been stated, that if the same salt solutions prepared in different ways will dialyse with very varying degrees of rapidity, it would seem that the same salt can exist in two liquid states.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Anniversary Meeting, March 30, 1880.

MR. WARREN DE LA RUE, President, in the Chair.

THE PRESIDENT, in his annual address, contrasted the condition of the Society during the past year with its position in 1869, when he also was President. The number of Fellows has increased from 522 to 1034; the income from £1122 to £2720; the number of papers read from 31 to 75. The *Journal* has also increased both in value and importance, especially in the addition of the abstracts, which form such a complete compendium of the progress of chemistry. As regards the library, £500 have been spent in purchasing new books, and a card catalogue is being prepared to increase the facility for finding books. The President then referred to the rapid progress of chemistry which he had specially noticed on returning to the chair: much of the aspect of chemical thought has changed. The elements once looked upon as most stable are now considered to be at any moment liable to be dissociated. Some months back spectroscopic evidence was brought forward which tended to show that the so-called elements were in reality compound bodies, and now V. Meyer has succeeded in dissociating chlorine, bromine, and iodine. As regards the spectrum itself, specific functions and properties can no longer be attributed to various parts of it; for Captain Abney has shown that every part acts actinically, and holds out a prospect of producing permanent photographs of the spectrum in its natural colours. The artificial production of the diamond is also said to have been effected, but Mr. Hannay's communication on this subject is so vague that it is impossible to pronounce any opinion upon it. The joint observations of Hannay and Hogarth on the solubility of solids in gases promise results of great interest. Remarkable observations have been made by Cailletet, Ansdell, &c., on the behaviour of various substances, mixtures, &c., under great pressure. The researches of Gladstone, Landolt, and others, on the refractive indices of carbon compounds, have been greatly extended by Brühl. The President then alluded briefly to the great diligence of organic chemists in the investigation of carbon compounds, and especially to the elucidation of the constitution of alkaloids and the carbohydrates. The synthesis of isatin and Baeyer's researches in the indigo group must, it would seem, result ere long in the discovery of a method for the artificial manufacture of indigo. Ladenburg has prepared atropine from tropine and tropic acid, and thus probably the first steps have been taken towards the synthesis of an alkaloid. Much light has been thrown on the constitution of the bases of the pyridin and picolin series, of the nicotin and cinchona alkaloids and of starch. Recent facts seem to show that our present symbolic system is inadequate to represent the constitution of carbon compounds. A new element, scandium, has been separated, corresponding to Mendelejeff's ekaboron, thus verifying his sagacity and the importance of the "Periodic Law." The President then referred to the objects and position of the Research Fund, and expressed a hope that all whose fortune allowed

would contribute largely to this fund, and thus promote the advancement of a science which may have contributed greatly to their own prosperity. He thanked the Drapers and the Goldsmiths' Companies for their donations to the fund. During the past year death has removed fourteen Fellows.

At the conclusion of the address,

Mr. F. A. ABEL proposed a vote of thanks to the President for his interesting address and for the zeal with which he had performed the duties of President during the past year. It had been a special pleasure to see Mr. De la Rue again in the Chair. Some of the older members would recal with gratitude the great exertions which had been made by Mr. De la Rue on behalf of the Society at a critical period of its history, when he was appointed Treasurer, and the present prosperous condition of the Society was in a great measure due to his efforts at that time. He hoped that the example set for a second time by the President would prove an incentive to others to subscribe liberally to the Research Fund.

Dr. GLADSTONE, in seconding the vote of thanks, said that he had listened with great interest to the successful *resumé* of chemical progress given by the President. The experiment of re-electing a former President for twelve months had proved a great success, and he hoped it would form a precedent. The small attendance was an indication not only of the holiday season but also of the peaceful prosperity which now reigned in the Society.

The PRESIDENT, in reply to the vote of thanks, said that as an original member he had taken great interest in the Society, and had been very glad to take up the management of the finances. The prosperous career of the Society was, however, due more to its own innate life than to any efforts of his. He had returned to the chair with some sense of his own inefficiency, owing to the enormous changes which had recently taken place in chemistry. He thanked the Council, Officers, and the Fellows of the Society for the kind way in which they had supported him during his year of office.

The SECRETARY read the Third Report of the Research Fund Committee. Grants to the amount of £495 have been made during the year. The assets consist of £4000 stock, and a balance at the bankers of £152. A detailed list of the grants is given. A donation of £105 from the Drapers' Company and £100 from the President form important items in the income. The Committee point out the desirability of obtaining further additions to the fund, as without such contributions the income from investments would have been quite inadequate. Results of investigations have been published by Dr. Tilden, Prof. Thorp, Dr. Wright, Mr. F. D. Brown, Dr. Armstrong, Dr. Bedson, Messrs. Hartley and Huntington. Dr. Japp has forwarded a paper, which will be read at the next meeting. Preliminary reports have been received from Dr. Dupré, and Messrs. Bolas, Burghardt, Jago, Shenstone, and Williams.

The SECRETARY then read the Treasurer's Report. The expenses on account of the *Journal* amount to £1870; on account of the Library, £607; house expenses, £191; various other items, including £720 for purchase of stock and a balance of £929 at the bank, make up a total of £4684. On the other side are—Balance at the bank in March last, £1964; subscriptions, &c., £2151; sale of *Journals*, £295; dividends, £217. The assets consist of £6988 Stock; balance at the bank, £927; cash in hand, £2.

Votes of thanks were then passed to the Treasurer, the Auditors, the Officers and Council, and the Editor, Sub-Editors, and Abstractors of the *Journal*.

A ballot was then held for the election of Officers and Council, Messrs. Grosjean and Kingzett being appointed scrutators. The following were elected:—

President—H. E. Roscoe.

Vice-Presidents—F. A. Abel, C. B., B. C. Brodie, Warren De la Rue, E. Frankland, J. H. Gladstone, A. W. Hofmann, W. Odling, Lyon Playfair, A. W. Williamson, J. Dewar,

J. H. Gilbert, N. S. Maskelyne, V. Harcourt, R. Angus Smith, J. Young.

Secretaries—W. H. Perkin, H. E. Armstrong.

Foreign Secretary—Hugo Müller.

Treasurer—W. J. Russell.

Other Members of the Council—M. Carteighe, C. Graham, C. W. Heaton, H. McLeod, E. J. Mills, J. M. Thomson, W. C. Roberts, W. A. Tilden, W. Thorp, T. E. Thorpe, J. L. Thudichum, R. Warington.

A Special Meeting was then held, at which the bye-law relating to the number of members required to be present for the election of Fellows was altered, the number 32 being substituted (Bye-laws, page 12, line 4) for 40.

Thursday, April 1, 1880.

Prof. H. E. Roscoe, President, in the Chair.

AFTER a few remarks from the President on taking the Chair for the first time, the following certificate was read for the first time:—J. Taylor.

The PRESIDENT then called upon Mr. GROVES to read a paper on "*Betorcinol and some of its Derivatives*," by J. STENHOUSE and C. E. GROVES. Some thirty-two years ago, in the course of an examination of lichens, a quantity of crude usnic acid from *Usnea barbata* and *Cladonia rangiferina* was submitted to destructive distillation. In the distillate a crystalline substance was found, of the composition $C_8H_{10}O_2$, homologous with and closely resembling orcinol; it was therefore named β -orcin. It was somewhat hastily inferred that the usnic acid in the above lichens was the source of the β -orcin. Subsequent attempts to prepare β -orcin by distilling pure usnic acid, however, completely failed. A quantity of *Usnea barbata* was therefore obtained, and the crude usnic acid extracted and submitted to destructive distillation. On adding to the distillate solution of chloride of lime a crimson-red colouration proved at once the presence of the orcinol-yielding body. On purifying the usnic acid no trace of the orcinol was obtained after destructive distillation. The source of the β -orcin—or, as the authors now name it, betorcinol—was therefore not usnic acid. To prepare Betorcinol, $C_8H_{10}O_2$ or $C_6H_2Me_2(HO)_2$:—*Usnea barbata*, carefully freed from other lichens, is exhausted with milk of lime, the solution precipitated by hydrochloric acid, and the precipitate boiled with lime and water. The clear solution is neutralised with hydrochloric acid and evaporated, when the crude betorcinol crystallises out. It is purified by crystallisation from benzene and from water; melts at $163^\circ C$.; it is less soluble than orcinol; gives a bright crimson colour with hypochlorites, orcinol a purplish red; the ammoniacal solution is rapidly coloured on exposure to air, whilst the corresponding orcinol solution is but slowly changed. Tetra-chloro-betorcinol, dichloro-betorcinol, the corresponding bromine derivatives, and a mono-nitro compound were prepared and examined. The acid which yields the betorcinol was separated after some trouble from the usnic acid by taking advantage of its greater solubility in cold ether. It melts at 186° , and decomposes at a somewhat higher temperature, carbonic anhydride and betorcinol being formed. It has the formula $C_{19}H_{20}O_7$, having the same relation to betorcinol that evernic acid, $C_{17}H_{16}O_7$, has to orcinol. As, however, it cannot be at present definitely asserted that it is a dimethyl-evernic acid, the authors have provisionally named it *barbatic acid*. Hesse obtained an acid from an usnea growing on *Calisaya* bark, melting at 172° , which he termed usnetic acid. This is probably not identical with barbatic acid, but the authors suggest that it may be an ethereal salt of barbatic acid, as Hesse used alcohol in extracting and purifying his substance.

The PRESIDENT, after complimenting the authors on the completeness and clearness of their communication, called on the SECRETARY to read a paper. "*Note on Chemical Equilibrium*," by M. M. P. MUIR. The object of this

paper is to describe a few measurements of the variation caused in chemical changes by modifications in the conditions of these changes, and to attempt to generalise some of the conditions of chemical equilibrium looking at the phenomena from a dynamical point of view. The influence of the manner of mixing on the changes which occur when potassium permanganate, oxalic and sulphuric acids, react in the presence of sulphate of manganese is illustrated by numerical results, showing that during the earlier part of the reaction the velocity of the chemical change is greater when permanganate is slowly added, and the liquids are not thoroughly mixed until after three minutes. The disturbing influence of a secondary change upon the primary is illustrated by a few numbers representing the relative velocities of the solution of ferric oxide by sulphuric acid, alone, in the presence of zinc, and when aided by the passage of an electric current. In the two latter cases the velocity of the change is considerably greater than in the former. The nascent hydrogen appears to decompose the product of the primary change—ferric sulphate—and to set free sulphuric acid in contact with ferric oxide, the latter being rapidly acted upon. Attention is drawn to the work of Prof. Willard Gibbs, wherein the conditions of equilibrium of a system of heterogeneous substances are deduced from a consideration of the great principles of the conservation and dissipation of energy, and a rough attempt is made to connect the so-called chemical induction with the commonly occurring phenomena of chemical action, and to trace analogies between chemical and electrostatic induction. The general conclusions arrived at are that chemical changes are always complex; that each change consists of at least two parts, a direct and a reverse change; that during the progress of either of these, secondary changes may be induced, which will in their turn modify the primary changes; that systems not in phases of absolute stability tend to undergo changes by contact with very minute quantities of matter in phases other than their own; that such systems will always tend to pass into that phase, the passage to which is attended with the maximum loss of Entropy; but that this tendency may to a certain extent be prevented by the action of impressed force.

Dr. ARMSTRONG thought that it would have been desirable to make a series of observations and take the mean results. Apparently only single observations had been made, and the slight differences observed might to some extent be due to experimental errors.

The PRESIDENT commented on the difficult nature of such investigations, but agreed with Dr. Armstrong that it would have been better to have made a series of observations.

The SECRETARY then read a communication from Japan, entitled "*Preliminary note on the Action of the New Diastase, Eurotin, on Starch*," by R. W. ATKINSON, Professor of Chemistry at Tôkiô. Korschett described in the *Transactions* of the German Asiatic Society, in some detail, the Japanese brewing process (*Dingler's Polyt. Journ.*, 230, 76), and mentioned the occurrence of a soluble ferment, which he named Eurotin, in the fermenting material employed, having the property of dissolving starch and converting it into sugar. A brief description of the process was given by the author in *Nature*, 1878. The fermenting body is termed Koji and is prepared as follows:—Washed rice is soaked in water till soft; it is then steamed for some hours until the starch has gelatinised. When lukewarm the mass is sprinkled with spores of the fungus, *Eurotium oryzae*. The grains are then well mixed and exposed in trays to a temperature of about 25° C. In

three days the mass is cemented together by the silky filaments of the mycelium and forms the "koji," which is used instead of malt in the brewery process. When extracted with water, koji yields a solution, reducing the Fehling test. When digested with water for about ten minutes the solution gives about 12 to 14 per cent of glucose. Alcohol also dissolves out sugar, so that probably some sugar is present in the substance before treatment with water. Korschett has shown that the cold aqueous extract of koji has properties resembling those of malt, and when added to gelatinised starch renders it limpid and forms sugar; he also concluded that the temperature of 45° to 50° was the most favourable to this change. The author of the present paper has examined more minutely into the chemical reactions of the brewing processes of the Japanese, and has come to the conclusion that the starch breaks up in this process into glucose and dextrin instead of maltose and dextrin, as in our mashing operations. Korschett assumed that maltose was formed, but gives no estimations of sugar or polariscopic determinations. The author then describes the process and gives analyses of the mash on different days. A mixture of koji, steamed rice, and water is made in the cold. On the 5th to 7th days, the mash is warmed by introducing tubs filled with hot water. The principal points in the analyses are given in the table below.

It is to be noted that between the 14th and 17th days, two further additions of steamed rice and water were made; and, again, on the 18th day a fourth addition of steamed rice ferment and water took place. The mash was then pressed, and during the interval the small quantity of dextrin underwent fermentation, and the specific rotatory power of the filtered liquid became almost *nil*. The filtered liquid (Saké), which contains the water used to rinse out the fermenting tuns, contains 11.14 per cent alcohol; glycerin and resin, 1.99; fixed acid, 0.13; volatile acid, 0.02; water, 86.72. The fermentation is probably spontaneous. The size of the ferment cells is a little less than that of the beer yeast. The author endeavoured to obtain the sugar formed by solution in alcohol, but no crystallisable sugar separated. Experiments made by mixing cold extract of "koji" with gelatinised starch invariably gave a rotatory power lower than that calculated from the amount of dextrose and dextrin, assuming only these two substances to be present. The author leaves the detailed consideration of these reactions for a future communication. At present he is satisfied to have shown that the diastase of "koji," unlike that of malt, yields glucose and dextrin when it acts on gelatinised starch.

Dr. ARMSTRONG said that the paper was very suggestive and interesting, but he was sorry that more details of the methods and especially of the polariscope used were not given. He must differ from the author, and did not think that he had proved that glucose was formed. The author seemed to think that the formation of dextrose and dextrin was due exclusively to the eurotin, an unorganised ferment resembling diastase, and to neglect the action of the organisms present, which are, as is well known, capable of converting starch into dextrose.

Mr. GROVES called attention to the large amount of alcohol present, and suggested that it might be due to the particular organisms present.

The SECRETARY then read a "*Note on the Products of Combustion of Coal-Gas*," by L. T. WRIGHT. Some years ago the author was much struck by the powerful odour, suggestive of ozone, which he noticed when coal-gas was consumed in a Bunsen burner in an atmosphere charged with the nitrogen oxides evolved from fuming nitric acid.

Per cent.	1st day.	3rd.	5th.	7th.	10th.	14th.	17th.	19th.	24th.	28th.
Alcohol	—	—	—	5.2	8.61	9.20	5.80	9.44	12.41	13.23
Dextrose	—	7.35	12.25	5.4	0.99	0.50	2.06	1.16	0.27	0.0
Dextrin	—	5.12	5.69	7.0	2.81	2.57	3.89	2.74	0.47	0.41
Starch and cellulose	38.2	26.5	21.49	15.64	15.67	16.15	19.25	132.6	10.22	8.69
Rotatory power ..	—	124°	106°	135°	100.7°	116°	160°	132.3°	48.2°	36°
Sp. gr. of mash ..	—	1.15	1.18	1.08	1.05	1.04	1.03	1.02	0.99	0.98

During some experiments on the estimation of sulphur in gas the author passed the products of combustion of a Bunsen burner through solutions of iodine, and noticed that after the iodine solution had been bleached, and the current of gas stopped, free iodine appeared after some time in the solution. This reaction was attributed to nitrous acid, and was completely prevented by adding sodium bicarbonate to the iodine solution; the sulphur could then be determined by burning half a cubic foot of gas. In 1877 the author proved that nitrous acid was produced by the combustion of coal-gas in ordinary air, but that if the gas and the air were both freed from ammonia no nitrous acid was formed (*Chem. Soc. Journ.*, 1879, *Trans.* 42); the futility of the ordinary methods of washing gases was also proved. The author has never had the least difficulty in obtaining a blue reaction with paper moistened with iodide of potassium and starch, when coal-gas is burning in a Bunsen burner. Since reading Mr. Ridout's paper on the production of ozone, read before the Society a few weeks back, further experiments have been made, and the author concludes that ozone is not formed by the combustion of coal-gas, and that while ordinary gas burned in air usually gives the blue colour with starch and iodide of potassium, no such reaction is obtained when ammonia and its compounds have been completely removed previous to combustion.

The SECRETARY then read a paper "On Polysulphides of Sodium," by H. CHAPMAN JONES. These bodies were usually prepared by the direct union of sodium and sulphur. The author establishes the existence of Na_2S_5 , which on heating gives off sulphur, and leaves Na_2S_4 , it being extremely difficult to obtain the substances pure. The body Na_2S_5 seems to be the highest sulphide procurable; in a body containing more sulphur than the penta-sulphide, the excess of sulphur seems to be merely dissolved. An aqueous solution of Na_2S_5 , when gently warmed, evolves sulphuretted hydrogen; the penta-sulphide cannot therefore be satisfactorily prepared by boiling a lower sulphide with excess of sulphur: on boiling the above solution no sulphur is precipitated, $\text{Na}_2\text{S}_2\text{O}_3$ and SH_2 being formed. The author has examined the action of cadmium carbonate and other cadmium salts on the penta-sulphide, and concludes that the precipitate consists of cadmium sulphide and sulphur, the latter being removable by carbon disulphide. The penta-sulphide is believed by the author to be a tetra-thio-sulphate: he hopes to be able to prepare the di- and tri-thio-sulphates, and has obtained on several occasions a substance (giving a dark green solution having an absorption-band) which may be one of these bodies. In conclusion the author gives some properties of a black residue which is uniformly left when sulphur is burned, but has not examined the substance, as Dr. T. Cross (*Berl. Ber.*, 1879, 788) is already investigating the subject.

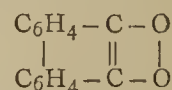
The next paper was read by the SECRETARY. "On the Reflection from Copper, and on the Colorimetric Estimation of Copper by means of the Reflection Cuprimeter," by T. BAYLEY. The author has shown that the light reflected from metallic copper contains all the elements of white light, but that the region of the spectrum to the red side of the D line is more intense than in the spectrum of the reflection from a white surface of equal illumination; the light transmitted by dilute solutions of cupric salts is deficient in those rays which the spectrum of reflection has in excess. It follows that if we look at a copper surface through a sufficient thickness of cupric sulphate solution, the metal appears silver-white, for the solution absorbs the excessive rays which make the copper red. Upon these facts the construction of the "reflection cuprimeter" is based. The direct light from the sky is reflected from a mirror of copper through two vertical tubes, closed at the bottom by plates of glass. The sheaves of light were concentrated by lenses on two layers of tissue-paper moistened with glycerin. The level of the liquid in the tubes can be adjusted by means of an aspirator. A standard solution of copper is made by dissolving 1 grm.

of copper in nitric acid, adding excess of sulphurous acid, and diluting to a litre. It was found that a depth of 8.01 centimetres of this solution gave the silver-white tint to the copper reflector. Iron does not interfere if reduced to the ferrous state. The degree of accuracy is shown by analyses. The apparatus is made by Messrs. Jackson, 65, Barbican.

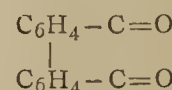
The next paper was "On Pyrene," by WATSON SMITH and G. W. DAVIES. The crude material was obtained from Dr. Schuchardt, and consisted of a dark brown crystalline mass. This was purified by solution in petroleum spirit and crystallisation. Crystals were light yellow; melted at 149° . A drawing of the crystals and a determination of the angles of the faces are given. The crystals are monoclinic. Analysis gave the formula $\text{C}_{16}\text{H}_{10}$. Vapour-density was found to be 6.912, calculated 6.999. The authors used Victor Meyer's method and a lead bath. They recommend smoking the glass, dipping into the melted lead in a luminous gas-flame: this prevents cracking and the adherence of the lead.

"Analysis of the Ash of the Wood of Two Varieties of the Eucalyptus," by WATSON SMITH. The two varieties were the red and blue gum trees, *E. rostrata* and *E. globulus*. The wood is remarkably hard, and gives rise to a powerful aromatic odour when planed. Specific gravity of *E. rostrata* is 0.8112, of *E. globulus* 0.752. Ash, respectively, 2.25 and 2.01 per cent; K_2O and Na_2O , 12.9 and 25 per cent; CaO , 43.8 per cent; 35.08 per cent, with small quantities of iron, alumina, &c. The author suggests the use of a decoction of the leaves of *E. globulus* as tea.

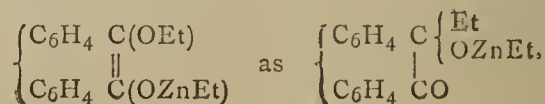
"On the Action of Organo-zinc Compounds on Quinons (Second Notice)," by F. R. JAPP. In a previous communication the author described a compound of the formula $\text{C}_{16}\text{H}_{14}\text{O}_2\text{C}_2\text{H}_6\text{O}$, but failed to isolate the substance $\text{C}_{16}\text{H}_{14}\text{O}_2$. In the present paper this body has been isolated, and its more important reactions studied. The alcohol was absorbed by sulphuric acid after being sealed up some months in an N-shaped glass tube. Analysis gave the formula $\text{C}_{16}\text{H}_{14}\text{O}_2$; the substance melted at 80° . In the reactions the compound $\text{C}_{16}\text{H}_{14}\text{O}_2\text{C}_2\text{H}_6\text{O}$ was employed. By oxidising a solution in glacial acetic acid with chromic anhydride a mass of orange-colour needles of phenanthren-quinon was obtained. By distillation with zinc-dust, and re-distilling the distillate, and treating the product with picric acid, the double compound of phenanthren and picric acid was obtained in long orange needles, melting at 145° , from which phenanthren was obtained, melting at 9.5° to 9.6° . On treating the compound $\text{C}_{16}\text{H}_{14}\text{O}_2\text{C}_2\text{H}_6\text{O}$ with caustic alkalies it dissolves, yielding a potassium salt in satiny flat needles, soluble in water. From the solution CO_2 precipitates the body $\text{C}_{16}\text{H}_{14}\text{O}_2$, which thus behaves like a phenol. The author then considers the question as to the constitution of the quinons, and comes to the conclusion, from the action of zinc-ethyl and from other considerations, that Graebe's views are correct, and that phenanthren-quinon has the structure of a peroxide, thus—



and is not, as suggested by Fittig,—



Dr. ARMSTRONG objected to the conclusions arrived at by Dr. JAPP, and thought that, though his argument was very ingenious, it was not conclusive; in fact, that it was just as likely that a substance having the constitution assigned by Fittig would give—



In fact, the probabilities were in favour of the first supposition when the behaviour of phenanthren-quinon with nascent hydrogen was considered.

The Society then adjourned to April 15, when the following papers will be read:—"On the Lecture Illustration of Chemical Curves," by Dr. Mills; and "On the Analysis of Organic Bodies containing Nitrogen," by W. H. Perkin.

NOTICES OF BOOKS.

Saint Germain-en-Laye. The Sewage of Paris. Operations of the Committee of Defence against the Projects of Stripping and Irrigating the Forest of Germain. (Eaux d'Egout de Paris. Travaux du Comité de Défense contre les Projets de Déboisement et d'Irrigation de la Forêt de Saint Germain-en-Laye.) Saint Germain: Bardin. Paris: Dunod.

THE value of forests in a sanitary, climatic, and even agricultural point of view, is a question on which the educated public can no longer plead ignorance. Were no other evidence available, the document in which Baron von Müller, the Government Botanist of Victoria, pleads for the maintenance of the Australian woodlands ought to be more than sufficient. It is, therefore, with lively regret that we learn the revival of a project which threatens the destruction of one of the forests surrounding Paris. Saint Germain is to be sacrificed to one of the most obtrusive and persistent whims of modern times, and 1400 hectares (equal in round number to 3360 acres) of its pleasant and salubrious woodlands are to be converted into a—sewage irrigation farm! Against this unhappy project the eighteen adjoining communes have felt themselves bound to protest, and have formed a joint committee of defence. The labours of this committee have brought to light facts which ought not to be overlooked by sanitary reformers in England.

The very project of carrying the sewage on to St. Germain is an avowal that the vaunted irrigation farm of Gennevilliers is a failure. We were told, not very long ago, that the plain of Gennevilliers—the soil of which is of an exceedingly suitable nature—would be quite sufficient to absorb the whole of the sewage of Paris—to purify it, and to thus restore the Seine to its pristine limpid condition. Three hundred thousand cubic metres of fœtid water were thus to be treated day by day the whole year round. In actual fact, from 25,000 to 30,000 cubic metres—or say one-tenth part of the quantity desired—have been thus applied, and have been found excessive and injurious. What, then, becomes of the original estimate, and what confidence can we feel in its propounders? That the inhabitants of Gennevilliers have suffered a real, tangible injury from this application of one-tenth part of the dose originally intended has been established beyond all doubt by the investigations ordered by the tribunals. The level of the underground waters, which was formerly 4 metres below the surface, has been raised 2 metres. Cellars which were formerly invaded by the water only when the Seine rose exceptionally high are now regularly under water, and their floors have had to be raised till there is no longer room in them to stand upright. "It is certain," says the work before us, "that an important manufacturer like M. Pommier would not have amused himself by placing the boilers of his engines where the furnaces were perpetually invaded by water. It is certain that sand quarries were formerly worked at Gennevilliers, and that they were not worked under water." Yet these open and palpable facts were up to the last moment denied by the municipal engineers. Nor was the water purified as it was charitably hoped might be the case. The quality of the water in the shallow wells of the district has undergone a decided change for the worse.

We may here be permitted to mention a small observation of our own. No further back than November last we had occasion to perform some experiments upon Paris sewage, and we learnt that at a certain place in Gennevilliers, not far from the Bridge of Saint Ouen, we might obtain a supply without the delay and expense of obtaining official permission to break into a sewer. The water thus procurable had every character of somewhat stale sewage, and was especially rich in sulphuretted hydrogen; and after a careful examination we found it could have no other source save the sewage of Paris, after percolating through a stratum of soil of certainly not less than two yards in depth. As regards the salubrity of the district, it is not denied that intermittent fevers, though not of a malignant type, have become more frequent since the introduction of irrigation. The plot of land actually irrigated does not, however, exceed 300 hectares out of a total extent of at least 1000. Hence it becomes exceedingly probable that were the irrigated tract extended the injury to health would become more serious.

The inhabitants of Gennevilliers are unanimous in declaring that on a calm summer evening the odour diffused is decidedly offensive. Visitors who make a short inspection of the place in the middle of the day do not perceive this nuisance. Having made a stay of a fortnight we can affirm that the residents of the village are not mistaken. We can likewise bear testimony to the extreme and costly vigilance which prevails in the removal of all deposits of organic matter from the channels. The work before us enquires very naturally whether a similar vigilance would or could be kept up if the irrigation tract were larger and more remote from inspection? If irrigation can only be prevented from becoming a nuisance and a danger by such minute and costly watchfulness, are not its claims to general adoption *ipso facto* very much weakened?

We shall return to this subject, with especial reference to the agricultural phase of the Paris experiments, and to the circumstances which indicate that at St. Germain irrigation would be a far more serious mistake than it has been at Gennevilliers.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 10, March 8, 1880.

Chemical Stability of Matter in Sonorous Vibrations.—M. Berthelot.—The author has investigated the question whether the ordinary sonorous vibrations of ponderable matter can provoke decompositions and combinations? Many chemists admit that explosive bodies may detonate under the influence of certain musical notes, but this theory is not incontestably examined. M. Berthelot has made experiments with very deep sounds (vibration, 100 per second), and very acute ones (vibration, 7200 per second). Ozone, arsenic hydride, sulphuric acid, in presence of ethylen, oxygenated water, and persulphuric acid, have been exposed to these vibrations without sensible effect. Hence, the author concludes that matter is stable under the influence of sonorous vibrations, but is decomposed when acted upon by ethereal vibrations, which are much more rapid than those even of the acutest sounds.

New Remarks on the Formation-Heat of Gaseous Chloral Hydrate.—M. Berthelot.—The author points out that the recent experiments of M. Wurtz upon this subject have been made under unfavourable conditions. The pressure, 0.16 m., has been too low. In proportion as the pressure is reduced we approach a tension at which

no combination takes place, a circumstance which greatly reduces, if it does not annul, the heat developed under a normal pressure. The volume of the gases in reaction was likewise too small.

Comparison between the Tension Curves of Saturated Vapours (Second communication).—M. Paul de Mondesir.—Not susceptible of useful abstraction.

Action of Electrolysis upon Terebenthen.—Ad. Rénard.—Terebenthen, along with sulphuric acid, alcohol, and water, was submitted to the action of five Bunsen elements. Hydrogen was evolved at the negative pole. Among the products the portion distilling between 170° and 250° yielded a portion of terebenthen which had escaped the reaction, cymen boiling at 178° to 180° , and monohydrate of terebenthen, $C_{10}H_{18}O$, boiling between 210° and 215° . It may be considered as a pseudo-alcohol. The aqueous liquid separated from the mixture of cymen and of the mono-hydrate contains an acid, the sulpho-ethylic derivative of a hydroxyl-campholic acid, differing from campholic acid by an excess of $2(OH)$.

Synthesis of Aromatic Aldehyds: Oil of Cummin.—Chloride of chromyl attacks the group CH_3 in connection with the phenyl radical, and by the ulterior action of water transforms it into the COH , characteristic of the aldehyds. If cymen is treated with chromyl chloride in a sulpho-carbonic solution there is formed a brown granular precipitate, which, after decomposition with water and purification, yields a yellow oil, presenting all the properties of the cuminic aldehyd extracted from the ordinary essential oil of cummin.

Two New Silicates of Alumina and Lithia.—P. Hautefeuille.—One of these artificial silicates approaches oligoclase, whilst the other may rank with orthose and albite.

Phosphates and Boro-phosphates of Magnesia and Lime from the Guano Beds of Mejillones.—M. Domeyko.—Guanos from latitudes 23° to 24° S. retain merely a trace of nitrogen, and consist chiefly of phosphates. The boracic acid in one sample of boro-phosphate reached 11 per cent.

Composition of the Waters of Cransac (Aveyron).—E. Willm.—This water is chiefly remarkable as containing nickel. Manganese and zinc were present, but no copper.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 19.

Conversion of Propyl-bromide into Iso-propyl-bromide.—A. Kekulé and H. Schröter.—Normal propyl-bromide, carefully purified and boiled at 71° , was heated for some time in a cohobator with aluminium bromide and distilled off. The product, after purification, was found to be iso-propyl-bromide, boiling at 60 to 63° .

Fumaric and Maleic Ether.—R. Anschütz.—This paper contains an account of bibrom-succinic diethyl-ether, of bibrom-succinic methyl-ether, fumaric diethyl-ether, fumaric dimethyl-ether, maleic diethyl-ether, maleic dimethyl-ether, mono-brom-maleate of silver and the corresponding fumaric salt, mono-brom-fumaric dimethyl-ether, and mono-brom-maleic diethyl-ether.

The Mixed Azo-Compounds.—C. Kappeler.—An investigation of azo-nitro-methan-benzol and of certain analogous compounds.

On Nitroso-ethyl-methyl-keton.—H. Gutknecht.—An examination of certain bodies formed by the action of nitrous acid upon acet-ester.

On Bioxy-fumaric Acid.—S. Tanatar.—The author has oxidised fumaric acid with potassic permanganate, obtaining bioxy-fumaric acid, along with oxalic and carbonic acids in smaller proportions. Free bioxy-fumaric acid causes a turbidity or precipitate in the solutions of lime salts, even of the sulphate.

Certain Derivatives of the Three Toluylic Acids.—E. Ador and A. A. Rilliet.—Not adapted for useful abstraction.

Reply to Delachanal's and Mermet's Claims of Priority.—C. Fahlberg and W. Iles.—The dispute refers to a method for the determination of sulphur (*Berichte*, xi., 1187). The authors show that their procedure is not identical with that of MM. Dalachanal and Mermet.

A Lecture Experiment.—F. Kessel.—An account of a phenomenon observed on bringing sulphuretted hydrogen into contact with fuming nitric acid.

Dimethyl-naphthyl-amin and Naphtho-quinon.—P. Mounet, F. Reverdin, and E. Noelting.—Perfectly pure dimethyl-naphthyl-amin does not give, with ferric chloride and sulphuric acid, the coloured reaction described by Landshoff. In the hope of obtaining a tinctorial product analogous to methyl-violet, the authors submitted it to oxidation under similar circumstances, but obtained merely a blackish resin of a pungent odour. Other oxidising agents gave no better results.

Amido-acids with Alcohol Radicles.—Peter Griess.—The compounds here described are trimethylated amido-salicylic acid, oxy-benz-betain hydriodate and hydrochlorate, benz-betain periodide and perbromide.

Contribution to a Knowledge of the Ureides.—E. Mulder.—An account of certain derivatives of barbituric acid.

Decomposition of Vegetable Indican in the Absence of Air.—E. Schunck and H. Röemer.—Vegetable indican, on decomposition by hydrochloric acid in the absence of air, gives neither indigo-blue nor indigo-white.

Decomposition of Rubianic Acid by Hydrochloric Acid in Presence of Mercury.—E. Schunck and H. Röemer.—On subjecting rubianic acid to a similar treatment to that applied in their last experiment, the authors obtained two bodies which they are about to examine.

Spectroscopic Notes.—Hermann W. Vogel.—The author recommends chemists who have no Bunsen flame at command to use as a substitute the flame of hydrogen, with which he has succeeded in obtaining the spectra of the earths and alkalies. He also gives instructions for the detection of cobalt along with iron and nickel. Cobalt salts, dissolved in alcohol and mixed with potassic sulphocyanide, give a blue liquid with a most characteristic spectrum. This reaction became more important after Zimmermann had shown that the sulpho-cyanides of cobalt and nickel are precipitated by sodium carbonate, which the corresponding iron compound is not. Upon these facts the author founds the following method for detecting cobalt in presence of nickel and iron. The solution in which the iron must be present in the ferric state is mixed with an excess of ammonium sulphocyanide, and then with sodium carbonate, till the blood-red colour has changed to a yellow. The whole is then filtered and the filtrate shaken up in a test-tube with a mixture of equal volumes of amyl alcohol and ether. This mixture takes up the cobalt sulphocyanide from the aqueous liquid and floats on the top as a blue stratum, in which the absorption-band may be found between C and D.

The Oxidation of Para-sulph-amin-toluylic Acid.—Oscar Jacobsen.—Not suitable for abstraction.

Ethylated Mercuramin Compounds.—H. Köhler.—An addendum to the author's paper (xii., 2208).

Ortho-tolyl-urethan, Ortho-tolyl-iso-cyanate, and Diortho-tolyl-urea.—R. H. C. Nevile and A. Winther.—A brief notice of the bodies named above.

Oxidation of Cholic Acid.—M. Kutscheroff.—None of the author's experiments upon the oxidation of cholic acid, whether amorphous or crystallised, have yielded fatty acids, which completely clashes with the results of Tappeiner.

The Third Pyridin-mono-carbonic Acid.—Z. H. Skraup.—Not suitable for abstraction.

Reply to H. Schiff with Reference to the latter's Remarks on his Researches on the Constitutional Formulæ of the Ferric Compounds.—D. Tommasi.—The writer complains that Schiff has misrepresented his views on the oxy-chlorides and basic nitrates of iron.

Certain Mono-chloro-crotonic Acids.—G. W. A. Kahlbaum.—An account of mono-chloro-tetracrylic acid, mono-chloro-crotonic acid, and mono-chlor-iso-crotonic acid.

Conversion of Piperidin into Pyridin.—W. Koenigs.—This conversion is effected by oxidation.

Tinctorial Bodies of the Rosanilin Group.—E. and O. Fischer.—A very important paper, but quite incapable of useful abstraction.

Transformations of the Lævo-rotatory Terpen from French Oil of Turpentine by means of Hydration and Dehydration.—F. Flawitzky.—The author has obtained a lævo-rotatory iso-terpen.

Action of Sulphur upon Phenyl-Benzamid.—A. W. Hofmann.—An account of a new compound, benzenyl-amido-phenyl-mercaptan.

Bulletin de la Société Chimique de Paris,
No. 4, Feb. 20, 1880.

Volumetric Determination of Active Oxygen in Barium Peroxide and Oxygenated Waters.—M. Armand Bertrand.—Already noticed.

Artificial Production of a Crystalline Iron Arseniate.—MM. A. Verneuil and Leon Bourgeois.—The authors have obtained scorodite by acting upon iron with a concentrated solution of arsenic acid at 140° to 150° in a sealed tube.

Reversion of Phosphates Soluble in Ammonium Citrate.—M. A. Colson.—The citrate does not dissolve the ammonia magnesium phosphate, though this body is slightly soluble in water. Bicalcic phosphate prepared under certain conditions leaves up to 73 per cent of its weight undissolved in the citrate. Certain superphosphates prepared with the siliceous ores of Vaucluse underwent a rapid reversion, losing in a fortnight from 10 to 15 per cent of soluble phosphate. The author considers that this change is due to the sulphate of lime, which removes water of hydration from the assimilable phosphates and renders them to a great extent insoluble in the citrate. Superphosphates prepared with phosphoric acid revert but slightly and slowly, whilst those prepared with sulphuric acid undergo a greater reversion the more considerable the proportion of the latter.

Preparation of Pure Levulose.—M. Ch. Girard.—Not suitable for abridgment.

Reactions Produced by Aqueous Dimethylamin upon Metallic Solutions.—M. Camille Vincent.—With neutral magnesium salts there is formed a white precipitate insoluble in excess. With the salts of glucinum and zirconium the results are similar. Aluminic salts give a white precipitate soluble in excess. Ferrous salts give a dirty white precipitate, and ferric salts an ochre-brown, both insoluble in excess. The green chromic salts give a green precipitate and the violet modification a greenish-grey, both insoluble in excess. Manganese salts give a white precipitate which rapidly turns brown on exposure to the air. The presence of sal-ammoniac prevents the precipitation. Cobalt salts give a blue-grey precipitate and nickel salts an apple-green, both insoluble in excess. Uranic salts give a yellow precipitate insoluble in excess. Zinc salts give a white precipitate soluble in excess. Cadmium salts give a white precipitate insoluble in excess. The salts of tin all give white precipitates, the stannous being insoluble and the stannic soluble in excess. Antimony: the salts of Sb_2O_3 give a white precipitate insoluble in excess and those of Sb_2O_5 also a white precipitate soluble in excess. Bismuth gives a white precipitate in-

soluble in excess, as do all the soluble salts of lead. Copper salts give a blue precipitate, which is turned deeper by an excess but does not dissolve. Mercurous salts give an insoluble black precipitate and mercuric salts a white precipitate, insoluble in excess, but soluble in much water. Silver salts yield a deep grey precipitate, soluble in excess. Dimethylamin dissolves silver chloride. Concentrated palladic solutions give a brown precipitate soluble in excess and forming a brown liquid which turns to a light yellow. Auric chloride gives a yellow precipitate, soluble in excess and forming a deep brown liquid. Platinum chloride forms an orange-yellow precipitate of a double salt soluble in water.

Russian Chemical Society, Session of March 1/13, 1879.—M. Lagermark sent in a memoir on the synthesis of tetrolic acid. The same chemist, in conjunction with M. Eltekoff, presented a paper on the synthesis of pyroterebic acid. M. Beketoff explained his process for determining the hydration-heat of sodium protoxide. M. Flavitzky has obtained from French oil of turpentine a terebenthene whose rotatory power, $[\alpha]_D = 43.4^\circ$. M. J. Goldstein explained the theoretic manner of calculating the boiling points of the saturated hydrocarbons of normal structure. M. Latschinoff announced that he had been for three years engaged with the study of the oxidation of cholalic acid by permanganate.

Chemiker Zeitung.
No. 5, 1880.

The Phosphorus Question.—The author points out the injurious character of the match manufacture where the ordinary or white phosphorus is in use, and considers that the sanitary precautions adopted in various establishments are utterly useless. The loss from fires, occasioned by the careless use of matches, in Germany alone, for the ten years 1862 to 1871, is calculated at £900,000.

Separation of Gold by Quartation with Zinc.—C. A. M. Balling.—The author criticises Jüptner's method (*Zeitschrift für Anal. Chemie*, 1879, p. 104), and points out certain defects. On melting the zinc with the alloy in an open porcelain crucible the former is partially oxidised, the film of oxide hindering the contact of the metals. If the fusion is performed under a layer of resin, the vapours become ignited and the resin is often burnt away before the fusion is effected. Even in a covered crucible the resin is also soon volatilised, whilst particles of carbon are deposited on the lid and the sides, and falling back into the crucible contaminate the alloy. The finer particles of carbon render the solution in nitric acid turbid and cannot be entirely removed by washing. The author, therefore, modifies the process as follows:—He used cadmium, which is more readily fusible for quartation, instead of zinc, and takes potassium cyanide as a cover. The metals unite readily under the melted cyanide at the heat of a Berzelius spirit-lamp and a homogeneous regulus is soon obtained. The crucible, when cold, is placed in a beaker and the potassium cyanide is dissolved in water. The solution is poured away and the metal is washed with water. It is then introduced into a flask and boiled once with nitric acid of specific gravity 1.20 and then thrice with nitric acid of specific gravity 1.30. The cadmium is completely dissolved. Two and a half parts of this metal suffice for quartation. The granule of gold retains the form of the original metal and is transferred to a small crucible for ignition.—*Oest. Zeitschrift Berg. u. Hüttenw.*

Determination of Sulphur in Pyrites.—R. Fresenius.—The precipitation of barium sulphate in boiling solutions does not take place instantaneously. Appreciable quantities remain at first in solution. If the solution contains ferric chloride, this has, on the one hand, a solvent action upon the barium sulphate, which, on the other hand, always appears red after ignition, and the ferric oxide producing this colour cannot be removed by treatment

with hot hydrochloric acid. These two sources of error act in opposite directions, but the solvent action of the hydrochloric acid and the ferric chloride preponderates. In an examination of Lunge's process (precipitation of the boiling liquid containing ferric chloride and some hydrochloric acid with boiling barium chloride and immediate filtration) Fresenius has obtained deficiencies of 1 to 1.7 per cent in a pyrites containing 43.8 per cent of sulphur, as ascertained by his own method (*Zeitschrift Anal. Chemie*, xvi., p. 339).—*Zeitschrift Anal. Chemie*, xix., p. 53.

NOTES AND QUERIES.

Valuation of Soda-Ash.—The formulæ given below will show in what proportion to mix a strong and a weak ash, so as to obtain one of a given intermediate strength. Let a , b , and c = percentage of alkali in strong, weak, and intermediate ash, respectively; W = weight required of strong ash, and w = weight required of weak ash. Then—

$$W = \frac{c-b}{a-b}, \text{ and } w = \frac{a-c}{a-b}.$$

An example will make this clear:—Suppose it is desired to find what weight of strong ash at 52 per cent, when mixed with ash at 12 per cent, will be required to make a ton of ash at 34 per cent. Then—

$$W = \frac{c-b}{a-b} = \frac{34-12}{52-12} = \frac{22}{40} = \frac{11}{20}.$$

Therefore 11 cwts. at 52 per cent and 9 cwts. at 12 per cent will produce 1 ton of 34 per cent ash.—EDWARD JACKSON.

MEETINGS FOR THE WEEK.

- MONDAY, 12th.—Medical, 8.30.
 — Royal Geographical, 8.30.
 — Society of Arts, 8. Robert W. Edis, F.S.A., "The Decoration and Furniture of Town Houses."
 TUESDAY, 13th.—Civil Engineers, 8.
 — Photographic, 8.
 — Anthropological Institute, 8.
 — Royal Medical and Chirurgical, 8.30.
 — Royal Institution, 3. Prof. Huxley, "Dogs and the Problems Connected with them."
 WEDNESDAY, 14th.—Society of Arts, 8. Henry B. Wheatley, F.S.A., "The History and Art of Bookbinding."
 — Microscopical, 8.
 — Geological, 8.
 THURSDAY, 15th.—Royal Society Club, 6.30.
 — Royal Institution, 3. Prof. Tyndall, "Light as a Mode of Motion: Theories of Light and Colour."
 — Royal, 8.30.
 — Chemical, 8. Dr. Mills, "On the Lecture Illustration of Chemical Curves." W. H. Perkin, F.R.S., "On the Analysis of Organic Bodies Containing Nitrogen (continued)."
 FRIDAY, 16th.—Royal Institution, 8. M. Ernest Renan, "Marcus Aurelius," 9.
 — Society of Arts, 8. Prof. Vamberg, "Russia's Influence over the Inhabitants of Central Asia During the Last Ten Years."
 SATURDAY, 17th.—Royal Institution, 3. Mr. James Sully, "Art and Vision."

TO CORRESPONDENTS.

J. H.—For the best account of Spence's metal, see *CHEMICAL NEWS*, vol. xli., p. 87.

ROYAL POLYTECHNIC.—Lectures on the Phenomena of Nature, illustrated by Dissolving Views and Experiments, by Mr. J. L. King. Arctic Exploration, its History and Prospects, by Mr. W. R. May. Balmain's Luminous Paint, or a Trap to Catch a Sunbeam. Wonderful New Automatic "Blondin," which will traverse the whole length of the Hall on a rope suspended thirty feet from the floor. Tableaux Vivants of events in the life of Mary Queen of Scots; the descriptive poem by Bell, read by Miss Alice Burnelle, the character of Mary impersonated by Miss Eva Russell. New Ghost Piece, written by Ellis Reynolds, entitled *The Magician Foiled*. Also, *Leotard the Automaton*, the *Diving Bell*, the *Electric Organ*, *Going Machinery*, and many Novelties. Open from 10 till 1, 2 till 5, and 6 till 10; for times of Entertainments see daily papers. Admission, 1s. Reserved Seats, 2s., 1s., and 6d. extra.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1064.

BETORCINOL AND SOME OF ITS DERIVATIVES.

By J. STENHOUSE and CHARLES E. GROVES.

(ABSTRACT.)

ABOUT thirty-two years ago, in the course of an extended examination of the lichens with regard to the organic compounds contained in them, and derivatives of these compounds, a quantity of the crude usnic acid obtained from *Usnea barbata* and *Cladonia rangiferina* was submitted to destructive distillation. The distillate was found to contain a crystalline substance of the composition $C_8H_{10}O_2$, homologous with orcinol and very closely resembling it in most of its properties. This new member of the orcinol group was named β -orcin, and as it was known that the erythrin and lecanoric acid from the *Rocellas* and the *Lecanoras* yielded orcinol, it was somewhat hastily inferred that the usnic acid in the *Usnea* and *Cladonia* was the source of the so-called β -orcin. Subsequent attempts to prepare the β -orcin by the distillation of pure usnic acid from *U. barbata* yielded negative results, not a trace of the orcinol could be detected.

As there were not only many points of interest connected with this higher homologue of orcinol, but also great uncertainty as to what was the real source of the compound, a quantity of *U. barbata* was obtained from two different localities in Scotland, and carefully freed from every trace of evernia or other lichen. The crude usnic acid extracted from this usnea was submitted to destructive distillation, and the distillate examined for the orcinol by means of chloride of lime solution. The crimson-red colouration produced at once showed that the usneas from both localities contained the orcinol-yielding compound: it remained to determine whether this was the usnic acid or some other principle accompanying it in the lichen. Destructive distillation of the carefully purified usnic acid at once settled this point; not a trace of the β -orcin could be discovered in the distillate. There could no longer be any doubt, therefore, that the usnic acid was not the source from which the β -orcin was derived, but another substance accompanying it in the lichen in the same way that evernic acid accompanies usnic acid in the *Evernia prunastri*.

Betorcinol,* $C_8H_{10}O_2$ or $C_6H_2Me_2(OH)_2$. In order to prepare this member of the orcinol series, *U. barbata*, which has been carefully freed from every particle of evernia or other lichen which may be present, is exhausted with milk of lime in the usual way, and the crude acids thrown down on adding hydrochloric acid to the lime solution, are boiled with lime and water for some hours, taking care to exclude the air so as to avoid oxidation. The clear solution separated from the insoluble basic calcium usnate is at once neutralised with hydrochloric acid and evaporated as far as possible on the water-bath, when the crude betorcinol crystallises out, being almost insoluble in concentrated solutions of calcium chloride. It is purified by successive crystallisations from benzene and from water. The crystals were found to have the same melting-point, $163^\circ C.$, as the original specimen prepared thirty-two years ago, and to be identical with it in its chemical properties. Betorcinol differs from orcinol not only in the melting-point and in being much less soluble in water, but also in its chemical reactions, especially with hypochlorites and with ammonia.

Betorcinol gives a characteristic bright crimson colouration with hypochlorites very different from the purplish red obtained with orcinol. The ammoniacal solution, moreover, becomes rapidly coloured on exposure to the air, whilst the corresponding orcinol solution is but slowly changed.

Chloro-betorcinols.—Betorcinol yields two chlorinated derivatives, both of which are colourless crystalline substances.

Tetra-chloro-betorcinol, $C_8H_6Cl_4O_2$, is easily prepared by adding a solution of the orcinol to chlorine hydrate, taking care to keep the latter in excess, and crystallising the product from light petroleum. It forms large prisms (m.p. 109°), which are readily converted into *dichloro-betorcinol*, $C_8H_8Cl_2O_2$, by the action of hydriodic acid; this crystallises from light petroleum in long colourless needles (m.p. 142°).

Bromo-betorcinols, corresponding with the two chloro-betorcinols, have been prepared and examined. *Tetra-bromo-betorcinol*, $C_8H_6Br_4O_2$, is obtained in a manner somewhat similar to the chlorinated derivative by adding betorcinol solution to a mixture of bromine and water, keeping the bromine in excess. It crystallises from petroleum in large colourless prisms (m.p. 101°), which are converted into *dibromo-betorcinol* by the action of hydriodic acid. The dibromated derivative is much more easily prepared, however, by acting on betorcinol with the requisite proportion of bromine diluted with carbon bisulphide, and crystallising the product from carbon bisulphide and from petroleum. It forms long colourless needles (m.p. 155°).

Moniodo-betorcinol, $C_8H_9IO_2$, is a colourless crystalline compound, obtained by the action of lead oxide in excess on an ethereal solution of betorcinol and iodine in a manner precisely similar to that employed in the preparation of moniodorcinol. It is only very sparingly soluble in boiling water, and crystallises out on cooling. Its melting-point is 92° .

Mono-nitroso-betorcinol, $C_8H_9(NO)O_2$, is thrown down as a bright red precipitate on pouring an aqueous solution of betorcinol into a mixture of nitrosyl sulphate and water. It is soluble in hot water and easily soluble in spirit, but cannot be crystallised from these menstrua. It is also soluble in hot glacial acetic acid, and on cooling separates in minute prismatic crystals of a brilliant crimson colour. All attempts to obtain a crystalline nitro-derivative of betorcinol were fruitless, whether by the oxidation of the nitroso-compound or by the direct action of nitric acid on betorcinol. In each case nothing but amorphous flocculent substances were produced.

The Acid which yields Betorcinol.—As the crude mixed acids from the usnea contain but a very small quantity of this acid in proportion to the usnic acid, great difficulties were experienced at first in separating the two, and it was not until after many fruitless trials that a process was discovered which permitted the new acid to be isolated with certainty. The method is founded on the difference in the solubility of the two acids in benzene and in ether respectively, the new acid being much more soluble in cold ether than usnic acid is. It crystallises from benzene in colourless needles or long plates, and sometimes in short prisms. It melts at 186° , and at a somewhat higher temperature decomposes with evolution of carbonic anhydride and formation of betorcinol. The analyses of this acid correspond with the formula $C_{19}H_{20}O_7$, which has the same relation to betorcinol that evernic acid, $C_{17}H_{16}O_7$, has to orcinol. As, however, it cannot be considered to be definitely decided that it is a dimethyl-evernic acid, it has been named provisionally *barbatic acid* from the circumstance of its existing along with usnic acid in the *U. barbata*.

By treating an usnea found growing on Calisaya bark with lime and dilute alcohol, Hesse succeeded in obtaining an acid of melting-point 172° , which he termed usnic acid. Although this acid resembles barbatic acid in many of its properties, the much lower melting-point and the

* As the prefix α and β is now almost universally employed to distinguish isomerides, it has been thought advisable to change the name of this homologue of orcinol from " β -orcin" to betorcinol.

higher percentage of carbon in the former (about 1.7 per cent), renders it improbable that the two are identical, especially as he states that when heated it gave no sublimate. It is well known that the lichen acids derived from orcinol, such as lecanoric and evernic acids, yield ethereal salts with the greatest readiness when boiled with alcohol, and a knowledge of this fact made the authors studiously avoid the use of alcohol in the preparation of barbatic acid, whilst Hesse not only employed dilute alcohol to extract the mixed acids from the lichen, but purified his usnetic acid by dissolving it in hot dilute alcohol and evaporating the solution.

ON NASCENT HYDROGEN.

By Dr. D. TOMMASI.

I HAVE just received from the Secretary of the Dutch Society of Sciences at Harlem, the original memoir by Dr. Phipson on catalytic force. My first duty is to sincerely thank Prof. von Baumhauer for the kind manner in which he remembered to send me Dr. Phipson's memoir. I have read the work with much interest, and, although I do not share Dr. Phipson's opinions in any degree, I cannot do less than compliment my honourable adversary on the manner in which he has treated so difficult a question as catalytic force, a method which is as clear as it is profound.

Dr. Phipson takes hardly any notice in his memoir of anything but catalytic force; as to the question of the nascent state, he only glances at it. Catalytic force takes up 34 pages of the memoir, and Dr. Phipson only devotes about half a page to the nascent state of matter. Here is what he says in this half page:—

"We know since chemistry has existed as a science, how great an influence the *nascent state* of a body has on the compounds it can form; one such compound is *only* possible when one of its constituents is in the nascent state; another, only when both the bodies which form it are in this state. What is called the "*nascent state*" is, I think, nothing more or less than the allotropic state of bodies entering into combination; it is already some time since I published this idea. M. Houzeau seems to be of the same opinion as myself. Considering these facts, it seems incontestible that *every time oxygen enters into or leaves compounds it is in the state of ozone*. When we reflect further on the results already obtained with hydrogen, chlorine, bromine, sulphur, and phosphorus, we are inclined to believe that all the simple bodies should behave in the same manner; that is to say, that all bodies may have an allotropic state analogous to ozone, and that they are in this state at the moment of entering into combination, or when they are in the *nascent state*."

See, now, what I have said about nascent hydrogen:—

"If hydrogen in the nascent state is endowed with a greater affinity for other bodies than it has in the ordinary condition, it is simply because hydrogen at the moment of leaving a compound is accompanied by a large quantity of heat, which is generated during its being set at liberty. Therefore nascent hydrogen is nothing more than ordinary hydrogen under different physical conditions. In my opinion, nascent hydrogen is merely hydrogen + heat. In fact all the reductions which are produced by nascent hydrogen can be obtained equally well with ordinary hydrogen and a higher temperature. And the differences which are observed between the hydrogens produced by different chemical reactions, are caused by these reactions not all developing the same amount of heat."

Now, I ask what resemblance can there be between the explanation put forward by Dr. Phipson and that given by myself?

How could Dr. Phipson say that my theory on the nascent state was the same as that which he made known in 1858, with this single difference, that in the place of "*electricity*" I put "*heat*," while, speaking of the nascent

state in his memoir, Dr. Phipson *does not once use the word "electricity"?*

What are the experiments which Dr. Phipson has made to prove that nascent hydrogen is not hydrogen accompanied by heat or electricity, but hydrogen in an allotropic state similar to ozone, as he pretends? None, that I know of.

Again, I was the first to give a precise and scientific definition of what nascent hydrogen is, and I was also the first to collect a sufficient number of facts to form the basis of a theory. I have given up two entire years to the study of nascent hydrogen, I have published ten memoirs on this subject, and I have made a very large number of experiments. I should also like to make some observations on the manner in which Dr. Phipson explains the phenomena formerly known as catalytic, but as this would be a deviation from the subject of the present note, I reserve my observations for a future time.

DETECTION OF HYDROCHLORIC ACID BY SULPHURIC ACID AND ACID POTASSIUM CHROMATE.

By H. W. WILEY.

THIS well-known test is more conveniently made in the following way than by distillation in a retort as usually directed. Two small beakers are taken, of different sizes, the smaller of which will fit into the larger, leaving a space of two or three centimetres between the two bottoms. The chloride and chromate are well rubbed together, and placed in the larger beaker, care being taken that none of the particles touch the sides of the vessel. A few drops of H_2SO_4 are added, and the smaller beaker then put in place. A lump of ice, and if necessary a little salt, are then placed in inner beaker. The (so-called) chloro-chromic acid is given off at a very gentle heat (do not boil), and is readily condensed on the cold surface of the beaker. If there is more than a mere trace of chloride a watch-glass can be substituted for the smaller beaker.

After the reaction has ceased the inner beaker is removed, and the usual test, $[(\text{H}_4\text{N})\text{OH}]$, is applied. I extend and modify this in the following way:—

(1.) After adding a drop of $(\text{H}_4\text{N})\text{OH}$ to the CrO_2Cl_2 (still on bottom of inverted beaker) $\text{HC}_2\text{H}_3\text{O}_2$ is added in excess. A drop solution of $\text{Pb}(\text{C}_2\text{H}_3\text{O}_2)_2$ then is added: PbCrO_4 is precipitated. This will detect HCl when so little is present that the change of colour with $(\text{H}_4\text{N})\text{OH}$ is not perceptible.

(2.) This is an easier and far more delicate process. In fact it seems quite as sensitive and far more striking than the test with silver nitrate. The CrO_2Cl_2 is prepared as above. With a stirring-rod a very little H_2SO_4 is placed on a white porcelain surface and near by a minute crystal of strychnia. The end of the stirring-rod is then moistened with the CrO_2Cl_2 , and brought in contact with the H_2SO_4 and strychnia. The colour test for strychnia is developed with even greater brilliancy than with $\text{K}_2\text{Cr}_2\text{O}_7$: the merest trace of a chloride is most unmistakeably revealed by this test. If bromides are present they do not at all interfere with the above reaction. Bromine itself with strychnia and H_2SO_4 gives no play of colours. Bromides, moreover, when present, can usually be detected at the same time with the chlorides. The bromine formed is readily condensed, and forms either distinct globules or imparts a deeper colour to the CrO_2Cl_2 .

With iodides the case is quite different. In many cases I have found no trace of CrO_2Cl_2 when both chlorides and iodides were present. At other times the iodine which is set free seems to be dissolved in the CrO_2Cl_2 . In such cases, on adding $(\text{H}_4\text{N})\text{OH}$ to the reddish yellow distillate, the whole mass turns black, from the formation of iodide of nitrogen. On filtering this off the reactions for CrO_2Cl_2 ,

or the chromate formed from it, can be obtained in either of the ways specified above, but not with the same exactness as when iodides are entirely absent. The iodide of nitrogen on the filter-paper when dried will explode spontaneously, even when present in exceedingly minute quantities. It appears to be a much better (*i.e.*, more explosive) article than when prepared in the usual way.

I have yet to investigate the order of succession of the phenomena above mentioned as influenced by varying proportions of the several substances in the test employed. I have not been without a suspicion that bromides might possibly give rise to a trace of CrO_2Br_2 when treated as described. I have not, however, been able to procure an absolutely pure bromide with which I might determine the matter by experiment. All the authorities, however, whom I have consulted agree that only pure bromine is formed. The bromides I have tried all give faint reactions by the strychnia test, but I cannot say that they do not contain a trace of chloride. If someone in possession of an absolutely pure bromide would make the experiment the result would be of interest.

I am still investigating the anomalies produced in the chloride reaction with H_2SO_4 and $\text{K}_2\text{Cr}_2\text{O}_7$ by admixture of oxides.

Lafayette, Indiana, U.S.A.

ON DETECTION AND ESTIMATION OF ARSENIC.

By T. D. BOEKE, D.S.,

Laboratory of the High Burghal School at Alkmaar, Holland.

It is a well-known fact that the "white arsenic" and other arsenical compounds are extensively used for many purposes—*e.g.*, as a sheep-wash, as a means to prevent the growth of fungi in damp rooms, &c. The highly poisonous character of arsenical compounds has led in many countries to legal measures in order to prevent their being procured for criminal purposes. Yet it would seem as if really effective means to prevent their abuse are scarcely possible without actual prohibition. In Holland, at least, though the sale of "white arsenic" is only allowed under special conditions, poisoning by this compound is by no means a rarity. In not less than seven cases submitted to me for analysis white arsenic proved to be the agent which had produced death in six cases, and in one of them severe and painful illness.

As in all of them not only the quality but also the quantity of the poison formed the object of enquiry, I had ample occasion to become acquainted with the advantages and disadvantages of the existing methods of search and estimation. The following remarks about the detection and estimation of this element in cases of poisoning may perhaps prove useful to those that are called on to make similar experiments:—

I. Detection of Arsenic in Organic Mixtures.

The admirable method of James Marsh, at least in its present improved form, allows the detection of the minutest traces of arsenic, even in the most complicated mixtures, with such a degree of certainty and ease that it seems wholly unnecessary to use other methods, *e.g.*, that of Fresenius and Babo,* or that of Reinsch.† However, with one restriction, *viz.*, that the solution to be examined be wholly free from organic matter. This may be attained in two ways:—

- (1.) By deflagration of the well-dried matter by means of a mixture of saltpetre and soda.
- (2.) By heating a little bit of the matter to be examined, whether solid or liquid, with strong sulphuric acid and a few drops of nitric acid till the mass is wholly converted into a porous coal. Addition

of some scraps of filtering-paper (which should of course be free from arsenic) greatly quickens this reaction.

If this residual coal, after evaporation of the superfluous sulphuric acid, be carefully extracted by boiling water, and the formed sulphurous anhydride which it contains be totally driven out by boiling, a perfectly colourless liquid is obtained, which is generally quite free from organic matter.

It cannot be denied that minute traces of arsenic by both modes of treatment may escape detection; in the former case by volatilisation of metallic arsenic during the charring that precedes the deflagration; in the latter by volatilisation of some arsenious chloride formed by reaction of the chlorides present in the organic matter on the sulphuric acid. However, I prefer the latter method to the former, as the presence of the nitric acid during the possible evolution of hydrochloric acid in the then *fluid* mass greatly diminishes the chance of arsenious chloride being formed and driven out. Never, indeed, have I failed to detect by this treatment even the faintest traces of arsenic in organic masses; for instance, a few c.c. of blood of a poisoned victim, a minute portion of ground coffee containing "white arsenic," &c.

As for the experiment of Marsh itself, the precautions to be taken in making it are so amply described in many handbooks on toxicology (among which that of Dragendorff ought to be mentioned in the first place), that it may seem superfluous to allude to them here.

Among the numerous reactions to ascertain the presence of hydride of arsenic or of antimony, or of both, in the gas obtained, that of Lassaigne* ought never to be omitted. The black precipitate of silver being filtered off, addition of very diluted ammonia or sodium acetate (Avery†) to the filtrate occasions the very characteristic yellow silver arsenite to be formed. If the black precipitate containing the silver be treated, after Hoffmann,‡ with tartaric acid, the presence of antimony is easily detected by means of sulphuretted hydrogen. Still better results are to be had by Pettenkofer's reaction|| as it has been completed by Fresenius§ (heating of the obtained metallic rings in a slow current of sulphuretted hydrogen, and afterwards in a current of hydrochloric acid gas).

To ascertain if the metallic spots obtained on porcelain consist of arsenic or of antimony, I prefer calcium hypochlorite to the "eau de Javelle" recommended by Wachenroder,¶ as the latter often contains free chlorine, and in that case may dissolve antimony too.

Treatment of the spots with nitric acid (Dulk**) leads to safe results only if the evaporation of the nitric acid be effected with the utmost care.

If the above-mentioned reactions are carefully applied it is almost impossible that even the faintest traces of arsenic, or of antimony, or of both elements escape detection.

Estimation of Arsenic in Organic Mixtures.

Numerous are the means that have been proposed to convey the arsenic in organic mixtures into a compound which is fit to be used for its estimation. Experience has taught me that *variis modis bene fit* is here applicable; still I am of opinion that the following method, that I composed from well-known facts, combines an easy application and accurate results. The substance is heated on a water-bath with hydrochloric acid and potassic chlorate till it has assumed a liquid form (Fresenius and Babo, *l.c.*). Total solution by this means is never attained, as the chloranil (tetrachlorchinon), which is formed by the action of the nascent chlorine on the albuminoid matter, is only partly dissolved. When cold the acid liquid is filtered,

* *Journ. de Pharm.*, 1841, p. 52.

† *Silliman's American Journal*, xlvii., p. 255.

‡ *Ann. der Chem. und Pharm.*, cxv., p. 287.

|| *Pharmac. Centralbl.*, 1842, p. 551.

§ *Ann. der Chem. und Pharm.*, xliii., p. 361.

¶ *Arch. d. Pharm.*, (2), lxx., p. 14.

** *Arch. d. Pharm.*, xxxiv., 263.

* *Ann. der Chem. und Pharm.*, xlix., 287.

† *Journ. fur Prakt. Chem.* xxiv., 244.

and the residue on the filter washed till the acid reaction has nearly disappeared. This residue, if properly washed, never contains a trace of arsenic, as I had often the occasion to ascertain. The yellow filtrate is saturated (preferably in a wide-mouthed jar standing in a basin) with sodic carbonate (of course free from arsenic) till the reaction is strongly alkaline, and the liquid, whether turbid or not, is evaporated on a water-bath till the sodic chloride that it contains begins to crystallise out. By this evaporation it is greatly reduced in volume, so that even when kilogrammes of the organic matter have been treated in this way, generally not more than half a litre remains. During the evaporation it turns quite dark, and ammonia and amines escape from it. It is treated again with hydrochloric acid and potassic chlorate, filtered when necessary, and saturated with ammonia. Addition of the magnesian mixture causes all the arsenic which it contains to be precipitated as ammonio-magnesian arseniate, together with ammonio-magnesian phosphate. If this precipitate be filtered off after twenty-four hours, the separation is so complete that not the faintest trace of arsenic may be detected in the mother-liquor when treated afterwards by sulphuretted hydrogen.

In a comparatively short time it is possible by this method to collect all the arsenic contained in the most disgusting putrescent animal matter in the form of a well-crystallised compound. After being washed with dilute ammonia (1 to 10*) till the chlorine reaction has disappeared, it is almost quite white.

As it contains sometimes about half its weight of phosphate, and often some calcic sulphate, both derived from the animal substances, merely drying and weighing are not sufficient to reveal the quantity of arsenic it contains.

The best way of treating it further is to dissolve it in dilute sulphuric acid (1 to 5) and to reduce the arsenic pentoxide to trioxide by addition of sodic sulphite. After total expulsion of the sulphurous anhydride by boiling, the arsenic is precipitated by sulphuretted hydrogen.

If the magnesian double salts obtained have been sufficiently pure, the arsenical trisulphide prepared from it is quite free from uncombined sulphur and organic matter, and may be dried and weighed. If not, it is previously heated with strong sulphuric acid and some drops of nitric acid (Gautier†). In co-operation with Mr. H. J. Hamburger I have tried this method on blood mixed with known quantities of arsenic (doses of 3 to 5 grains, as these are generally deemed mortal ones), and compared it with the method that Otto describes in his "Ausführliches Lehrbuch der Anorg. Chem, 4^o, Aufl. III., p. 594.

First Experiment.—267 grms. of blood containing 0.1133 grms. of pure As_2O_3 was treated with HCl and KClO_3 . The filtered liquid was divided into two equal parts. The former part was treated subsequently for ten hours, then for two, and at last for seventeen hours more with sulphuretted hydrogen. Still the filtrate, as I noticed afterwards, contained arsenic. Only 0.05 grm. of pure As_2S_3 was collected, so that only 71.1 per cent of the added arsenic trioxide were found. The latter part was treated after my method. The crude magnesian precipitate obtained, after having been dried at 105° C., weighed 0.2071 grm., and the pure As_2S_3 prepared from it 0.065 grm., corresponding to 0.0523 grm. of As_2O_3 , which proved that 92.4 per cent of the added arsenic trioxide had been found. As from the latter only 0.1025 ammonio-magnesian arseniate could have been formed, the crude precipitate in this case consisted for more than half its weight of phosphate.

Second Experiment.—750 grms. of blood were mixed with 0.3102 grm. of pure As_2O_3 , and treated after my method. The crude magnesian precipitate was not weighed but dissolved, still moist, in diluted sulphuric acid, and the solution diluted to 250 c.c. 100 c.c. of this solution gave 0.1573 grm. of pure As_2S_3 ; another portion of

100 c.c. gave 0.1540 grm. These results proved that in the former part 0.3165, in the latter 0.3097 grm. of As_2O_3 had been found, i.e., respectively 102 and 99.9 per cent of the added quantity.

Third Experiment.—To 1 kilogram. of blood were added 0.32885 grm. of pure As_2O_3 , and the mixture treated scrupulously according to Otto's indications. The clear solution in hydrochloric acid took a volume of 2.39 litres. In one litre of it the arsenic was determined. The total precipitation of all the arsenic trisulphide by sulphuretted hydrogen took several days. The treatment of the crude sulphide after Meyer proved to be a very tedious and lengthy operation. At last the arsenic was weighed as ammonio-magnesian arseniate. This weighed 0.24395 grm., which indicated that 97 per cent of the added As_2O_3 had been found.

This last experiment proves that very fair results may be obtained also by Otto's method when strictly followed. But in this case it takes a good deal of time and trouble. Moreover, it has one serious disadvantage that I escaped by my method, viz., the large quantities of sulphuretted hydrogen that are required. More than one examiner has pointed out the danger of introducing arsenic by using for the evolution of this gas the common ferric sulphide and acid of trade, as they generally contain more or less of this element. But even in using materials quite free from arsenic it is very difficult to note the point when all the arsenic is precipitated by the sulphuretted hydrogen. For this reason I think I may safely recommend my method in cases in which arsenic in organic mixtures is to be determined.

NOTE ON STEEL PLATES.

By SERGIUS KERN, M.E., St. Petersburg.

THE author had an opportunity of making some experiments respecting thick steel plates, rolled out of Bessemer mild steel. The ingots before rolling were hammered; the specimens selected from the plates were crossway samples. The results show the great advantage of annealing the plates, as the author's opinion is, that in plates prepared out of ingot steel it is preferable to have a high elongation, the strength being not less than 23 tons and not more than 27 tons per square inch, especially for boiler plates.

Hammered Ingots.

Carbon	0.155 per cent.
Manganese	0.450 "
Sulphur	0.020 "
Phosphorus	0.010 "
Silicon	0.025 "
Copper	none

The six rolled plates were tested unannealed and annealed.

I.—Unannealed Plates.

Length, 8"; breadth, 1.5".

Samples.	Ultimate Stress. Tons per Square Inch.	Elongation Per cent.
1. }	29.54	24.60
2. } 1" thick ..	29.63	23.50
3. }	28.00	20.00
4. }	31.36	18.80
5. } 7/8" thick ..	26.80	21.30
6. }	30.00	20.50

II.—Annealed Plates.

Length, 8"; breadth, 1.5".

Samples.	Ultimate Stress. Tons per Square Inch.	Elongation Per cent.
1. }	25.53	31.50
2. } 1" thick ..	25.27	29.25
3. }	24.54	30.50
4. }	26.04	30.25
5. } 7/8" thick ..	23.89	31.50
6. }	24.16	32.00

* Probably by a mistake Fresenius in the last, not yet complete, edition of his "Anleitung zur Quant. Chem. Analyse," p. 369, prescribes ammonia of 1 to 3! This is much too strong and dissolves an appreciable quantity of the precipitate.

† *Ann. de Chim. et de Phys.*, [v.], viii., p. 384.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Ordinary Meeting, April 10, 1880.

Prof. FULLER in the Chair.

NEW Members:—Mr. W. O. Smith, Prof. Judd, F.R.S.

A paper on the "*Human Eye as an Automatic Photometer*," by Mr. WILLIAM ACKROYD, F.I.C., was read. It is difficult to get the value of a very intense light in terms of a weak one, because the relative physiological values of the similarly coloured constituents are unknown. The author's experiments were made to show that the eye itself is a fairly good light measurer. When a "spot" or star of light is looked at from a distance, it is seen to emit "rays" or spokes of light at all angles. These are due to the radiate structure of the crystalline lens and the lachrymal fluid on the surface of the cornea of the eye. The rays are of various lengths; and are shorter in the first and second quadrants, next the nose or near the blind spots, than on the third and fourth quadrants, a fact probably due to the insensibility of this region. The iris expands and contracts under the stimulus of light independently of the will, and both irises act sympathetically. Now, the iris lies between the seats of irregular refraction, and thus any change in the size of the pupillary aperture will be rendered evident by an alteration in the length of the longer rays of a spot or point of light. On this fact is based the use of the eye as an automatic photometer. The sensitiveness of the iris varies in different persons. The author finds that a sperm candle burning 120 grms. per hour produces a distinct movement of his iris when 14 yards distant. In employing the eye as a photometer the author adopts the principle that if the light from one source, A, falling on the eye is capable of producing movement of the iris at a distance, d , and the light from a different source, B, is capable of producing the same movement at the distance d' , then the relative intensities can be found from the squares of these distances. To carry this out in practice the observer is in the dark, and an artificial star is placed on a level with the eyes at a fixed distance. Below this place the light to be tested in the same plane. While gazing steadily at the star the other light is to be eclipsed and revealed, and the observer is to find a position where the revealing of the second light does not influence his iris, as shown by no apparent shortening of the rays of the star taking place. He then approaches gradually till a second position is reached, where the revealing of the second light *does* produce a movement of the iris. The distance between this position and the light, d , is measured. A third light is now put in place of the second, and the same observations repeated so as to get a second distance, d' . From these distances the relative intensities are calculated. The author's results agree pretty closely with those of Rumford's photometer; but he found that for some reason the first two observations have to be discarded as too inaccurate. Owing to the sympathy between the two irises these experiments were binocular. This sympathy may prove convenient in constructing an eye-photometer, since one eye can be turned to the light to be estimated, while the other is looking at the artificial star. This method of photometry would be too delicate for comparing powerful electric lights unless aided by mechanical means, which, however, the author hopes to supply.

Prof. AYRTON then offered an explanation of the experiment shown by Prof. Guthrie at last meeting to the effect that while flannel rubbed with ebonite was + electrified, and ebonite rubbed with glass was +, flannel rubbed with glass was -. Prof. Ayrton accounted for this apparent anomaly on the grounds that one or more of the substances was an electrolyte. Glass, for instance, is an electrolyte, and a battery had been made from it.

Experiments made by Prof. Perry and himself had shown that in a "pile" made up of divers substances, one or more of which were electrolytes, though the rest were metals, the electromotive force of the pile was equivalent to the algebraical sum of the several "pairs" composing it; but it was not equivalent to the electromotive force of the first and final plates made into a pair. That could not be predicted from the contact electromotive forces of the elementary pairs. When only metals were employed it could, but not in cases where an electrolyte entered. This same result would apply to Prof. Guthrie's frictional experiments. In answer to Prof. Guthrie's question whether electrolysis did not come into play in Prof. Ayrton's experiments, Prof. Ayrton replied that it could not operate to a greater extent than in Prof. Guthrie's experiments, as he had used a quadrant electrometer.

Dr. STONE then described a new tonometer, devised by Prof. Rudolf König, which he had recently seen in Paris. It consisted of a clockwork working into a tuning-fork, which produced no less than 128 escapes per second. To this clockwork, originally invented by an assistant of M. Breguet, and exhibited at the Paris Exhibition of 1856, Prof. König has added a Helmholtz vibration microscope moved by the clock, and the fork whose vibration-number is to be measured is placed vertically in the focus of the microscope. The apparatus is very portable and no loading of the forks is required.

Prof. HUGHES observed that he had patented a vibrating regulator in 1856.

Dr. GUTHRIE then exhibited an electric machine, formed of a collodion disk rubbed with catsfur and giving negative sparks. The collodion, after a suggestion of Capt. Abney, was put on by giving a disk a coat of collodion, then a coat of india-rubber dissolved in benzol, then a coat of collodion again. Prof. Guthrie also showed that an iron cylinder revolving round its longer axis, and with a current flowing in a wire parallel to it, has power to deflect a magnetic needle.

Prof. AYRTON stated that he had found the mere rotation of an iron cylinder produce the deflection in question, and therefore thought the current was not required to produce the effect shown.

NEWCASTLE-UPON-TYNE CHEMICAL SOCIETY

General Meeting, February 26, 1880.

Mr. JOHN GLOVER in the Chair.

THE minutes of last meeting were read and confirmed.

Mr. W. Beer and Mr. T. T. Sill were elected members of the Society.

The names of Mr. W. B. Campbell and Mr. W. B. Jackson were read for the first time.

THE CHAIRMAN—The next business is the discussion on Dr. Lunge's paper on the "Composition and Analysis of Weldon Mud." It is a most interesting paper, and reflects great credit on Dr. Lunge. We shall be glad to hear the remarks of any member on the subject.

Mr. JOHN PATTINSON—Dr. Lunge has furnished us with a paper which possesses much interest for many of the members of this Society. I have nothing to say about the relative merits of Bunsen's iodometric process and the permanganate and bichromate processes of determining oxygen in manganese oxides, further than that all the processes are probably capable of yielding accurate results when the operator, by experience, has learnt the best methods of using each process. For my own part, having had most experience with the bichromate process, I have most confidence in results obtained by it, and prefer it to either of the other processes.

I must, however, take exception to Dr. Lunge's statement on page 346, that calcium chloride answers the same purpose as ferric or zinc chloride in causing the whole of the manganese in a solution to be precipitated as peroxide

when the solution is boiled with solution of bleaching powder. About a couple of years ago I was very anxious to supersede, by a volumetric method, the troublesome and incorrect gravimetric methods then in use, for determining the total manganese in ores and other substances, and one of the essential requisites of such a process was that I should obtain the whole of the manganese in a definite state of oxidation, preferably as the peroxide. At that time I made a great number of experiments for this purpose, using known quantities of very carefully prepared pure protosquioxide of manganese (Mn_3O_4). Now I knew that many of the text-books state that a solution of bleaching-powder precipitates manganese from its solutions as MnO_2 , and I of course tried this method very thoroughly; but I found that, treat the solutions as I would, there was invariably a deficiency of manganese indicated when calculating the amount of manganese from the amount of peroxide found in the precipitate. In most of these experiments there was considerable excess of calcium chloride present when the precipitation occurred—at least fifteen equivalents calcium chloride to one of manganese. On referring to my note-book, I find that the average percentage of manganese obtained as peroxide in 61 tests made in presence of this excess of calcium chloride was 97.9 per cent of the total manganese present, the highest result being 99.37 per cent and the lowest 96 per cent. These experiments were made with the solutions at various temperatures, from 60° to 212° F. Two precipitations made when the solutions were boiling indicated respectively 97.31 and 97.49 per cent of the manganese precipitated as peroxide. When, however, ferric chloride is present in about equal quantity with the amount of manganese chloride, and the other conditions fulfilled, which are fully described in my paper read before the Chemical Society (see *Journal of the Chemical Society*, vol. xxxv., page 365), then the whole of the manganese is invariably obtained as peroxide.

I find by a careful perusal of Dr. Lunge's paper that he has not even tried the method I have proposed. He says at page 346:—"But in our Weldon mud it is not even necessary to add the salts mentioned by Mr. Pattinson, for there is always quite a sufficient quantity of calcium chloride present to satisfy the requirements of the case; and we may assume from the outset that Mr. Weldon's prescription as it stands will give perfectly correct results. I have therefore directed my experiments to this line; to find out how far the process could be simplified without taking away from its accuracy." He then describes some experiments with equal quantities of Weldon mud, which of course contained an unknown quantity of manganese, the results of which experiments show that the same amount of manganese was indicated under the circumstances described. Then, on page 347, Dr. Lunge says:—"Lastly, 5 c.c. was supersaturated with HCl, bleach liquor, water and calcium carbonate added, just as described by Mr. Pattinson. Result of two testings again: 8.4 c.c."—that is, the same result as the other experiments gave. Now, this is not the method which I have described as being essential to success. It is like playing "Hamlet" with the part of Hamlet omitted. The all-important part of the process is having a sufficient quantity of ferric chloride present when the precipitation takes place, and this Dr. Lunge has entirely omitted. Had Dr. Lunge tried a portion by the method I have described, I have no doubt he would have had a higher amount of manganese indicated than in his other experiments. Since Dr. Lunge's paper was read, to make assurance doubly sure, I have had the following three experiments made under the conditions I have found to give the best results:—

No. 1. With no calcium chloride present other than was in the bleach solution, and that formed by the action of the carbonate of lime on the chloride of manganese. No ferric chloride present.

No. 2. With 10 equivalents of calcium chloride in addition to the other calcium chloride, but no iron present.

No. 3. With 30 equivalents of calcium chloride in addition to the other, but no iron present.

No. 4. About 15 equivalents of calcium chloride present and one equivalent of iron as perchloride.

The following results were obtained:—

No. 1.	98.49	per cent of the Mn obtained as MnO_2 .		
No. 2.	99.04	do.	do.	do.
No. 3.	99.04	do.	do.	do.
No. 4.	99.95	do.	do.	do.

Thus showing, although a very large excess of calcium chloride is present, yet that, unless ferric chloride is also present, the whole of the manganese is not obtained as MnO_2 .

I quite admit that for manufacturing purposes the tests obtained without the addition of ferric or zinc chloride may be sufficiently exact; but this is certainly not the case for tests which have to decide the value of cargoes of say 1000 tons of ore, in which the difference of half a per cent in the manganese will make a difference of about £40 in price to be paid and received.

I think I have given abundant evidence to show that I am justified in coming to the conclusion that Dr. Lunge is wrong in assuming that chloride of calcium plays the part of ferric and zinc chloride in determining the precipitation of all manganese as peroxide under the circumstances described, and that the method he describes for determining total manganese in Weldon mud does not indicate the whole of the manganese.

I may mention here that I do not recommend the use of zinc chloride or zinc oxide in the place of ferric chloride and calcium carbonate when bichromate solution is used to titrate the remaining ferrous salt, for I find that the zinc very seriously interferes with the distinctness of the end reaction with solution of red prussiate of potash.

Mr. LAIDLER—I have also made a number of experiments to ascertain the effect of chloride of calcium in determining manganese by precipitating as peroxide with solution of bleaching-powder, and always found that the manganese was never precipitated entirely in the form of peroxide; but when ferric chloride is present when the precipitation takes place the results are always accurate, being never more than one or two hundredths of a per cent above or below the 100 parts.

On page 343, Dr. Lunge, in experimenting with Volhard's process, obtained from 5 c.c. of mud, with the addition of 2 grms. ZnSO_4 , 0.1757 grm. of manganese, and from the same quantity of mud without zinc 0.1765 grm. manganese. Now, if the latter experiment is correct, as Dr. Lunge proves, he has obtained 0.5 per cent more manganese without zinc than with it, which, I think, is quite contrary to the experience of other experimenters. From this it seems to me that the method of measuring the mud by means of a pipette is not sufficiently accurate to decide the question as to the part played by zinc, and I think some better method of obtaining two identical quantities of such a substance as Weldon mud should have been resorted to than measuring with a small 5 c.c. pipette.

I should also like to ask if any member has tried Mr. Pattinson's process in its entirety in the estimation of the total manganese in Weldon mud; and if so, what were the results obtained as compared with the usual method of testing?

Mr. GLENDINNING—Since last meeting my attention has been drawn to the influence of CaCl_2 on thick batches. I caused three batches to be worked containing respectively 2, 1.9, and 1.8 equivalents of CaCl_2 to 1 of MnCl_2 . The first two gave very good results, both as to manganese recovered and the quantity of base. The third gave a batch inclined to be thick. Those experienced in the matter know that when the slaty appearance, which is indicative of thickness, first makes its appearance, a very little addition of MnCl_2 is sufficient to cure it. So far as these results go, deficiency of CaCl_2 seems to be in some measure the cause of thick batches; and this is supported by the fact that such batches are frequently obtained at

the commencement of a Weldon process, and also when liquor from extra well-settled mud is used, in both of which cases the proportion of CaCl_2 to MnCl_2 will be low as compared with ordinary liquors.

An opinion which is pretty generally held is that excess of lime is the cause of thick batches, but analysis has shown that the quantity of lime is not necessarily excessive in such cases.

Dr. Lunge has advised the use of less heat as a means of avoiding thick batches; but as the most obvious action of CaCl_2 in the process is its solvent action upon lime, and as a high rather than a low temperature favours this action, therefore working at low temperature appears to be tantamount to working with a less proportion of CaCl_2 .

Dr. AFFLECK—I have no experience with Weldon mud myself, but should like to ask whether any members agree with Dr. Lunge's opponent in considering Weldon mud to be a mere mixture and not a chemical compound?

Mr. GLENDINNING—I have a full analysis of the mud, made at a very early period of the history of the process, so that it compares very unfavourably with the batches at present made, but it will show the very small amount of lime present in the mud:—

MnO	0.405	equivalents.
CaO	0.282	"
MgO	0.329	"
CaCO_3	0.063	"
Fe_2O_3	0.033	"
Al_2O_3	0.044	"
Base	1.156	"

The CHAIRMAN—Do you infer, then, that the mud is a mixture?

Mr. STUART—Mr. Weldon holds the opinion that the mud is an unquestionable salt—is an acid manganite of calcium consisting of one equivalent of CaO combined with two equivalents of MnO_2 , and in which the base is 0.5. The proportion of CaCl_2 to MnCl_2 , mentioned by Dr. Lunge, is, I confess, new to me, but well worthy of further investigation.

Mr. GLENDINNING—In our ordinary working the proportion of CaCl_2 is much about the same; perhaps rather less.

Mr. PATTINSON—I have not studied the question of the nature of Weldon mud very closely, but it may be interesting to point out that MnO_2 has a very strong tendency to carry down with it very considerable quantities of bases from solution. In making experiments on the estimation of total manganese I found it carry down almost every base, particularly baryta and potash; and Wright and Luff point out that considerable quantities of potash are taken down with MnO_2 on reducing KMnO_4 with alcohol.

Mr. STUART—Mr. Weldon has clearly demonstrated the fact that hydrated MnO_2 combines easily with all the alkalies and alkaline earths to form salts with these bases.

Dr. AFFLECK—Has any one tried precipitating manganese liquors with caustic soda, and does the same excess require to be used as in the case of lime?

Mr. STUART—Mr. Weldon has tried it and finds it very suitable. An equivalent is used to precipitate, and then the same excess is used as of lime to secure peroxidation.

Dr. AFFLECK—If the mud is a definite compound we should expect the same amount of soda to be required as of lime; but if a mixture, and if oxidation takes place simply when the solution is sufficiently alkaline, it ought to require a larger excess of lime than of caustic soda.

The CHAIRMAN—Probably Mr. Weldon will take up this point next month. I am sure we have had a most interesting discussion.

A Method of Producing Acetal.—MM. Engel and de Girard.—The authors obtain acetal in considerable proportions by passing a current of phosphuretted hydrogen into a cold mixture of alcohol and aldehyd.—*Comptes Rendus*.

CORRESPONDENCE.

COMPOSITION OF "WELDON MUD."

To the Editor of the Chemical News.

SIR,—As the session of the Newcastle-upon-Tyne Chemical Society is now over, I beg to trouble you with a few remarks concerning the discussion of my paper on Weldon mud, read before that Society.

The reason why I assumed from the outset that Mr. Weldon's prescription for estimating the total manganese as it stands, yields correct results, was that there is a very large excess of calcium chloride present. Whilst Mr. Pattinson recommends ferric chloride, and Kessler zinc chloride, Volhard has proved that calcium or barium salts equally prevent the precipitation of MnO along with MnO_2 .

I had, on repeating Volhard's process, found that the calcium chloride present in Weldon mud is sufficient for the same purpose, so that the addition of zinc salt to the solution makes no difference in the result of the test. It is true that in Volhard's and Pattinson's methods the precipitation of MnO_2 does not take place in the same way, viz., by potassium permanganate in the former, by bleaching-powder in the latter; but as there exists hitherto only one explanation for the formation of MnO along with MnO_2 in those cases, viz., the acid properties of MnO_2 , I assumed, perhaps too rashly, that calcium chloride would have the same effect in Pattinson's as it has in Volhard's test. Now, Mr. Pattinson adduces experiments according to which, even with an excess of calcium chloride, 0.9 per cent Mn less is obtained than by adding ferric chloride. Probably a larger number of experiments would be desirable to settle this point definitely, but I do not mean to go into it at all, at least just now, for even taking it for granted I should say that it is of little consequence for my present purpose. An uncertainty as to one hundredth part of the amount of manganese may be of importance in a 1000 ton cargo of iron ore, as exemplified by Mr. Pattinson; but not in the case to which *alone* my paper referred, viz., the testing of Weldon mud. Here it is, in the first instance, quite sufficient to know the percentage of Mn to 1 per cent of the total, and, secondly, the only practicable method of taking the samples, viz., by means of a pipette, does not in itself admit of absolute accuracy, so that analytical processes of extreme refinement are out of place here.

Dr. Affleck has quite erroneously attributed to me the opinion that Weldon mud is a mere mixture, and not a chemical compound. This is the opinion of Dr. Post, from which I most decidedly dissent. In the present state of our knowledge I consider the acid properties of manganese peroxide fully proved, although I think it likely that its formula may not be simply MnO_2 , but a polymer of this; from those acid properties I cannot but conclude that in Weldon mud there must be basic oxides in chemical combination, not merely in mechanical mixture, with manganese peroxide.—I am, &c.,

GEORGE LUNGE.

Zürich, March 29, 1880.

THE DIRECT METHOD FOR TESTING VITRIOL CHAMBER EXITS.

To the Editor of the Chemical News.

SIR,—In my letter (see CHEMICAL NEWS, vol. xli., p. 70) I confined myself to stating facts. Mr. Vasey (p. 92) replied, not by a fair argument, but by an insinuation to the effect that my work was "incorrect" and "unreliable." On p. 145, Mr. Vasey admits the truth of the statements made in my letter, as will be seen by comparison, but he does not withdraw the insinuation. I therefore reply to his letter (p. 92) as follows:—

- (1.) That the opinion which he says I copied from him was not his originally.
- (2.) That I was present at the meeting of the Faraday Club in January, 1879, and I did hear Mr. Davis read the paper referred to.
- (3.) That I was too busy with other reforms when I became manager here to attend to introducing the nitrometer test.

—I am, &c.,

W. JESSE LOVETT.

Victoria Chemical Works, Bradford,
Manchester, April 8, 1880.

PEROXIDE OF HYDROGEN AND OZONE.

To the Editor of the Chemical News.

SIR,—In a letter (CHEMICAL NEWS, vol. xxxviii., p. 243) addressed to you some time ago, upon the subject of a paper by Mr. Albert R. Leeds, which had appeared the week before in your excellent journal, I wrote as follows:—"The author seems to have fallen into an error committed by many other observers, namely, that of estimating as ozone everything in the atmosphere capable of colouring so-called ozonoscopic papers."

After a silence of more than twelve months Mr. Leeds has, in your last issue, taken me to task for this "unwarranted charge," and I shall feel obliged if you will now allow me to make an explanation. In his paper Mr. Leeds gave a long table, which was headed "Register of Ozone Observations made at Upper Saranac Lake," &c., and in one column, with the heading "Ozone," he gave the degrees registered at different places. No mention was made of peroxide of hydrogen in this part of the communication; and not having the remotest idea of what might follow, I ventured to call attention to what seemed to me to be a serious defect in the observations. Indeed I was anxious to point out the necessity of looking for peroxide of hydrogen in the atmosphere surrounding "hemlocks, pines, larches, and spruces." It is not to be denied that Mr. Leeds did, in a subsequent communication, show the ozonoscopes to be affected by this reagent, but still he did not use this further observation towards explaining the results in which I was specially interested; and on again reading his paper I cannot resist the impression that the table in question was intended, generally, to set forth the amount of ozone in the atmosphere at different places.

As for misquoting Mr. Leeds, I made neither quotation nor misquotation, as will be seen on referring to my letter. Mr. Leeds may rest assured that I entertain the most lively appreciation of his investigations, which are cited in more than one place in my new work entitled "Nature's Hygiene."

To proceed now to another matter mentioned by Mr. Leeds. If we exclude, for the time being, the oxidation of phosphorus, I must confess that I am not aware of any known process of slow oxidation which has been established to produce ozone, contravene as this confession may "the statements of those who have experimentally investigated the subject during the past forty years." But whose investigations, and what slow oxidations, would Mr. Leeds have me bear in mind?

It is quite true that, in my further critical note (CHEMICAL NEWS, vol. xl., p. 96), as Mr. Leeds says, I adduced "no new and crucial experiments," but at the same time I was able to refer to some such experiments previously published by me, and I have made a considerable number of other experiments which I do not intend to publish, because in their incompleteness they resemble some which have been published by others.

As regards, however, the oxidation of phosphorus, I have had some special apparatus made for studying this process, and hope shortly to be able to say something more certain about it and its results.

Although I have raised doubts as to the formation of

ozone during the oxidation of phosphorus, I shall be quite prepared to believe that ozone is formed should the necessary evidence be forthcoming, but in the meantime I cannot attach much chemical weight to so imponderable a thing as an odour.

The other day I made the first experiment with my new apparatus, in which a quantity of phosphorus was submitted to atmospheric oxidation in five successive vessels. The gas was washed, and then passed into acidified iodide of potassium. The iodine liberated during a certain time required 0.8 c.c. of a decinormal solution of sodic thiosulphate, while the water in which the phosphorus was half submerged liberated iodine equal to 2.2 c.c. of the same thiosulphate solution.

As for Lamont's theory regarding the assumed formation of ozone, it is faulty in not explaining the necessity of the presence of water during the oxidation of phosphorus.

Finally, it will be seen that Mr. Leeds confirms a statement often made by me, viz., that it is very difficult to remove, by washing, peroxide of hydrogen suspended in an aerial current.

After all, the subject merely calls for more experiments, which in their turn demand time and resources, and while I hope that both Mr. Leeds and Prof. McLeod can command more of these things than I can, I will do my best in the matter.—I am, &c.,

C. T. KINGZETT.

12, Auriol Road, West Kensington, W.

EDISON'S ELECTRIC LIGHT.

To the Editor of the Chemical News.

SIR,—As I judge from your last letter that you would be interested in the information, I will give you the result of some measurements recently made by Prof. A. M. Mayer, Ph.D., Mr. B. F. Thomas, Ph.D., and myself, on one of Edison's new carbon lamps. The lamp in question was kindly loaned to Prof. Mayer by Mr. E. A. Beach, Editor of the *Scientific American*.

We found, in the first place, that the electrical resistance of the carbon loop or "horse-shoe" rapidly decreased at high temperatures, so that while it was equal to 123 ohms at the ordinary temperature, it fell to about 76 ohms when so hot that the lamp gave a light of about 15 candles. Using a battery of 50 Bunsen cells as a source of current, and placing a "copper voltameter" (i.e., plates of copper in concentrated solution of cupric sulphate), a galvanometer, and the lamp in the same circuit, we were enabled readily to measure the actual current when the lamp was giving a light of 15 candles. This was almost 1 volt., or exactly 0.905 of a volt. Such a current through a resistance of 76 ohms would of course convert therein, into heat and light, an amount of energy which, expressed in foot-pounds, would be about 2750 ft.-lbs. In other words, about one-twelfth of a horse-power. Twelve such lights could therefore theoretically be operated by one horse-power of energy in the shape of an electric current. In other words, with such a lamp the theoretical maximum of conversion into light is 12 burners of 15 candles each, or 180 candles per horse-power of energy in the electric current.

With the best form of dynamo-electric machine and "arc" lamp which I have tried in my Light House Board experiments I have obtained 1800 candles per horse-power of mechanical force applied to the driving-pulley of the machine. The theoretical maximum of Mr. Edison's lamp is thus ten times less than the practical efficiency of the arc light.

These results agree entirely with those obtained by Profs. Barker, Rowland, Young, and Brackett, who, as I see from to-day's papers, have been making similar measurements for Mr. Edison, at Menlo Park. Mr. Edison's lamp, therefore, like all previous efforts to employ incan.

descent conductors, involves an enormous loss of energy as compared with the arc light.

The numerous other questions bearing upon its commercial value I will not allude to here.—I am, &c.,

HENRY MORTON.

Stevens Institute of Technology,
Hoboken, New Jersey,
March 25, 1880.

ALKALIMETRIC DETERMINATION OF SULPHATES.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 158, Mr. Otto Hehner points out that the method for the alkalimetric determination of sulphates which I published on p. 114 of the same paper is not new, but has been originally proposed by M. F. Jean. Mr. Hehner says: "It would surely be a simple matter to look over the indices of a few volumes of the CHEMICAL NEWS, or of any other chemical journal, before describing and publishing methods as new." I fully admit the truth of this sentence, and am sorry that in trusting too much to my memory with regard to the analytical literature of the last few years I overlooked M. Jean's and M. Haubst's papers. But whilst giving M. Jean every credit due to him for having first published the principle of the method, I must insist on claiming that the details of my paper are entirely my own, and differ widely from Jean's details. In his original paper (*Comptes Rendus*, 83), Jean says: "I have assured myself that on working with sufficiently dilute solutions (*solutions peu concentrées*) and under the conditions indicated above, the alkaline carbonates do not decompose the sulphate of baryta." I have in vain looked in that paper for the "conditions indicated above," neither have I been able to find out what solutions M. Jean considers to be "sufficiently dilute." In that paper, nearly the whole of which, by the way, is given in the extract in the CHEMICAL NEWS (vol. xxxiv., p. 272), there is not one figure of analytical results given. In my paper I have worked out the method for one dilution and given the results which I found. The dilution is such as will occur most frequently in technical work. In that dilution I have found that sodium carbonate does sometimes decompose barium sulphate. Without having any knowledge of M. Jean's paper, I naturally tried a plan similar to his when I started my experiments, it being the shortest way, and I had to give it up as I found it not to be trustworthy.

In a paper published in conjunction with M. Pellet (*Bull. Soc. Chim.*, vol. xxvii.), and dealing with the analyses of the most important sulphates and their mixtures, all the analytical data that M. Jean gives are two analyses, not of mixtures, but of sulphates by themselves. Though one of these results, in which he used 0.112 gr. and found 0.110 gr. of potassium sulphate, is 1.8 per cent too low, he says:—"This method gives very exact results (*des résultats très-exacts*). A reaction, however simple, comprising the analysis of so many substances requires more than two analytical data to be established as a method, and M. Jean can hardly claim more than having proposed the principle of the method.

M. Jean mentions nothing of impurities in baryta, and yet it is well known to every chemist that it is impossible to buy pure baryta and very difficult to prepare it. The use of the method in technical laboratories, where one cannot always prepare one's own pure chemicals, is out of the question unless chemists are enabled to work with impure material such as they can purchase. I have shown in my paper how to do this, and also how to do away with the washing of the precipitates. I think I have now shown clearly enough that even if I had known M. Jean's paper I should have published my paper the same as I did, only altering the title. After the sentence quoted above from Mr. Hehner's letter, that gentleman goes on to say:—"Much time might thus be saved to reading chemists and

confusion avoided." I cannot admit that this sentence has any bearing on my paper. If Mr. Hehner had taken the trouble to read my paper he would very likely have left out his general remarks, and his letter would be no worse for it.—I am, &c.,

J. GROSSMANN.

St. Helens, Lancashire,
April 10, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 11, March 15, 1880.

Reply to the Observations of M. Berthelot concerning Chloral Hydrate.—A. Wurtz.—M. Berthelot admits in his last paper that "everything leads us to believe that chloral hydrate is dissociated in part at 100°, and that the decomposition tends to become complete at 100°, and at a low pressure." This opinion approximating closely to what M. Wurtz has maintained, it is needless to prolong the discussion.

Action of Oxygenated Water upon Silver Oxide and upon Metallic Silver.—M. Berthelot.—When oxygenated water is brought in contact with silver oxide reduction takes place, but the volume of oxygen set free is merely equal to that present in the oxygenated water. The residual matter contains the whole, both of the silver and the oxygen originally present, but the distribution of these elements has been changed under the action of the hydrogen peroxide, so as to represent a mechanical mixture, in which one-third of the silver is free and the corresponding proportion of oxygen converts the residue of the silver oxide into sesquioxide.

On a Microphonic Apparatus receiving Words at a Distance.—MM. P. Bert and d'Arsonville.—The authors, in the course of their researches on deafness, have attempted to construct a micro-telephone, which should solve the following double problem:—On the one hand, strengthen the sound-vibrations of speech, which the telephone merely weakens, and on the other hand, collect these vibrations at the distance of several metres from the speaker. The latter portion of this task they consider as accomplished.

The Economical Yield of Electromotors, and the Measure of the Quantity of Energy which Traverses an Electric Circuit.—M. Marcel Deprez.—The quantity of energy is proportional to the product of the intensities of the two currents, or to the product of the deflections of the needles.

Application of the Telephone to the Measure of the Torsion of the Motor Shaft of Moving Machinery.—C. Resio.—The author proposes an arrangement for measuring the torsion of a moving shaft, and consequently the effort and the work transmitted to such shaft.

A Process for the Measurement of Elevated Temperatures.—J. M. Crafts and F. Meier.—This paper does not admit of useful abstraction.

Electrolysis of Malonic Acid.—E. Bourgoïn.—In the free state this acid is very stable as regards the current, and becomes concentrated in the positive compartment—a double character, which distinguishes it from its lower homologue, oxalic acid. In a concentrated and alkaline solution it does not, in any case, give rise to a carbide of hydrogen, which distinguishes it from succinic acid. These differences are such that if free methylen had not hitherto escaped from all researches, we should be tempted to believe that synthetic malonic acid is not the true homologue of oxalic and succinic acids.

Synthesis of Ulmic Bodies.—A. Millot.—On passing an electric current into acidulated water, taking for the negative electrode a plate of platinum, and for the positive electrode a piece of gas-coke, the coke is quickly disaggregated and falls to the bottom as a powder. If an alkaline solution is substituted for the acid, the coke is still broken up, but a portion dissolves in the liquid, which takes an intense black colour. The author used an ammoniacal solution subjected to the action of a thermoelectric battery, about equal to two Bunsen elements. The black liquid obtained is precipitated by mineral acids, when a substance is produced analogous to the nitrogenous glucose described by M. Thenard. The precipitate is entirely soluble in water when the acid has been removed by washing, but after desiccation at 150° it no longer dissolves in water. It does not yield ammonia if boiled with concentrated potassa, but if heated with potassa it gives rise to potassic cyanide. The coke used had been purified by means of a current of coke at a red heat for thirty hours, a time more than sufficient for the removal of the hydrocarbons.

The Products of the Splitting up of Proteic Bodies.—M. Bleunard.—The author has formerly shown that the organic matter of stag's horn is decomposed by hydrate of baryta in a manner analogous to albumin, yielding, in addition to ammonia and carbonic and oxalic acids, an amidic mixture, answering approximately to the general formula $C_nH_{2n}N_2O_4$, the value of n being lower than in the amidic mixture from albumin. Further researches have shown that the chief part of the mixture consisted of a gluco-protein, answering to the formula $C_6H_{12}N_2O_4$.

Artificial Production of Felspars with Barytic, Strontic, and Plumbic Bases, Corresponding to Oligoclase, Labradorite, and Anorthite.—F. Fouqué and A. Lévy.—The authors describe the proportions of the ingredients and the procedures employed.

Examination of Volcanic Dust falling January 4, 1880, at Dominica.—M. Daubrée.—This dust is remarkable as containing a considerable proportion of iron pyrites and of galena.

Separation of Minerals heavier than Quartz by means of a Melted Mixture of Lead and Zinc Chlorides.—R. Bréon.—The specific gravity of liquid lead chloride is 5, and that of zinc chloride 2.4. By mixing these two bodies in variable proportions, liquids may be formed ranging between these two limits, in which certain portions of a mineral powder will sink, whilst others float on the surface.

Composition of the Mineral Waters of Bussang (Vosges).—E. Willm.—These waters contain iron, manganese, and arsenic, and calcium phosphate, borate, and fluoride.

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Chemiker Zeitung.
No. 6.

Determination of Sulphur in Pyrites.—G. Lunge.—The author maintains that his process, though yielding results less absolutely accurate than those obtained by the method of Fresenius, is yet preferable for use in chemical works where time is the first object. He thinks that the larger proportion of sulphur obtained by the process of Fresenius is partly due to the fact, that both galena and heavy spar, bodies frequently found in pyrites—are attacked by the dry process.

Determination of Manganese and Cobalt.—C. Rössler.—The cobaltous salt is mixed with a measured quantity of a silver solution of known strength, the liquid is heated on the water-bath and precipitated with potassa lye (not with sodium carbonate). There is formed a deep black precipitate of the composition $Ag_4OCo_2O_3$. In other respects the process is conducted as for manganese. If nickel is simultaneously present, the results are too low. More accurate results are obtained, if from the nitric solu-

tion of the two metals the cobalt is first separated by means of potassium nitrite; the precipitate, filtered off without washing, is then re-dissolved in nitric acid, and the cobalt is determined volumetrically.

Experimental Researches on Infection with Town Sewage and Water from Manufactories.—Dr. R. Emmerich.—Acid, recently voided urine is, according to the author, harmless as water, and there is hence no occasion to forbid its discharge into large rivers. As urine in a few days after emission becomes intensely poisonous, its retention in the vicinity of human habitations and its infiltration into the subsoil of towns should be pronounced dangerous. Human solid excrement immediately after its discharge produced deadly effects if suspended in water and injected subcutaneously. But if diluted with 20,000 parts of water, it can be injected even to the extent of 60 c.c. without occasioning injury. Hence water containing excrement in such proportions may be used for drinking! [How if choleraic or typhoid germs are present? *Ed. C. N.*] Into rapid streams, where the dilution must be at least 100,000-fold, sewage may be discharged without hesitation.—*Bayr. Akad. d. Wissenschaft.*

—
Annalen der Physik und Chemie.
Heft 3, No. 11, 1879.

The Law of Stokes.—E. Hagenbach.—After a prolonged investigation the author concludes that at present there exists no reason for rejecting the law of Stokes, and that Lommel's classification of fluorescent bodies is not founded upon any essential difference in their behaviour.

Experimental Researches for Determining the Index of Refraction of Liquefied Gases.—L. Bleekrode.—The author's results are given in the form of tables.

The True Theory of Fresnel's Interference-Phenomena.—H. F. Weber.—Fresnel asserted that observation and measurement confirmed all the deductions which he drew from his theory. This agreement between theory and fact does not exist; a close examination of Fresnel's interferences makes it manifest that not one of his theoretical deductions agrees with the phenomena. It is therefore to be wished that all researches which have been based upon this theory, *e.g.*, the experiments of Töpler and Boltzmann on the vibrations of the air, might undergo a revision.

Influence of Temperature upon Tuning-Forks.—H. Kayser.—The author's results are as follows:—(1.) The number of vibrations of a tuning-fork is not unaffected by the temperature, of which, between 0° and 30° , it is a lineal function. (2.) The influence of the temperature is greater the higher the tone of the fork. (3.) In moderate changes of temperature, such as may occur in a dwelling room, the number of vibrations is affected in the second decimal place. (4.) The coefficient of elasticity of steel increases with the temperature between 0° and 30° .

The Galvanic Conductivity of Metallic Alloys.—Emil Elsässer.—The author enumerates a series of alloys which conduct without undergoing decomposition. From these composite conductors to the electrolytes the transition is gradual, and between the two groups are substances which conduct at low temperatures without decomposition, but undergo electrolysis at higher temperatures, and in part even before they are melted. A third class of bodies have not undergone electrolysis because they are too infusible, and a fourth class of compound bodies conduct neither with nor without decomposition. It would be important to determine for each element with which of its combinations electrolytic conductivity begins.

Electric Perforation of Glass.—E. Mach and S. Doubrava.—The authors found an explanation of Prof. von Waltenhofen's experiment on the fact that the glass surface in comparison with a stearin surface must be regarded as conductive.

Electric Perforation of Glass.—A. von Waltenhofen.—This paper requires the accompanying illustrations.

Movement of Plates between the Electrodes of the Holtz Machine.—S. Doubrava.—The author describes a number of experiments undertaken with a view to the explanation of the above-mentioned phenomena.

Relations between Galvanic Resistance and Specific Heat.—Felix Auerbach.—A mathematical paper, not susceptible of useful abstraction.

Currents of the Gramme Machine.—O. E. Meyer and F. Auerbach.—This memoir cannot be intelligibly abstracted without the accompanying illustrations.

Description of Experiments undertaken with Electricity of High Tension by means of the Telephone.—L. Weber.—These experiments seem to support the assumption that the part played by an electrolyte in an induction circuit consists not alone in a condensing activity of the molecules, but in a real conductivity, which must be regarded either as electrolytic convection or as a purely galvanic process.

NOTES AND QUERIES.

Keeping Salts of Ammonia.—Can any of your numerous readers inform the writer whether large stocks of sulphate and muriate of ammonia lose weight by keeping in dry sheds for twelve or eighteen months? The opinion of parties who can speak from absolute experience is respectfully asked for.—H. J. BLAND.

Formic Acid.—I note in an article in the *Journal of Science* for April, page 227, that "it is found that if carbonic oxide . . . is passed over caustic soda and lime . . . formic acid is generated in such abundance that, if required, it would be available for industrial purposes." Can you inform me of any published account of the process referred to? If this acid could be produced at a sufficiently low price I could use large quantities of it. The cheapest means of making it hitherto known—viz., by distilling oxalic acid in presence of glycerin—is too costly for the purpose required.—F. G. L.

Valuation of Soda-Ash.—(Reply to "K").—The difference between the desired strength and the strength of the strong alkali gives the quantity of the weak alkali (kelp, salt, &c.) that must be used; and the difference between the desired strength and the strength of the weak alkali (kelp, salt, &c.) gives the proportionate quantity of strong alkali that must be used. It is done at a glance. Thus, suppose we want to reduce 52 per cent ash to 48 per cent with 11 per cent kelp salt. This gives 4 parts kelp salt to 37 parts of the 52 per cent ash: practically, 1 shovelful to 9.

$$\begin{array}{c} 52 = 37 \\ \quad \quad \quad \backslash \\ \quad \quad \quad 48 \\ \quad \quad \quad / \\ 11 = 4 \end{array}$$

—F. E. V.

MEETINGS FOR THE WEEK

MONDAY, 19th.—Medical, 8.30.

— Society of Arts, 8. Robert W. Edis, F.S.A., "The Decoration and Furniture of Town Houses."

TUESDAY, 20th.—Civil Engineers, 8.

— Zoological, 8.30.

— Pathological, 8.30.

— Royal Institution, 3. Mr. R. H. Scott, "Wind and Weather."

WEDNESDAY, 21st.—Society of Arts, 8. Mr. Holman Hunt, "The Present System of Obtaining Materials in use by Artist Painters, as Compared with that of the Old Masters."

— Meteorological, 7. Discussion on Mr. W. Ellis's paper "On the Greenwich Sunshine Records 1876-80." G. M. Whipple, B.Sc., F.R.A.S., F.M.S., "On the Rate at which Barometric Changes Traverse the British Isles." J. W. Zambra, F.M.S., "A New Form of Six's Self-Registering Thermometer."

THURSDAY, 22nd.—Royal Society Club, 6.30.

— Royal, 8.30.

— Zoological, 4.

— Royal Institution, 3. Prof. Tyndall, "Light as a Mode of Motion: Theories of Light and Colour."

FRIDAY, 23rd.—Royal Institution, 8. Mr. W. H. Fellock, "Dumas Pere," 9.

— Quekett, 8.

— Clinical, 8.

SATURDAY, 24th.—Royal Institution, 3. Mr. James Sully, "Art and Vision."

— Physical, 3.

TO CORRESPONDENTS.

G. D. R.—Bunsen's "Gasometry" is one of the best works you can consult.

THE JOURNAL OF SCIENCE

for April (Price 1s. 6d.), includes—

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THE CHEMICAL NEWS.

VOL. XLI. No. 1065.

A NEW

BLOWPIPE TEST FOR PHOSPHORIC ACID.

By Lieut.-Colonel W. A. ROSS, late R.A.

PHOSPHORUS, whether oxidised or not, is so nearly universally present in the organised world—and probably more than we imagine in the inorganic—that I will not apologise for now introducing this branch of the important subject of its detection and afterwards of its estimation, to the notice of your many readers. I am the more anxious to do this *at once*, as I wish to obtain the opinion of competent chemists regarding the reliability of this test before it appears in my work, "An Alphabetical Manual of Blowpipe Analysis," which Mr. Trübner has now in the press.

(1.) The test is based on the well-known property of pure tungstic acid to afford a brilliant *blue* bead in the "reducing flame" of the blowpipe with *phosphor*-salt, but only a yellow or brownish bead in "R.F." with borax: a fact first proved by Bergmann, in correction of some blowpipe experiments on Tungsten by its illustrious discoverer, Scheele. (*Vide* Scheele's "Chemical Essays," p. 294). In "Pyrology" (page 190) I have applied this test by using an acid medium, viz., boric acid; but I find that the test is far more delicate—in fact quite equal to one of the most delicate of known reactions, sodium manganate—by using instead, soda or potash.

(2.) Tungsten or Wolfram—as all chemists, pyrological or "wet," know—"forms three oxides, WO_2 , WO_3 , and W_2O_5 , neither of which exhibits basic properties." ("Fownes," page 507). As far as blowpipe operations are concerned, the trioxide forms *yellow* or colourless solutions, as in borax in the "oxidising flame": the dioxide is very dark brown or *black*—it appears floating as insoluble black fragments in boric acid; and the curious intermediate oxide, which I assert to be such an excellent detector of phosphoric acid,* gives a *fine blue* colour to microcosmic salt, "R.F."

(3.) Your readers will doubtless see now, that the basis of the reasoning employed by me is that, commencing to operate with the WO_3 , even the "reducing flame" of the blowpipe is unable to reduce the yellow trioxide of tungsten below the blue intermediate oxide, when it is dissolved in such a highly oxidised and oxidising reagent as phosphoric acid; and it is this fact which makes the latter such an excellent detective of tungsten in presence of all the soda of microcosmic salt. I have, therefore, by this test, simply ascertained that even such a reducing agent as potash or soda, is, before the blowpipe, unable to keep contained tungsten below the blue or intermediate condition of oxidation when traces of free phosphoric acid are present, and the alkaline bead is placed in the position I have termed "the peroxidising pyrocone," or P.P.

(4.) By this delicate test it seems possible to prove (what I have for years suspected) that it is the presence of phosphoric acid in extremely minute proportion which causes hydrated silicic acid to opalesce (just as a trace of it causes the analogous boric acid to opalesce), and to form the gem called the *Opal*.

(5.) A bead of pure potassium carbonate is first fused upon platinum wire, and as much powdered "rock-crystal" or pure silica treated on it before the blowpipe as will form a *clear* bead of potash glass. A small fragment or bead of potassium pyrotungstate (made by fusing pure tungstic

acid with pure potash on aluminium plate until all effervescence has ceased) is now taken up on the glass bead and treated in P.P. If the silica was pure, and all carbonic acid has been eliminated from the potash, there will be no decided colour, but the combined bead will remain a dull greenish grey colour.

(6.) Instead of taking up "rock-crystal," powdered *opal* is now combined with the potash bead and treated as in (5), when a *brilliant greenish blue* (turquoise) colour results, which can be made more blue by adding potash.

(7.) In the case of phosphates, such as *coprolite* or *kunkur*, the powdered assay is first dissolved in a bead of boric acid by cautiously adding traces of potash, which forms sufficient potassium borate to dissolve the lime, &c., leaving the phosphoric acid *free*, when I cannot but believe that some analogous combination to the well-known $12WO_3K_8Si_4O_6$ is formed, which is *blue*.

(8.) An authority on the blowpipe has written to me that he believed the blue colour to be due to a trace of manganese contained as an impurity in the tungstic acid from which my potassium tungstate was made, but he is, I believe, now convinced that he was wrong in that supposition.

EDIBLE EARTH FROM JAPAN.

By E. G. LOVE, Ph.D.,
Gas Examiner for the City of New York.

AMONG the many questions which present themselves in studying the customs of semi-civilised peoples, none is more interesting than that of the prevalent and deeply-rooted custom of earth eating: nor is this custom easily accounted for. Alex. von Humboldt attributed it to the "desire to fill the stomach, and thus in a measure to allay the pangs of hunger." Prof. Fuchs thinks a more satisfactory explanation is to be found in the fact that the eating of the unctuous mass obtained by moistening these earths with water produces sensations similar to those felt in eating any fatty substance. A very natural theory would be that it was eaten for its nutritious effect, but analyses show hardly more than 1 per cent of anything approaching nutritious matter. It is also noticeable that in many countries where this custom exists there is no scarcity of other and nutritious food. Sometimes it is eaten because ignorantly supposed to contain certain beneficial properties. However completely these theories may explain this strange custom, there seems to be sufficient ground for the belief that among the more uncivilised, in times of great want, the people were compelled to eat earth to partially allay hunger, that in this way a disease was formed from which it was difficult to free themselves when food again became abundant. Among certain negro tribes there can be no doubt as to the existence of such a disease—a disease of the nutritive functions. Other more civilised peoples may have commenced eating earth for some superstitious reasons or for some pleasurable sensations produced, and ultimately the disease became established. In all cases, however, the existence of the disease seems necessary to explain the custom.

That the Japanese people, in part even, should be addicted to earth eating will surprise many who know the resources at the command of this people, and the little necessity they have for eating earth. Some time ago I had placed in my hands a specimen of edible earth from Tsietonai, on the north coast of Yesso, and used by the Ainos as food. The meaning of the word Tsietonai (Eat-Earth Valley) has reference to this fact. The Ainos seem to think the earth contains some beneficial substances. It is by no means a necessity with them, for really they are a meat-eating people, having venison in abundance, and nature has also supplied them with much in the way of a vegetable diet.

* It will also detect traces of boric acid under certain conditions, which must provide against the formation of alkaline borates; e.g., the use of pure silica as a medium.

The bed from which this specimen was obtained occurs in a small valley, and is several feet in thickness. It is of a light grey colour and very fine in structure. The following is an analysis:—

Silica	67.19
Alumina	13.61
Oxide of iron	1.11
Oxide of manganese	0.07
Lime	3.89
Magnesia	1.99
Potassa	0.23
Soda	0.75
Sulphuric acid	0.19
Phosphoric acid	trace
Water and volatile matter,	11.02
<hr/>	
	100.05

This analysis shows that the earth is essentially a silicate of alumina with silicic acid, and is similar in composition to those eaten by the Javanese and Laplanders. The "volatile matter," which was only small in amount, consists of the fragments of the leaf of some plant, which either the people intentionally mix with the clay for the aromatic principle it contains, or which was accidentally picked up when the specimen was taken. On treating this leaf with ether and evaporating, I obtained some oily matter of an agreeable odour, but too small in amount to enable me to determine its nature.

The Ainos eat this clay in the form of a soup. Several pounds are boiled with lily roots in a small quantity of water, and afterwards strained, and the soup thus prepared is claimed to be a very palatable dish.

New York, March 8, 1880.

A DIRECT METHOD OF TESTING VITRIOL EXITS FOR NITROGEN COMPOUNDS.*

By GEORGE E. DAVIS.

VITRIOL chamber exits, as you are aware, are usually tested by ascertaining the total acidity of the gases, expressed in grains per cubic foot. In some works it is usual to differentiate these acid gases, and to state the results in such a manner as to show how much of this acidity is due to nitrogen compounds and how much to those of sulphur. In order to make this test, the usual plan is take the total acidity of the solution through which a definite volume of the acid gases has been passed, and afterwards to precipitate and weigh the sulphuric acid as the barium salt. By this means the sulphuric acid is estimated directly, the nitrogen acids being determined by difference. You will therefore see that any error in the analytical operation is sure to be thrown upon the nitrogen compounds.

The method I am about to describe is a direct method; may be direct for the nitrogen compounds only, or for the three principal constituents of chamber-exit gases, the sulphur compounds, those of nitrogen, and the hydrochloric acid. The manipulation is simple, it is rapid and accurate, and the total acidity is a check upon the other figures.

I must first of all mention the very convenient continuous aspirator which I have been using, and may be of use to those who have not a good water-supply near to the chamber-exit. It consists of a 10-litre glass bottle with two tubulatures, one at the top and the other at the side near the bottom. Into the bottom tubulature is fitted a cork, into which is inserted a tube for the outflow of water, and regulated by a screw pincock. The top tubulature is closed with a cork having a central perforation, a long tube passes down through the cork to nearly the bottom of the vessel, which by this means is converted into a

Marriott's bottle. When set at any definite speed of outflow the water will continue to run at one even rate until the bottle is nearly empty. I have had an aspirator of this kind working continuously without any attention for periods of thirty-six hours. Good clear water free from any suspended matter must be used with this form of continuous aspirator.

Whether or not this form of aspirator is used the test is made in the same way. A measured quantity of peroxide of hydrogen and the necessary volume of water is placed in the aspiration tubes, and a known volume of the exit-gases drawn through. The solution is then to be made up to a definite volume, and divided into three portions. One is titrated with pure normal sodium hydrate solution, and subsequently acidified, and the sulphuric acid precipitated with barium chloride. The second portion is warmed, treated with solution of potassium permanganate to decompose the peroxide of hydrogen, neutralised with carbonate of soda solution, and titrated for hydrochloric acid with decinormal silver nitrate in the usual way. The third portion is treated with a little silver sulphate, neutralised, filtered, and evaporated on the water-bath to about 1 c.c. When cooled a drop or two of sulphuric acid is to be added to decompose carbonates, and the solution is then transferred to Crum's nitrogen tube. Twice its volume of pure and concentrated sulphuric acid is now added, and the whole shaken up with the mercury; the evolved nitric oxide is allowed to cool, and measured.

10 c.c. of a 10 volume solution of peroxide of hydrogen will oxidise about 7 grains of sulphurous acid. The process may, perhaps, be better understood by an example:—Mr. Vasey has been good enough to aspirate for me 0.625 of a cubic foot of the gases from a vitriol exit by means of the aspirator I have just mentioned. The sample was taken between 12.30 p.m., Sept. 19, 1878, and 11 a.m., Sept. 20, 1878, or a continuous run of twenty-two and a half hours. When the sample was handed to me I made it up to 625 c.c., so that each 100 c.c. should be equivalent to one-tenth of a cubic foot of the exit-gases. 500 c.c. required 0.9 c.c. of normal soda solution, and gave 1.2 grains of sulphate of barium. 100 c.c. when treated in the nitrogen tube gave 0.8 c.c. of nitric oxide. The hydrochloric acid was found to be 0.04 grain per cubic foot. We have, then,—

Grains per cubic foot.

Carbonate of soda neutralised by S compounds	1.092
" " " " N "	0.282
" " " " HCl "	0.059
	<hr/>
	1.433

Carbonate of soda actually neutralised 1.468

If the nitrogen compounds and the total acidity be all that is required the operations are very simple. The solution of the gases must be titrated with normal pure caustic soda, and then evaporated as above upon the water-bath to 1 or 2 c.c. The subsequent treatment is the same as before described.

The method of using peroxide of hydrogen only in the aspiration-tubes you will find much better than the plan I at first adopted of putting the peroxide to a known volume of caustic soda solution. In the latter case a much larger quantity of sodium sulphate is produced, which does not conduce to the easy working of the process.

Several members have raised objections to this process owing to the alleged instability of peroxide of hydrogen. It has been stated that the solution might be decomposed by simply drawing air through it. This might be the case in an alkaline solution, as simple shaking will liberate most of the oxygen; but in an acid solution peroxide of hydrogen is very stable, as may be inferred when I tell you that the sample Mr. Vasey collected still contained some undecomposed peroxide, though alkaline and a current of air had been bubbling through it for over twenty-

* Read before the Faraday Club, February 19, 1879.

two hours. If anyone will work the process with intelligence I am sure he will be satisfied with it.

In the discussion which ensued several members stated that they had tried an alkaline solution of peroxide of hydrogen with a continuous aspirator, and found that oxygen was liberated. Mr. Vasey gave as his opinion that the aspirator mentioned above was too small. He had to fill the bottle three times to get 0.625 foot, and the current of water was so slow that unless the water used was very clean and free from suspended matters the narrow water passage was easily stopped up.

THE VOLATILISING-POINT OF METALLIC ARSENIC.

By EWING G. M. CONECHY.

THE statement of the temperature of volatilisation of metallic arsenic being given by different authors at temperatures varying from 180° C. to a dull red heat, and the true value being required for some work on which Mr. Hannay is engaged, I have determined it by using the melting-point of solids placed near the arsenic to indicate the temperature at which it gave a distinct sublimate. Dr. Carnelly's determinations of melting-points were used, and to prevent oxidation of the arsenic the experiments were conducted in an atmosphere of coal-gas.

I placed in a glass tube small quantities of metallic arsenic, argentic chloride, argentic phosphate, and zinc iodide, and heated gradually. When the arsenic had yielded a distinct sublimate, I removed the source of heat, and found that the zinc iodide had completely melted, while the argentic chloride had agglomerated and was on the point of melting. I therefore concluded that the arsenic did not volatilise below 446° C., which is the melting-point of zinc iodide, and not above 457° C., which is the melting-point of argentic chloride; so 449° to 450° C. is the temperature at which arsenic volatilises. These experiments were repeated several times with same results.

The statements I refer to are—

- "Watts's Dictionary," 180° C.
- "Miller's Chemistry," 180° C.
- "Graham's Chemistry," 180° C.
- "Gmelin's Chemistry," dull red heat.
- (Mitchell), dull red heat.

Private Laboratory,
Sword Street, Glasgow.

ON PORTLAND CEMENT.*

By Mr. WATSON.

PRESUMING that the members of this Society are not so conversant with the history of the manufacture of Portland cement as with that of soda and its compounds, I will venture to preface this short paper with a biographical sketch of the former, commencing at its birth.

In the year 1824, Mr. Joseph Aspdin, of Leeds, in the county of York, master bricklayer, made application at the Patent Office to protect a process for making cement or artificial stone for stuccoing buildings, water-works, cisterns, or any other purpose to which it may be applicable, and which he called Portland cement. The process of manufacture described in his specification is as follows:—

"I take a specific quantity of limestone, such as that generally used for making or repairing roads, and I take it from the roads after it is reduced to a puddle or powder, but if I cannot procure a sufficient quantity of the above from the roads, I obtain the limestone itself and cause the puddle or powder, or the limestone, as the case may be, to be calcined.

"I then take a specific quantity of argillaceous earth or clay, and mix them with water to a state approaching impalpability, either by manual labour or machinery. After this proceeding, I put the above mixture into a slip pan for evaporation, either by the heat of the sun, or by submitting it to the action of fire or steam conveyed in flues or pipes under or near the pan, till the water is entirely evaporated. Then I break the said mixture into suitable lumps, and calcine them in a furnace similar to a lime kiln, till the carbonic acid is entirely expelled. The mixture so calcined is to be ground, beat, or rolled, to a fine powder and is then in a fit state for making cement of artificial stone."

Although Monsieur Vicat, the eminent French chemist, as well as others had succeeded in producing hydraulic cement, closely akin to Aspdin's Portland, long before the date of his specification, and that also by the admixture and burning together of various calcareous and argillaceous substances, there is little doubt that the manufacture of the material known as Portland cement dates from that epoch when Mr. Aspdin filed his specification. How he discovered that the scrapings of the Yorkshire roads could be turned to such good account, there is not evidence to show, but we have proof sufficient that these scrapings when subjected to the manipulation of the observant Yorkshireman, became the pioneer of an important branch of manufacture.

Almost at the same time that Aspdin was experimenting and accomplishing great things with his road mud, Sir C. W. Pasley, of the Royal Engineers, who was stationed at Chatham, was exercising his mind and making elaborate experiments with a view of improving his knowledge of mortars for building purposes, and after sundry trials and failures, he succeeded in making cement identical with Aspdin's Portland, but produced from the chalk and clay of his locality.

Thus, it is curious to observe, Portland cement had its birthplace, although Aspdin had the honour of christening it, in two different places almost simultaneously, and while one inventor secured his raw materials from the hard calcareous and argillaceous formations of the Oolitic system, the other supplied himself from the soft cretaceous chalk strata and the slimy fluviatile accumulations of the post-tertiary period.

Portland cement, like most other young and tender things, was subjected in its early youth to the fierce winds of criticism, and the severe and biting blasts of prejudice; but, as is usually the case when a good thing is offered to the British public, it quickly gained reputation and waxed strong in favour, year by year increasing in demand up to the present, and I think I am under-estimating when, taking last year's statistics as a guide, I calculate the present production in England to exceed *half-a-million* tons per annum. The contagion for making Portland cement has likewise spread beyond the shores of the British Isles, a considerable quantity being now made at various places on the Continent where suitable materials are procurable.

Until very recently the process adopted and the machinery employed in the manufacture of Portland cement have varied very little indeed; the *modus operandi* may be described in a few words. The chalk and clay are thoroughly mixed in a pug or wash mill with a large proportion of water amounting to three or four times the weight of the materials. After being well and carefully incorporated, this liquid slurry is pumped or conveyed by other mechanical means to becks or settling ponds, where the heavier portion of the slurry subsides, leaving the superincumbent water necessary for the mixing on the top; this settlement takes many weeks to complete, the water is gradually drawn off, leaving a substance technically called "slip" at the bottom of the beck. This slip is then conveyed by barrows and spread on flats, where it is dried, the requisite heat being derived from coke ovens built beneath the flats. When the moisture is all evaporated, the dried material is removed and loaded into kilns adjoining, fuel being added

* A paper read before the Newcastle-upon-Tyne Chemical Society, February 26, 1880.

sufficient for the requisite amount of calcination, the waste heat and gases being liberated through the chimney or sugar loaf cone of the kiln, which few will have failed to observe who have ever been in the vicinity of a Portland cement manufactory. After the important process of calcination is completed, the clinker is drawn from the kiln, conveyed to the mill, then ground by horizontal millstones and is ready for the consumer.

To those who have been able to follow this rough description it will be apparent that the aim of the cement maker is, in the first place, to secure a thorough and uniform mechanical incorporation of the raw materials he has to deal with; secondly, to preserve or sustain that intimate relationship during the drying process; and, thirdly, to induce perfect and equal chemical combination of the component parts by the application of heat.

It is almost needless to remark that, of course, the primary step is the selection of raw materials suitable for manipulation, and then to decide upon proportions necessary for a successful result, which can only be arrived at by a careful chemical analysis. The object of the cement maker is to produce a mechanical mixture of carbonate of lime and silicate of alumina, the proportions of which are so evenly balanced that only sufficient lime is present to enter into chemical combinations with the silica and alumina in the clay, and produce a double silicate of lime and alumina.

Although the method of manipulation described is still extensively adopted, yet, like all other devices of man, it is crowded with imperfections, and among the multitude of these shortcomings a few may be mentioned. The chalk usually employed does vary either in chemical proportion or specific gravity to any extent, but the alluvial deposits are not so uniform in their character. A formation of clay has been found to vary continually, more particularly as regards its specific weight, or rather the weight of the particles composing the formation. Take as an instance the uncombined silica, which is present more or less in every alluvial deposit. The high specific gravity of these uncombined particles as compared with the other substances present, naturally causes them to separate from the remaining portion of the conglomeratic mass, when the agitation of the mixing process ceases, unless the solidity of the mixture is sufficiently dense to hold those silicious molecules in suspension. A percentage of uncombined silica is not an objection in the process of cement making, provided the proportion is not in excess, the particles are not coarse, and the quantity is equally diffused throughout the mixture. These varying properties in weight lead to serious results in the settling beds, unequal subsidence occurring, thereby rendering a perfect chemical combination in the process of calcination impossible.

Another difficulty presents itself in the serious wear and tear and continual dilapidation of the drying flats, in consequence of the unequal and sudden expansion and contraction of the floor, unavoidable when subjected to the influence of the heat given off from the coke-ovens beneath, and the frigid temperature of the semi-liquid slurry when deposited on the floor above.

The liberation of the waste heat given off from the kilns, without an attempt at utilisation, is also unquestionably an imperfection.

I have drawn your attention to the shortcomings of the cement maker, with the hope that you will appreciate a hasty description of some improvements which have recently been introduced in the manufacture of Portland cement.

Johnson's patent chamber-kiln most effectually annihilates two of the objections just referred to, inasmuch as it takes the place of the extravagant flats and coke-ovens, and serves to utilise the waste heat given off from the kiln. Mr. Johnson's invention consists of a kiln of the ordinary construction; but, instead of being surmounted with the usual cone, through the top of which the waste heat is liberated, it is arched over, forming a crown of

semi-circular shape. One side of this arch is continued with a flat bottom, in a horizontal direction, forming a semi-circular flue of chamber, supported on piers or ground work. The waste heat from the kiln traverses the length of this flue, and eventually escapes through a chimney at the further end. When the kiln is charged, the liquid slurry from the wash mill is pumped or conveyed through pipes to the top of the flue or chamber; these pipes are furnished with outlets through which the slurry flows, dropping through manholes on the roof of the chamber, and finally depositing itself on the floor of the same, which is composed of ordinary concrete.

Sufficient material having entered the chamber requisite for the re-charging of the kiln, the manholes and other apertures are closed, the kiln is fired, and the heat evolved, passing over the slurry in the flue or chamber, thoroughly dries the material ready for the re-loading of the kiln. Thus the heat given off from one charge is made to dry the slurry requisite for the re-loading. The chamber is constructed of sufficient height to enable a workman to move about in it conveniently.

When describing the process of mixing the crude materials in the wash mill, I mentioned that a large proportion of water is necessary to secure thorough amalgamation. As this large amount of water requires to be drawn off again through the agency of the objectionable subsiding ponds, or by evaporation where the patent chamber kiln is employed, it is obvious that any system which will ensure satisfactory results, and at the same time will allow the manipulator to dispense with a large proportion of this water, is very valuable. Goreham's patent system possesses this advantage. Mr. Goreham subjects the materials to a process of wet grinding, which takes the place of washing. The chalk and clay are thrown together into a mill exactly similar to the old-fashioned arrangement, but instead of being reduced to a liquid with the usual large proportion of water, they are brought, by the addition of only one-third their weight of water, to a consistency just thin enough to flow out of the mill. But as this mixture contains a large proportion of coarse particles of the aggregates, which require further reduction, the mixture is passed through horizontal millstones.

By this process the particles are speedily reduced to an impalpable paste of sufficient solidity to render the intermediate process of settlement unnecessary, and the evaporation in the patent chamber an easy task.

Several cement manufactories have recently adopted the combined processes of Johnson's chamber kilns and Goreham's wet grinding, with results far exceeding their most sanguine expectations, and can it be wondered at? When new appliances are introduced in the process of any manufacture, the adoption of which results in the economy of fuel and labour, as well as greatly diminished space requisite for manipulation, the advantage gained by the manufacturer must be considerable; but, if these economical changes can at the same time ensure *higher perfection* in the *quality* of the finished manufacture, the value of those appliances cannot be over estimated.

In conclusion, I would remark that since these improvements have been introduced, rendering the difficulties previously referred to things of the past, and imparting to the manufacturer almost absolute control over the varied physical changes of the crust of the earth, I am led to believe from experiments, and the practical experience of others as well as myself, that the day is not far distant, when, aided by the chemist, the geologist, and the engineer, Portland cement makers may seek a wider field for their raw materials than the oceanic deposits of the chalk system and the alluvial clays of the tertiary period.

Decomposition of Cinnabar by Hydrochloric Acid.
—The sulphide of mercury whether black and amorphous or red and crystalline is attacked by chlorine, according to the temperature and concentration of the acid.—*Zeitschrift d. Allgem. Oester. Apoth. Vereins.*

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 15, 1880.

Prof. H. E. Roscoe, President, in the Chair.

THE following certificates were read for the first time:—E. Rawson, Kamze Lal Dey Rai Bahadour, Tarapwsanna Roy, of Calcutta, and R. G. Watts. The following gentlemen were balloted for and declared duly elected Fellows of the Society, Prof. McLeod and Dr. Wright being appointed scrutators:—M. Bechler, A. W. Black, E. C. Copas, W. J. Dibdin, A. C. Fryer, E. C. Graham, C. H. Gimmingham, R. Grimwood, W. F. Haydon, H. Liepmann, F. B. Last, W. Robinson, W. Regester, J. W. Stanley, H. H. Slater, A. J. Smith, H. C. Stephens, C. P. Scheibner, J. R. Skelton.

The following papers were read:—

"On the Lecture Illustration of Chemical Curves," by E. J. MILLS. The author has contrived for lecture illustration an apparatus extremely concrete in its effect, in which a chemical action or process constructs its curve under the eyes of the observer. It consists of eighteen tall glass cylinders, of about 270 c.c. capacity, which are filled with water and inverted in as many glass troughs, the cylinders being supported at equal distances from each other in a sort of test-tube frame. Each inverted cylinder has a 100 c.c. stoppered retort, so arranged that its beak can be made to dip under the mouth of the cylinder, the whole forming a series of gas generators and pneumatic troughs, arranged at equal distances from each other. With this apparatus the author has examined the action of dilute sulphuric acid on zinc, and sodic hydrate on aluminium, the two factors, which were varied, being the strength of the solution and the time during which the action was allowed to take place. Thus, equal pieces of clean zinc were placed in each of the retorts, the strength of the acid rising in successive retorts by 3 per cent. Thus various amounts of hydrogen were evolved and collected in the inverted cylinders. The surfaces of the water in the eighteen cylinders were then found to assume a curved line representing the course of the action. This the author designates a quantity curve. Similarly, by increasing the time during which the action was allowed to proceed by equal increments, another curve was obtained—the time curve. The errors in the method are pointed out, but a sufficiently accurate general impression will be conveyed of the true curve, as the eye neglects the small variations.

After a few remarks by Mr. W. THORP, who had seen the experiments in Dr. Mills's laboratory, and who confirmed the care taken to ensure identity of conditions as to the zinc, temperature, &c.,

The PRESIDENT called on Mr. W. H. PERKIN to read his paper "On the Analysis of Organic Bodies containing Nitrogen" (continued). In the previous portion of the paper the author proposed to absorb the oxides of nitrogen formed during the combustion of nitrogenous substances by potassic chromate instead of decomposing them by metallic copper. In the combustion of uric acid oxide of copper was used: as some of this was reduced, it might be argued that the good results were partly due to the metallic copper and not to the chromate. Therefore, to test the process still further, combustions were made with plumbic chromate instead of oxide of copper, and it was found that the potassic chromate did not satisfactorily absorb the nitrous fumes. A quantity of precipitated manganic oxide was now made into a paste with a saturated solution of potassic chromate, and the mixture dried and heated somewhat strongly to drive off most of the water of hydration. The efficacy of this mixture was tested by its power of absorbing the red fumes evolved

when plumbic nitrate is heated. When the mixture was heated to a dull red heat, some of the nitrogen oxides passed, but if kept at a lower temperature the absorption was complete. The mixtures used consisted in one case of 1 part of chromate to 2 of manganic oxide; in others, of 2 parts of chromate to 1 of manganic oxide: both mixtures seemed to answer equally well. Combustions with dinitro-benzene and with ethyl-ortho-toluydin gave very satisfactory results. The author recommends potassic chromate containing 10 per cent of potassic dichromate, as the commercial precipitated manganic oxide may contain potash; powdered pyrolusite will not act satisfactorily. In making combustions, about 6 inches of the mixture are placed in the front part of the tube (the oxygen process being used). The whole of the tube is heated and dried with air in the usual way. The temperature of the mixture is then allowed to fall to about 200° to 250° C. (in the combustion furnace used by the author, about one-fourth of the burners were kept alight and were half turned down). The combustion is proceeded with in the ordinary way. By re-heating the mixture strongly and passing a current of air, the oxides of nitrogen are removed and the mixture is ready for a second combustion. Thus the same mixture has been used seven times. If the organic substance contains sulphur, a greater length of the mixture must be used, the back part being strongly heated to absorb the sulphur dioxide, and the front part maintained at 200° to 250° to absorb the oxides of nitrogen.

The PRESIDENT said that all present could appreciate the value of this convenient method. The author had hit on a blot in our methods of organic analysis, and apparently completely removed it.

Dr. WRIGHT commented on the great boon the author had conferred on analysts by the introduction of this method.

The PRESIDENT then called on Prof. McLEOD to communicate some results obtained "On the Volatilisation of Solids in Vacuo," by W. DOUGLAS HERMAN. Prof. McLeod, requiring some pure phosphorus, endeavoured to obtain some by distilling ordinary phosphorus in an atmosphere of carbon dioxide, but without success. He then wrote to Mr. Herman, who had been investigating the question, enquiring where his paper on the subject was published. In reply, Mr. Herman forwarded the present communication with a request that he would read it. The paper, however, was incomplete; Prof. McLeod therefore gave an abstract of the paper, with a short account of some experiments he had himself made, and exhibited some illustrative specimens. Mr. Herman sealed up some phosphorus in glass tubes containing air, and in vacuum tubes. On distillation in the light the phosphorus in the vacuum tubes condensed in minute yellow crystals. When, however, the distillation was conducted in the dark, most beautiful colourless transparent adamantine crystals were obtained. The phosphorus was dried with filter paper. The author states that when these crystals were exposed to daylight they became rough and opaque, and at the same time yellow or red. As regards the opacity, Prof. McLeod has obtained somewhat different results. Mr. Herman also observed that when phosphorus was heated in sealed tubes to 140° it became suffused, and remained in the liquid state for some months after cooling. This phosphorus turned red in the sunlight. It was interesting to note that the red amorphous phosphorus could therefore be obtained from solid, liquid, and gaseous phosphorus by the action of light alone. In one case a crystal of phosphorus was obtained 8 m.m. long. Mr. Herman also made some most interesting experiments with sulphur. Sulphur sealed up in vacuum tubes distilled over at the temperature of boiling water, i.e., 11° below its melting-point, and condensed in minute drops of liquid sulphur, forming a cloud on the glass. After a time minute crystals appeared and began to grow, the liquid disappearing *pari passu*, until the whole was converted into crystals. Similar ex-

periments were made with iodine, selenium, ammonium chloride, &c. Prof. McLeod then gave an account of the experiments he had made. He had sealed up phosphorus in glass tubes, carefully absorbing all moisture, by melting the phosphorus in a bulb connected with a second bulb containing phosphoric anhydride. He had obtained most beautiful crystals, some of which were exhibited. They resembled in appearance diamonds, being perfectly colourless, with an adamantine lustre, perfectly transparent, and from their high refractive index sparkling with prismatic colours. Prof. McLeod had not been able to confirm Mr. Herman's statement that these crystals became opaque in daylight, and thought that this was due to the presence of a trace of water containing a trace of air; as from experiments he had made, by distilling some phosphorus as above described and then exposing it to the action of boiled water and light, the crystals became red, but remained transparent. If, however, unboiled water was used a white coating was formed similar to that on ordinary phosphorus kept under water and exposed to light.

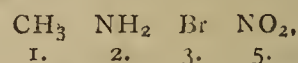
The PRESIDENT had listened with great interest to the lucid explanation given by Prof. McLeod; as to the specimens they would speak for themselves. He hoped Mr. Herman would finish and publish his paper, and especially complete the interesting observations about the volatilisation of sulphur.

Mr. WARINGTON then communicated a paper "*On the Determination of Nitric Acid as Nitric Oxide by means of its Reaction with Ferrous Chloride.*" The author has previously communicated some results on the determination of nitric acid by Crum and Frankland's process: with small quantities of nitric acid; in the presence of sugar and other organic matter, the results were always low. Schlessing's method was therefore examined, which consists in boiling the solution of the nitrate in a flask until all air has been expelled, and then boiling with strong solution of ferrous chloride and hydrochloric acid for ten minutes. The nitric oxide is collected over mercury, oxidised into nitric acid, and titrated. An atmosphere of carbonic anhydride was then used to displace the air: in this way results were obtained equal to 95 per cent of the nitrogen taken. When cane-sugar, gelatin, &c., were added the results came too low; but by increasing the quantity of ferrous chloride good results were obtained. Another modification largely used consisted in collecting the gas over caustic soda. As the quantities of nitric acid to be determined by the author were extremely small, being equal to 5 to $\frac{1}{2}$ m.grm. of nitrogen, it seemed to him best to estimate the nitric oxide given off by gas analysis. A chloride of calcium bath was also introduced for heating the nitrate, the best temperature being 130° to 140° C. It was found that however long the mixture was boiled, bubbles of gas were being constantly evolved, due to the introduction of air with the hydrochloric acid. However, with small quantities (5 and 1.4 m.grm.) of N, gas = 99 per cent of the N was obtained. On analysing this gas it was found to contain only 95 to 87 per cent of N. It was noticed when sugar was present that much brown fluid distilled over, and on examination it was found that a marked diminution of the gas took place by allowing it to stand over caustic soda, and especially with caustic soda and the brownish distillate, as much as 42 per cent of nitric oxide being thus absorbed with the caustic soda alone; so the author cannot help thinking that many analyses in which the gas has been collected over caustic soda are in rather a perilous condition. If the gas is analysed at once, before much absorption has taken place, the error is not large. The author then described the apparatus which he found to be most convenient. It consists of a double-necked globular receiver. From one end passes the delivery-tube; through the other, a tube for a stream of carbonic anhydride, and a somewhat long vertical tube furnished with a stoppered funnel at the top for introducing the hydrochloric acid. The air is first cleared out by passing carbonic anhydride for thirty minutes the

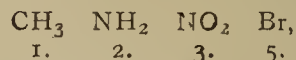
acid is then introduced, and the solution evaporated to dryness by the chloride of calcium bath at 138° to 140° ; the nitric oxide swept out by carbonic anhydride and analysed. In this way, with 1.14 m.grms. of N, results = 92.9 per cent were obtained. Sugar and organic matter have no effect on the results. The author mentioned, in conclusion, that a strong solution of ferrous chloride was extremely useful for absorbing nitric oxide.

Dr. ARMSTRONG suggested the use of the Sprengel pump instead of a stream of carbonic anhydride to get rid of the air.

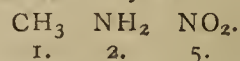
The next paper was taken as read. "*On the Six Possible Isomeric Dibrom-toluols, and other of the Bromo- and Bromo-nitro-derivatives of Toluol related thereto,*" by R. H. C. NEVILLE and A. WINTHER. In the table given by Körner in his memoir on "The Isomerism of the Aromatic Compounds containing Six Atoms of Carbon," the rule is laid down that on introducing one of the halogens or the nitro group into aniline it takes the para position. Körner's paper deals only with the substitution of the second hydrogen atom in mono derivatives of benzol, and consequently in this case only with aniline. But it appears to the authors that the rule is of more general application, and that if bromine, or the other halogens, or the nitro group, be caused directly to act upon aniline or its homologues, toluydin, xylydin, &c., or probably other amido compounds, the acid group nearly always, if possible, takes the para position to the amido group. If the para position be occupied by any other group, it then takes one of the ortho positions, but under no circumstances can one of the above acid groups be directly introduced into a position meta to the amido group. The authors think that the above rule will also hold good with diamido compounds. Some experiments of Wrobleosky (*Fahrs.*, 1870, 528, and 1871, 450) appear to point to conclusions at variance with the above theory. The authors have therefore repeated his experiments, and established the correctness of the above theory against the experiments of Wrobleosky. They obtained a brom-nitro-toluydin (m.p. 180.3°), having the constitution—



the constitution of Wrobleosky's substance (m.p. 139°) being—



and ordinary nitro-ortho-toluydin—



The authors next consider the dibromo-toluol in which both the bromine atoms are meta to the CH_3 group, and promise a further communication on the dibromo-toluol 1, 2, 6, and the tribromo- and tetrabromo-toluols. The paper is lengthy, and contains detailed accounts of the preparation and analysis of the numerous compounds investigated.

The Society then adjourned to May 6, when the following papers will be read:—"On the Action of Sodium on Ethereal Salts of Phenyl-acetic Acid," by Dr. Hodgkinson; "Estimation of Nitrogen in Carbon Compounds," by C. E. Groves; "On Sage Oil," by M. M. P. Muir. It was also announced that the discussion on Dr. Tidy's paper, "On River Water," will take place on May 20. (The paper will be printed early in May.)

A Case of Permanent Polarity in Steel opposed to that of the Magnetising Coil by which it is Produced.—A. Righi.—The author finds that on taking bars of the same steel and of the same diameter but of decreasing lengths, we arrive at a certain length which no longer yields any magnetisation, whilst with smaller lengths we obtain a permanent polarity opposed to that of the coil.—*Comptes Rendus.*

NOTICES OF BOOKS.

Saint Germain-en-Laye. The Sewage of Paris. Operations of the Committee of Defence against the Projects of Stripping and Irrigating the Forest of Germain. (Eaux d'Egout de Paris. Travaux du Comité de Défense contre les Projets de Déboisement et d'Irrigation de la Forêt de Saint Germain-en-Laye) Saint Germain : Bardin. Paris : Dunod.

(SECOND NOTICE.)

It is an indisputable fact that if we fill a flower-pot with any garden-mould, or any fair average arable soil, and pour upon it a glass of town-sewage, the water will trickle out below deprived of its offensive smell, of its colour, and freed to a great extent of its objectionable constituents. It is also a fact that sewage applied to land in moderate quantities and in time of drought, or when the state of the crops renders a supply of moisture needful, produces most satisfactory results. But these facts by no means warrant the conclusion of sewage-irrigationists that the liquid refuse of a town can thus be got rid of the whole year round. The supply is constant, the demand intermittent. The town always wants to get rid of its sewage, and the land is not always in a state to receive it with advantage. The fields of France, like those of England, are often moistened enough, and more than enough, by the rain and by the rise of rivers. To admit more liquid over them at such a time must simply be to convert the soil into semi-fluid mud. Consequently at such times the farmers and gardeners will not accept the sewage. Nor can the difficulty be overcome by enlarging the irrigated area. Excessive rains are not commonly confined to small patches of land, and if one plot of, say, a thousand acres, is water-logged, as a rule the adjacent plots on every side will be in the same condition.

At Gennevilliers, one particular plot of the irrigated land is known as the "town garden," and is kept in apparently good condition by a most lavish expenditure of labour. Whether the produce obtained covers the cost of production is, to say the least, very doubtful. Whether a larger area, such as that proposed to be formed at St. Germain, would or could be as skilfully and carefully managed is also much to be questioned. Everyone knows that the market-gardener above all things seeks to bring his produce to a saleable condition as early as possible. But this end cannot be accomplished by irrigation, which, as every physicist knows, must tend to lower the temperature of the soil. Hence the vegetables of Gennevilliers never arrive early in the market. The irrigation grounds only give a luxuriant yield in the height of summer, when vegetables are produced plentifully everywhere, and have consequently become a drug in the market. This objection to irrigation falls away in hot climates, a circumstance overlooked by certain enthusiasts, who fail to see that what confers fertility in India may prove rather injurious in England.

The crops cultivated at Gennevilliers are few. Cabbages and artichokes are very abundant, and we find also peppermint, wormwood, a few strawberries, and some fruit trees. Grain is exceptional, and when planted it is rarely and sparingly irrigated, and is cut green for the use of cattle. Cabbages are perhaps the leading feature in the irrigated portion of the peninsula of Gennevilliers, and their cultivation requires a great amount of labour. The ground must first be laid out in ridges and furrows, so levelled as to permit the proper flow of the sewage. When the cabbages are mature the soil must not merely be turned over, but those parts where the plants have grown must be mixed with those which formed the furrows, and then the whole plot must be laid out anew. Nor must it be supposed that the sewage-fed cabbages, though often large, are crisp and firm, like those grown in a rich but unirrigated field. It is very easy to thrust the finger up to the knuckle into the heart of one of the Gennevilliers cabbages.

There are some meadows along the road from Gennevilliers to Asnières, but these are only irrigated for a short time after the grass is cut, and the sewage is then turned off while it grows. The grass is not adapted for conversion into hay, but is fed immediately when cut. With all possible care in clearing out the furrows the surface of these meadows becomes matted, and it is found necessary to turn the soil over and lay them down to grass again. The grass, moreover, is found to have a purgative action, which unfits it for the regular food of cattle.

The Defence Committee are far from pronouncing sewage irrigation as injurious under all circumstances. They consider that a farmer who should have a supply of sewage at his command, and who could use it at certain seasons of the year or in certain states of the weather, would find it very advantageous, but it is obvious that such irrigation could play no appreciable part in the regular disposal of the sewage of a great city.

It must be admitted that for an experiment on the disposal of sewage on this principle Gennevilliers enjoys an exceptionally favourable position. The soil is deep and uniformly porous, neither apt to retain the water in pools on its surface nor to let it pass through cracks. All garden-produce finds a ready market in Paris without the expense of a long carriage. Yet with all these advantages the success of the scheme is as dubious in a financial respect as it is from the point of view of public health and convenience.

At St. Germain-en-Laye all conditions are less favourable. The surface of the soil is of a more retentive nature, e.g., near Nanterre and Bezons, where pools, formed by the inundations of the Seine, have been known to remain for four months until dried up by evaporation. What a district for irrigation! The distance from Paris is also considerable, and farm- and garden-produce can consequently only find a market at a very tangible outlay for carriage. In fine, the project may be described as an attempt to convert a pleasant district into a focus of malaria without the excuse of profit.

The results at Gennevilliers are simply what we have always expected,—another case of the unsatisfactory working of irrigation in a high latitude and a moist climate.

On the Constitution of the Naphthalins and of their Derivatives. By F. REVERDIN and E. NOELTING. Geneva: H. Georg.

THIS treatise comprises evidence in support of the formula for naphthalin as proposed by Erlenmeyer and Graebe; an account of the isomerism of the derivatives of naphthalin and of their constitution. An appendix gives the bibliography of the subject. The well-known reputation of the authors will be a sufficient guarantee for the accuracy and thoroughness of this monograph, and render it valuable as a work of reference to all engaged in the study of this interesting class of compounds.

The Scale-Photometer: a New Instrument for the Mechanical Measurement of Light, along with Contributions to the History and Theory of Mechanical Photometry. (Das Skalen Photometer: ein Neues Instrument zur Mechanischen Messung des Lichtes.) By FRIEDRICH ZÖLLNER, Professor of Physical Astronomy at the University of Leipzig. With four illustrations and an Appendix to Vol. III. of the "Scientific Treatises" on the "History of the Fourth Dimension," and on the Hypnotic Experiments of Professor Weinheld. Leipzig: Staackmann.

THE author was, so far back as 1857, engaged with the construction of a "polarisation-photometer." At that time the physiological and the chemical effects of light afforded the only known means for the determination of its intensity. In optical investigations, however the construction of the instruments might vary, everything ulti-

mately turned upon the power of the human eye to recognise the degrees of brightness of two surfaces, placed in close juxtaposition under favourable circumstances. If photometry was ever to be removed from the region of physiology to that of physical measurement, properties of light must be discovered and utilised which are capable of leading to measurable changes in space, *i.e.*, visible movements. This want, which the author experienced twenty-three years ago, is supplied by the aid of the radiometer. He points out very distinctly that Mr. Crookes, from the very outset of his discoveries, carried on experiments in this direction, and made various proposals for the construction of suitable photometers. If, for instance, the ultra-red rays of the spectrum—*i.e.*, the invisible heat-rays—are cut off by means of a concentrated aqueous solution of alum, and the ultra-violet, or invisible, chemical rays are at the same time kept off by a solution of quinine sulphate, the radiometer can then be affected solely by those rays which otherwise act upon our sight, and which therefore are called light in the strictest sense of the word. Under these circumstances the radiometer becomes a true photometer. Professor Zöllner claims, therefore, not the principle, but simply the form and the construction of the scale-photometer. An intelligible description of the instrument cannot, of course, be given without the aid of the illustrations which accompany his treatise.

As might be expected, the two principal theories brought forward to explain the action of the radiometer are here carefully and minutely discussed. Concerning the view which has found most favour in this country Prof. Zöllner thus summarises his results:

"The explanation of the radiometric movements based upon the principles of the kinetic theory of gases makes assumptions concerning the relation of the mean way traversed by the gas-molecules to the dimensions of the receivers which do not exist in reality.

"This explanation, further, without advancing any sufficient reason, ignores the simultaneous existence of the vapours of mercury whose molecules have a mass more than sevenfold greater, and travel a much smaller mean distance than the molecules of the gas which is supposed to be active by the mechanical theory.

"Hence we are not entitled to behold in the radiometric phenomena discovered by Mr. Crookes an empirical confirmation of the kinetic gas theory."

Into the subjects treated in the appendix we cannot enter further than to ask those who use the word "hypnotism" a term invented, we believe, by a Dr. Braid, of Manchester—whether it really throws light upon any phenomena, physical, biological, or psychical, or whether it is not one of those unmeaning expressions which men so often put forward to hide their own ignorance?

Die Explosiven Stoffe, ihre Geschichte, Fabrikation, Eigenschaften, Prüfung, und Praktische Anwendung. (Explosives: their History, Manufacture, Properties, Examination, and Practical Application.) Von Dr. Fr. BÖCKMANN. Vienna, Pest, and Leipzig: A. Hartleben.

THIS treatise forms the sixtieth volume of a useful series of works entitled the "Chemico-technical Library" (*Chemisch-technische Bibliothek*). The author in his introduction points out the great development, or it might almost be said the revolution, which has taken place in the manufacture and in the very nature of explosives. He fully admits, however, that where sustained propulsion is required, as in case of projectiles, ordinary gunpowder still remains not merely unsurpassed, but without a rival. Where a sudden bursting shattering power is required it takes a very low rank in comparison with nitro-cellulose and nitro-glycerin.

Entering into the theory of explosives, he defines them as solids or liquids which can suddenly assume a gaseous condition. In so doing they occasion that sudden, violent, and irregular agitation of the air known as explosion.

Most of them contain an oxygen-bearer, *i.e.*, a solid or liquid body containing oxygen in a state of unstable combination, so that it may be readily transferred to carbonaceous matter present, the final products of the reaction being carbonic acid, watery vapour, and nitrogen. This rule is, of course, not without exceptions, as in case of nitrogen chloride and iodide, which contain neither oxygen nor carbon, and whose explosion is not so much a combustion as a simple falling to pieces.

The author divides the substances of which he treats into two classes—those directly explosive, gunpowder, loose gun-cotton and its analogues, nitro-mannite, nitro-glucose, picric acid, &c.; and those which are only indirectly explosible, such as compressed gun-cotton and nitro-glycerin with its mixtures.

As regards the history of gunpowder, the author considers it as decided that the invention was due neither to Roger Bacon nor to the German monk, Berthold Schwartz, whose very existence is questionable, but must rather be sought in India or China, and at a date anterior to the Christian era. In the eighth century, Marcus Gracchus, in his work "*Liber Ignium*," gives a receipt for an explosive mixture which does not differ widely from modern gunpowder. He takes 1 part of sulphur, 2 of charcoal, and 6 of saltpetre, and he recommends the wood of the willow as the best material for the preparation of the charcoal. The first introduction of gunpowder in mining operations appears to have been after the Thirty Years' War.

The manufacture of gunpowder is described with great thoroughness, from the sources and preparation of the three ingredients downwards. The best charcoal, it appears, is that obtained from the stalks of hemp, but only the hemp of the more southern parts of Europe is sufficiently well developed for this use. The use of superheated steam for the carbonisation of wood was found less advantageous than charring in ordinary cylinders, at least as far as military powder is concerned. The yield is smaller and the first outlay greater.

After completing his exhaustive account of ordinary gunpowder, he proceeds to mixtures of analogous character, but in which, instead of saltpetre, sodium or barium nitrate, or potassium chlorate or perchlorate, are partially or wholly introduced.

The author justly pronounces the white gunpowder of Augendre—a mixture of potassic chlorate and ferrocyanide with sugar—dangerous, as we know from personal observation. Passing over Sprengel's preparations—in which trinitro-benzol and picric acid are employed—and the picrate powders of Borlinetto, Designolle, and Brugère, we come to the most important class of modern explosives, those produced by the action of concentrated nitric acid upon organic matter. The discovery of gun-cotton (nitro-cellulose) by Braconnot, Pelouze, Schönbein, Böttger, and Otto, and the great improvement in its preparation due to Lenk, are described very fully. The process of the latter, as most of our readers are aware, has been still further developed by Prof. Abel, and is carried on in England on a large scale. The account of the manufacture of gun-cotton, as carried on at Stowmarket* and at Waltham Abbey is both accurate and minute. He considers nitro-gelatin—otherwise known as gelatin-dynamite—will prove the most formidable rival of gun-cotton if it can be rendered less sensitive to the effects of shocks or explosions which may be produced near at hand—*e.g.*, by an enemy's fire or by the discharge of a mine or a torpedo.

We come next to nitro-glycerin and its various disguises, such as dynamite. It must be remarked, however, that the author uses this word generically, so as to include not merely the ordinary dynamite—a mixture of nitro-glycerin with infusorial earth or with china-clay—but such explosives as lithofracteur, Brain's powder, lignose, dualin, cellulose dynamite, &c., in which nitro-glycerin is compounded with other explosive bodies. In all these mixtures, as in the original nitro-glycerin and in gun-cotton, the

* We understand that the manufacture of gun-cotton at Stowmarket temporarily at least, suspended.

explosion-point lies considerably higher than the point of ignition.

The second section of the work, at which we can but glance briefly, treats of the practical applications of explosives, and the precautions necessary for their storage and transport, including an abstract of the German laws regulating the carriage of dynamite, &c., by land or water. A summary of the whole art of blasting, and the arrangement of mines and torpedoes, with the various forms given to these destructive appliances, concludes a book which we should have been happy to notice at greater length did space permit.

For manufacturers of explosives, military and mining engineers, this treatise must prove of the highest value. Nowhere else have we seen this important subject treated with such thoroughness and accuracy. The book is plentifully illustrated, and is furnished with abundant bibliographical references.

CORRESPONDENCE.

AMMONIA FROM THE NITROGEN OF THE ATMOSPHERE AND THE HYDROGEN OF WATER.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 155, you refer to Messrs. Rickman and Thompson's patent for the production of ammonia (in the form of the chloride) from atmospheric nitrogen and nascent hydrogen. I have since obtained copy of this patent, filed March 15, and find that it is only the preliminary specification, and there is therefore some difficulty of arriving at facts. However, it appears to me, as far as I am enabled to judge from same and your explanatory remarks, that the *bulk*, at all events, of the ammonia obtained by this operation is that given off during the combustion of the coal-dust during the coking process.

Dr. Angus Smith, in his last report in connection with the Noxious Vapours Act, advocates the abstraction of the ammonia for agricultural purposes from the coal used in iron works, &c., and Messrs. Rickmann and Thompson's patent seems well adapted to be worked advantageously in connection therewith, if the ammonia they obtain is, as I surmise, principally from the coal.—I am, &c.,

J. H.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 12, March 22, 1880.

Silver Teroxide.—M. Berthelot.—The author has re-examined the compound first obtained in 1804 by Ritter from the electrolysis of silver oxide. The substance is unstable, and in a short time the black brilliant metallic crystalline needles crumble to a black amorphous powder, with escape of oxygen. This change is accelerated by heat, and becomes explosive a little below 100°. It is also decomposed by washing, the water gradually carrying off silver nitrate. The analysis of the recently-prepared compound shows the composition— $4\text{AgO}_3, \text{NO}_6\text{Ag}, \text{HO}$. The author regards it as the silver-salt of an argento-nitric acid.

Decomposition of Potassium Permanganate by Oxygenated Water.—M. Berthelot.—These two compounds, if brought in contact in a strongly acid liquid, are reciprocally decomposed, losing all their active oxygen, and being reduced to the state of protoxide. This change is due to the formation of an unstable compound, whose spontaneous destruction explains the consecutive evolution of oxygen. This new colourless compound is stable at -12° in the medium where it is produced, but is destroyed on assuming the ordinary temperature. The author regards this compound as a hydrogen teroxide formed by the oxidation of oxygenated water by potassic permanganate. It is analogous to several peroxides and metallic acids, and to hydrogen tersulphide.

Electric Regulation of Time in Paris.—M. Tresca.—The system employed is composed of a certain number of horar centres distributed over two telegraphic nets and of the clocks of the city kept to the true time, so that the error can never exceed a minute.

Analysis of the Luminous Phenomena Produced by Electric Discharges in Rarified Gases.—E. Fernet.—A large vertical tube, to each of the ends of which are soldered wires serving to pass the discharges of a Ruhmkorff's coil, is placed in permanent connection with a Geissler pump. The tube is placed behind a screen in which is an upright slit. Opposite the slit is fitted a revolving mirror moved by a small turbine, and having its axis of rotation also vertical. The displacement of the image seen in the mirror renders it possible to observe the successive appearances offered by those points of the tube left free by the slit. As soon as the rarefaction is sufficient to permit the passage of the inverse induced current, corresponding to the closing of the circuit, as well as of the direct induced current, corresponding to the breaking of the circuit, two images are seen in the mirror. Three of the images thus obtained are figured.

Thermic Laws of Electric Sparks Produced by the Ordinary, Incomplete, and Partial Discharges of Condensers.—E. Villari.—The author proposes the following laws:—(1.) When in a conductive arc there are formed two sparks, one of which is against the discharger, the sum of the heats produced is constant. (2.) The sum of the lengths of the two sparks (one of which is against the discharger) is constant. (3.) The total electric resistance which the two sparks encounter in the gases where they are formed is constant. (4.) The quantity of electricity which constitutes the discharge of a condenser is constant whatever is the length of one of the sparks which are formed in the discharge itself. (5.) The total heat developed by the different sparks of the discharge of a condenser is inversely as its surface.

Photography of the Solar Spectrum.—E. Conche.—The proofs which the author presents to the Academy give the prolongation of the spectrum into the obscure symmetric region of the blue with reference to the extreme red.

Density of Iodine at High Temperatures.—J. M. Crafts and F. Meier.—M. H. Sainte-Claire Deville has not only been the first to study the densities of bodies at very high temperatures, but he has been able to penetrate to the root of the question in his researches upon dissociation, and to reveal to us the causes of many anomalous densities. In the researches of MM. Deville and Troost the density of iodine was determined as normal at 860° and 1040° , and serves as a point of comparison for the measurement of other densities. M. V. Meyer, on the contrary, finds the density of iodine anomalous after 590° . The want of agreement which seems to exist between these results has led us to take up the study of the subject, making use of a different process for the measurement of the temperatures, and a modified apparatus for the determinations of the density. M. Victor Meyer has been so obliging as to communicate some details on his process in order to facilitate a comparison. The precise determination of the temperature seemed to us of the greatest

importance, and we have not made use of the calorimetric method, because, among other reasons, the fundamental data of this method have not yet been thoroughly established for very high temperatures. It has appeared to us doubtful whether all the parts of a cylinder heated by a naked flame take an equal temperature, and in all the following experiments we surrounded the cylinder with an iron or earthenware muffle. We employed for measuring the temperature an air thermometer described in a paper presented at the last sitting of the Academy, and the experiments on density followed immediately after the determinations of temperature. For this purpose the apparatus was transformed by very simple additions into an air thermometer of the ordinary form. It was closed hermetically after the introduction of the vessel containing the substance to be volatilised, and at a given moment this vessel was let fall from the cold part into the hot part of the thermometer, and the increase of volume was read. The iodine had been purified according to the method of M. Stas, by solution in potassium iodide and fractionated precipitation. We give, in the first part of the following table, the results obtained at different temperatures by M. Meyer, and in the second part our own, but we have reasons for believing that the two series of measurements of temperature are not strictly comparable. The columns D' and \bar{D} give the proportions between the densities found and the theoretic density of iodine = 8.786.

Temperature.	Density.	D' \bar{D}
450°	8.84; 8.85	
586°	8.73; 8.71; 8.71	0.99
842°	6.68; 6.80; 6.80	0.77
1030°	5.75; 5.74	0.66
1570°	5.60; 5.67; 5.71; 5.81	0.63
445°	8.70; 8.78; 8.75	
830° to 880°	8.04; 8.11	0.92
1020° to 1050°	7.02; 7.18; 6.83	0.80
1275°	6.07; 5.57	0.66
1390°	5.23; 5.33	0.60

M. Victor Meyer concludes from his observations that iodine is dissociated in the same manner as the chlorine evolved from platinous chloride; that towards 1000° the density is reduced to 0.66 of the normal density, and remains constant at this figure up to 1570°, which he regards as the highest temperature of the Perrot furnace. We estimate this same temperature at below 1400°, and at this point we find the density of iodine below the two-thirds, which, according to M. Meyer, would be the extreme limit of the dissociation. We find the decrease of density progressive, without stopping-point, from 600° to the limit of our observations. In other terms, if the cause of diminution is dissociation, we may admit that the molecule I_2 tends to resolve itself into 2 atoms. We hope that further experiments will enable us to draw up a curve expressing with sufficient precision the relation between density and temperature, and the results obtained with the halogens allow us to foreshadow the possibility of finding a relation between the anomalous density and the atomic weight—a relation which it will be useful to seek also in the other periodic groups of elements. We admit that the density is normal for chlorine up to 1350°, decidedly anomalous for iodine, and probably intermediate for bromine. To account for the differences between his observations and those of MM. Deville and Troost, M. Victor Meyer suggests that the rapid evaporation and the presence of a permanent gas may favour the dissociation of the iodine. The second of these hypotheses seems sufficient to explain the small differences between the density which we find for iodine at 860° to 1040°, and the figures of MM. Deville and Troost, and before becoming acquainted with the memoir of M. Meyer we pointed out to the Chemical Society that the ingenious connection established by M. Deville between the phenomena of

ebullition and of dissociation may furnish the key for the solution of this problem.

A Method of Producing Acetal.—MM. R. Engel and De Girard.

Specific Heats of Solutions of Potassa and Soda.—M. Hammerl.—The author's results are given in the form of tables.

Alkaloids of the Permanganate Tree.—C. Tanret.—A description of four compounds, to which the author gives the names of pelletierin, iso-pelletierin, methyl-pelletierin, and pseudo-pelletierin. The separation of these bodies and their properties are described.

Artificial Production of a Leucotephrite identical with the Crystalline Lavas of Vesuvius and of Somma: Nascent Crystalline Forms of Leucite and Nephelin.—F. Fouqué and A. M. Lévy.—An illustrated paper, not admitting of useful reproduction.

Artificial Reproduction of Spinel and Corundum.—S. Meunier.—The author brings in contact in a heated tube, aluminium chloride, watery vapour, and metallic magnesium.

L'Orosi. Giornale di Chimica, Farmacia, e Scienze Affini.
No. 12, December, 1879.

A New and very Sensitive Method of Detecting Free Hydrochloric Acid in a Solution of Ferric Chloride.—Prof. Nicola Rease.—The author finds that a solution of ordinary phenol treated with ferric chloride takes an amethyst colouration, turning to a brown. But if a drop of hydrochloric acid is added to the solution the liquid either assumes no colouration at all or assumes a greenish tint. He proceeds as follows:—1 grm. of the crystalline phenol of commerce is heated in 100 c.c. of pure water. He then pours 1 c.c. of the liquid ferric chloride into 50 c.c. of pure water. This solution being in a small test-beaker set on a sheet of white paper, the solution of phenol is added drop by drop. The first drop, if the solution is slightly acid, produces a fugitive colouration, but if it is strongly acid no colour appears. On continuing to add the phenol solution the colour becomes permanent and gradually darkens. From the volume of the phenol solution consumed an approximate idea of the quantity of acid present may be formed.

Action of the Poison of Lupins upon Dogs.—Prof. F. Corso.—The poison of lupins is formed of certain alkaloids derived from coniin. The memoir is physiological rather than chemical in its purpose.

Reduction of Nitrates during Metallic Substitutions.—Dr. Giacomo Guetta.—The metallic nitrates, if heated with a metal capable of substituting itself for the metal of such nitrates, are partially reduced to ammonium nitrate. This reduction the author considers is in great part due to the heat liberated during the substitution of the metals, and which is capable of effecting, in the presence of the electric element formed by the two metals, the decomposition of the water, and of communicating to the hydrogen resulting from such decomposition the power of deoxidising the nitric acid and converting the nitrogen into ammonia.

Analytical Observations on Tin, and the Determination of the Limits of Sensitiveness of the Chief Reagents for Bases.—G. Pellizzari.—After attacking with hydrochloric acid the brown flocks precipitated by zinc from a solution which may contain tin and antimony, there is added potassium sulphocyanide and then ammonium molybdate. If a carmine-red colouration appears the presence of tin is indicated. If traces of antimony have been dissolved by the hydrochloric acid the reaction is not interfered with. The tartaric, oxalic, phosphoric, and acetic acids diminish the colour produced, or even prevent its formation. Heat also weakens the colouration.

Journal für Praktische Chemie.

Nos. 1, 2, and 3, 1880.

Acetyl-urea and certain of its Derivatives.—Dr. R. Leuckart.—The supposition that the bodies produced on the one hand from cyanic ether by union with ammonia, and on the other from ethylamin cyanate by the transposition of its elementary constituents are isomeric, but not identical, has not been confirmed. The nature of diphenyl-urea obtained by boiling both bodies with aniline, and of the diphenyl-biuret obtained from the ethyl-allophanic ethers by the same reaction, render it probable that if urea is to be regarded as carbamic amide, both these ethyl compounds must be regarded as carbamic ethyl-amid.

Thermo-chemical Researches.—Julius Thomsen.—The heat liberated in the reaction (R, O_2, CO) in case of most metals lies between those of the reactions (R, O_2, SO_2) and (R, O_2, N_2O_4), but the differences vary widely. Between the anhydrous carbonates and nitrates the difference of the formation-heat rises from potassium to silver, *i.e.*, from 7980 to 31,290 cal. The relation between the sulphates and the carbonates is the reverse, as here the difference from potassium to silver decreases from 22,620 to 3430 cal. On comparing the formation-heat of the sulphates and nitrates, the same approximate difference is found for the potassium, sodium, thallium, and ammonium salts, namely, on the average, 30,480 cal. There is also an almost equal difference for the salts of lithium, silver, lead, barium, and strontium, namely, 35,770 cal.

Thermo-chemical Researches.—Julius Thomsen.—This important paper, which does not admit of useful abstraction, treats of the chief numerical results of the author's investigations on the phenomena of affinity of the metals.

Contributions to a Knowledge of Cyanamid (Second Memoir: See vol. xi., p. 284).—E. Drechsel.—We have here an account of the preparation of cyanamid from potassium cyanate and ammonium sulphocyanide. The author then describes the various methods of its formation, its conversion into other cyanogen compounds and its constitution.

Formation of Fat in the Lower Fungi.—C. v. Nägeli and O. Loew.—The cells of fungi can obtain the materials for the production of fat from the most varied compounds, whether nitrogenous or not.

The Atomic Weight of Antimony.—F. Kessler.—The author considers the determinations of Schneider and Cooke untrustworthy, and regards the correct atomic weight as = 122.3.

Products of the Reaction of Chloro-carbonic Ether upon certain Amine Bases.—Dr. L. Schreiner.—The author obtains four new compounds, all of which are liquids at ordinary temperatures, whilst the urethans are solid, crystalline bodies.

Certain Ethylen Derivatives of Phenol and of Salicylic Acid.—A. Weddige.—The compounds mentioned in this preliminary communication are ethylen-di-para-nitro-phenol, brom-ethyl-para-nitro-phenol, ethylen-diortho-nitro-phenol, ethylen-disalicylic ethyl-ether, and ethylen-disalicylic acid.

Annalen der Physik und Chemie.

Heft 3, No. 11, 1879.

Remarks on Extra Currents in Iron Wires.—H. Herwig.—A reply to Lorenz (*Annalen*, vii., p. 185).

MISCELLANEOUS.

Yorkshire College of Science.—On the 12th inst. Mr. Hummel, the newly appointed instructor in dyeing and tinctorial chemistry, delivered an inaugural address. He explained in a very clear and forcible manner the

importance, or rather the necessity, of a scientific training for all persons who engage in the tinctorial arts. He denounced the rule-of-thumb system which so many of dyers still persist in following, and referred to certain "occult influences" which have been long ago exposed in the *CHEMICAL NEWS*. He pointed out that in the industrial arts, as well as in science, there is only one royal road to success—systematic experiment. He proposes to conduct his pupils through a series of such experiments, in which every influence which can bear upon the results produced will be carefully studied. Mr. Hummel is an hereditary dyer. His father studied under Chevreul, at the Gobelins, and afterwards held appointments at many dye-works in Italy, France, Austria, and Germany. Mr. Hummel, in turn, studied under Prof. Bolley at the Polytechnic School of Zürich, under the late Prof. Calvert at the Royal Institution, at Manchester, and has been engaged as manager in many important dye- and print-works in England and Scotland. After the conclusion of the lecture Prof. Thorpe made some impressive remarks on the neglect of scientific research in England, and pointed out that since 1860 there was scarcely a British name among the list of the discoverers of new colouring matters. He asked how it was that whilst we had a much larger supply of the raw material from which the coal-tar colours are produced, another country should surpass us in their discovery and their manufacture? The speaker found the reason in the higher educational system of Germany, where a university career is not merely very cheap, but where degrees and honours are awarded only to those who have proved their competence by successful research. We cannot but rejoice at so open a protest against the system of cram and examinationism now dominant in England, and we feel every confidence that the Yorkshire College of Science will prove a focus of invention and discovery.

South London School of Pharmacy.—The Medals and Certificates for the term ending the 15th April were presented by the Secretary, on the 17th April, to the following successful competitors:—Senior Chemistry—Medal, Mr. Price; Certificate, Mr. Carruthers. Junior Chemistry—Medal, Mr. Hill; Certificate, Mr. Waring. Materia Medica—Medal, Mr. Williams; Certificate, Mr. Horne. Botany—Medal, Mr. Wakley; Certificate, Mr. Waring. Pharmacy—Medal, Mr. Waring. The Secretary's Prize for Organic Chemistry was presented by the Chairman (Rev. S. Backe Harris) to Mr. W. Williams.

NOTES AND QUERIES.

Polarisation Symbols.—I am sorely perplexed as to the real meaning of the terms $[a]_D$ and $[a]_D$, and the relation they bear to each other, as applied to sugars and other carbohydrates. If you can give me any assistance, or direct me to where I can get it, I shall feel obliged.—W.M.C. [The symbols in question express the extent to which the plane of polarisation is made to rotate by the sugars, &c. Their respective value will be found in any work which explains the optical analysis of sugars.—Ed. C.N.]

Keeping Salts of Ammonia.—(Reply to H. J. Bland)—A purchaser of salts of ammonia of English manufacture will certainly experience a loss in weight if the goods are kept in store for any time. Taking the *sulphate* as an example: it contains originally 4 to 6 per cent of moisture—independent of any water of crystallisation—and this moisture is rapidly given off in a dry atmosphere; less so during damp weather, and it may even *gain* at first in weight in the latter case, owing to the presence of free (sulphuric) acid, although in a well-made salt this should be very trifling. Keeping it for twelve or eighteen months a loss in weight is unavoidable, and will depend more or less upon the moisture originally present. It is as well to point out, however, that if the sulphate is not stored for re-sale, but by a manure manufacturer to be used up eventually in the preparation of ammoniated compound, there will not be any real loss in money value to the holder, as supposing the sulphate originally contained 24 per cent NH_3 it will in the dry state contain at least 25 per cent, and then the loss in weight by evaporation of water, &c., is made up by the gain in strength of the dry article. The only sulphate that does not lose weight is the American kiln-dried, which has frequently been sold on our markets.—J. H.

TO CORRESPONDENTS.

W. W. Walker.—We do not know of such a work.

MEETINGS FOR THE WEEK.

- MONDAY, 26th.—Medical, 8.30.
 — Society of Arts, 8. Robert W. Edis, F.S.A., "The
 Decoration and Furniture of Town Houses."
 — Royal Geographical, 8.30.
 — Philosophical Club (Anniversary), 6.30.
 TUESDAY, 27th.—Civil Engineers, 8.
 — Anthropological Institute, 8.
 — Royal Institution, 3. Mr. R. H. Scott, "Wind and
 Weather."
 — Royal Medical and Chirurgical, 8.30.
 — Society of Arts, 8. Mr. C. G. W. Lock, "Iceland
 and its Resources."
 WEDNESDAY, 28th.—Society of Arts, 8. Thomas Fletcher, F.C.S.,
 "Recent Improvements in Gas Furnaces for
 Domestic and Laboratory Purposes."
 — Geological, 8.
 — London Institution (Anniversary), 12.
 THURSDAY, 29th.—Royal Society Club, 6.30.
 — Royal, 8.30.
 — Zoological (Anniversary), 1.
 — Royal Institution, 3. Prof. Tyndall, "Light as a
 Mode of Motion: Theories of Light and Colour."
 FRIDAY, 30th.—Royal Institution, 8. Mr. Spottiswoode, "Electricity
 in Transit," 9.
 SATURDAY, May 1st.—Royal Institution. Annual Meeting, 2.

SPECIALITIES.

- MANGANESE** Finest Crystallised, Lump, Ground, and Granu-
 lated of every quality, and PREPARED for
Chlorine, Glass, Steel, Ferro-Manganese,
Pottery, Battery, and EVERY PURPOSE.
- BAUXITE** (Hydrate of Alumina) Of high percentage of
 Alumina and almost free from Iron The
 CHEAPEST, BEST, and MOST PROFITABLE
 article from which to produce ALUM, ALUM
 CAKE, and SULPHATE OF ALUMINA, &c.
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SOME ELECTRICAL MEASUREMENTS OF ONE OF MR. EDISON'S HORSESHOE LAMPS.

By HENRY MORTON, Ph.D., ALFRED M. MAYER, Ph.D.,
and B. F. THOMAS, A.M.,
at the Stevens Institute of Technology.

THE lamp with which the following measurements were made is one of the carbonised paper horseshoe pattern of recent construction, marked No. 154, and kindly loaned to the present writers by the *Scientific American*.

The first object, on receiving the lamp, was to determine roughly what amount and character of electric current would be needed to operate it efficiently. With this view a number of cells of a small Grove's battery were set up, having each an active zinc surface of 20 square inches and a platinum surface of 18 square inches.

The lamp being placed in the situation usually occupied by the standard burner in a Sugg's photometer, the battery was, cell by cell, thrown into circuit.

When 10 cells had been introduced the horseshoe showed a dull red; with 15 cells, a bright red; with 34 cells the light of 1 candle was given; with 40 cells the light of $4\frac{1}{2}$ candles, and with 45 cells the light of $9\frac{1}{2}$ candles, and with 48 cells, 16 candles.

Having thus determined what amount of electric current would be required for experiments, arrangements were made to measure accurately the resistance of the horseshoe while in actual use and emitting different amounts of light. The resistance of this carbon thread at the ordinary temperature had been already determined as 123 ohms in the usual way, but it was presumed, as had been shown by Matthiessen (*Phil. Mag.*, xvi., 1858, pp. 220, 221), that this resistance would diminish with rise of temperature.

To measure the resistance under these circumstances the apparatus was arranged as follows:—The current from the battery was divided in two branches, which traversed, in opposite directions, the two equal coils of a differential galvanometer. One branch then traversed the lamp, while the other passed through a set of adjustable resistances composed of German-silver wires stretched in the free air of the laboratory, to avoid heating. (Careful tests of these resistances showed that no sensible heating occurred under these circumstances.)

Matters being thus arranged, the resistances were adjusted until the galvanometer showed no deflection, when the candle power of the lamp was taken repeatedly in the photometer, and the amount of resistance was noted.

These measurements were several times repeated, shifting the coils of the galvanometer and reversing the direction of the current.

The results so obtained were as follows:—

Resistances.	Condition of Loop.
123 ohms	Cold
94 "	Orange light
83.7 "	$\frac{1}{10}$ candle
79.8 "	5 "
75 "	18 "

The photometric measurement was in all these cases taken with the carbon loop at right angles to the axis of the photometer, which was, of course, much in favour of the electric lamp. On turning the lamp round so as to bring the carbon loop with its plane parallel with the axis of the photometer, *i.e.*, the edge of the loop turned toward the photometer disk, the light was greatly diminished, so that it was reduced to one-third of what it was with the loop sideways to the photometer disk.

Having thus determined the resistance of the lamp when in actual use, it was next desirable to measure the quantity of the current flowing under the same conditions.

To do this the current from fifty cells of battery was passed through a tangent galvanometer as a mere check or indicator of variations, and then through a copper voltameter, *i.e.*, a jar containing solution of cupric sulphates with copper electrodes immersed, and then through the lamp, placed in the photometer.

Under these conditions it was found that during an hour the light gradually varied from about 16 candles at the beginning to about 14 candles at the end, making an average of about 15 candles, measured with side of loop toward disk.

The galvanometer during this time only showed a fall of half a degree in the deflection of the needle.

Carefully drying and weighing the copper electrodes, it was found that one had gained 1.0624 grms.

Now, it is well known that a current of one weber deposits 0.00326 grm. of copper per second, which would make 1.1736 grms. in an hour; therefore the current in the present case must have been on the average—

$$\frac{1.0624}{1.1736} = 0.905 \text{ webers,}$$

or a little less than one weber.

Having thus obtained the resistance of the lamp when emitting a light of 15 candles, namely, 76 ohms, and the amount of current passing under the same conditions, namely, 0.905 weber, we have all the experimental data required for the determination of the energy transformed or expended in the lamp, expressed in foot-pounds. For this we multiply together the square of the current, the resistance, the constant 0.737335 (which expresses the fraction of a foot-pound involved in a current of one weber traversing a resistance of one ohm for one second), and the number of seconds in a minute. Thus, in the present case, we have $0.905^2 = 0.8125$, and—

$$0.8125 \times 76 \times 0.737335 \times 60 = 2753.76 \text{ foot-pounds.}$$

Dividing these foot-pounds per minute by the number of foot-pounds per minute in a horse power, that is, 33,000, we have 0.08, that is, about eight one-hundredths or one-twelfth of a horse power as the energy expended in each lamp.

It would thus appear that with such lamps as this, one horse-power of energy in the current would operate 12 lamps of the same resistance with an average candle-power of 10 candles each,* or 120 candles in the aggregate.

Assuming that a Siemens or Brush machine were employed to generate the electric current, such a current would be obtained, as has been shown by numerous experiments, with a loss of about 40 per cent of the mechanical energy applied to the driving pulley of the machine. To operate these 12 lamps, therefore, we should have to apply more than one horse-power to the pulley of the machine, so that when this loss in transformation had been encountered there should be one horse-power of electric energy produced. This would call for $1\frac{2}{3}$ horse-power applied to the pulley of the dynamo-electric machine by the steam-engine.

To produce one horse-power in a steam-engine of the best construction about 3 lbs. of coal per hour must be burned, and therefore for one and two-thirds horse-power 5 lbs. of coal must be burned.

On the other hand, 1 lb. of gas coal will produce 5 cubic feet of gas, and will leave, besides, a large part of its weight in coke, to say nothing of other "residuals," which will represent practically about the difference in value between "steam-making" and "gas-making coal,"

* This average was determined in the following manner:—The lamp was attached to a horizontal divided circle at its base, and being placed in the photometer its light was then measured at various azimuths, differing by 10° from 0° to 180° . The various quantities so found were added and divided by the number of terms, and it was thus shown that the actual average light emission was the arithmetical mean of the extremes, which were 15 candles and 5 candles respectively. This result was not anticipated, but it so came out on two repetitions of the measurements.

so that it will not be unfair to take 5 lbs. of gas coal as the equivalent of 5 lbs. of steam coal.

These 5 lbs. of gas coal will then yield 25 cubic feet of gas, which, if burned in five gas-burners of the best construction, will give from 20 to 22 candles each, or 100 to 110 candles in the aggregate.

We have, then, the twelve Edison lamps producing 120 candles, and the five gas-burners producing 100 to 110 candles, with an equivalent expenditure of fuel.

If each apparatus and system could be worked with equal facility and economy, this would of course show *something* in favour of the electric light; but when in fact everything in this regard is against the electric light, which demands vastly more machinery, and that of a more delicate kind, requires more skillful management, shows more liability to disarrangement and waste, and presents an utter lack of the storage capacity which secures such a vast efficiency, convenience, and economy in gas, then we see that this relatively trifling economy disappears or ceases to have any controlling importance in the practical relations of the subject.

ON THE

EFFICIENCY OF EDISON'S ELECTRIC LIGHT.

By Prof. H. A. ROWLAND, of the Johns Hopkins University,
and Prof. GEORGE F. BARKER, of the University of
Pennsylvania.

THE great interest which is now being felt throughout the civilised world is the success of the various attempts to light houses by electricity, together with the contradictory statements made with respect to Mr. Edison's method, have induced us to attempt a brief examination of the efficiency of his light. We deemed this the more important because most of the information on the subject has not been given to the public in a trustworthy form. We have endeavoured to make a brief but conclusive test of the efficiency of the light, that is, the amount of light which could be obtained from one horse-power of work given out by the steam engine. For if the light be economical, the minor points, such as making the carbon strips last, can undoubtedly be put into practical shape.

Three methods of testing the efficiency presented themselves to us. The first was by means of measuring the horse-power required to drive the machine, together with the number of lights which it would give. But the dynamometer was not in very good working order, and it was difficult to determine the number of lights and their photometric power, as they were scattered throughout a long distance, and so this method was abandoned. Another method was by measuring the resistance of, and amount of current passing through a single lamp. But the instruments available for this purpose were very rough, and so this method was abandoned for the third one. This method consisted in putting the lamp under water and observing the total amount of heat generated in the water per minute. For this purpose, a calorimeter, holding about 1½ kil. of water, was made out of very thin copper: the lamp was held firmly in the centre, so that a stirrer could work around it. The temperature was noted on a delicate Baudin thermometer graduated to 0.1° C.

As the experiment was only meant to give a rough idea of the efficiency within two or three per cent, no correction was made for radiation, but the error was avoided as much as possible by having the mean temperature of the calorimeter as near that of the air as possible, and the rise of temperature small. The error would then be much less than one per cent. A small portion of the light escaped through the apertures in the cover, but the amount of energy must have been very minute.

In order to obtain the amount of light and eliminate all changes of the engine and machine, two lamps of nearly equal power were generally used, one being in the calorimeter while the other was being measured. They were then reversed and the mean of the results taken. The apparatus for measuring the light was one of the ordinary Bunsen instruments used for determining gas-lights, with a single candle at ten inches distance. The candles used were the ordinary standards burning 120 grains per hour. They were weighed before and after each experiment, but as the amount burned did not vary more than one per cent from 120 grains per hour, no correction was made.

As the strips of carbonised paper were flat, very much more light was given out in a direction perpendicular to the surface than in the plane of the edge. Two observations were taken of the photometric power, one in a direction perpendicular to the paper, and the other in the direction of the edge, and we are required to obtain the average light from these. If L is the photometric power perpendicular to the paper, and l that of the edge, then the average λ will evidently be very nearly—

$$\lambda = L \int_{\frac{1}{2}\pi}^0 \cos a \sin a da + l \int_{\frac{1}{2}\pi}^0 \sin^2 a da$$

$$\lambda = \frac{1}{2}L + \frac{\pi}{4}l$$

In the paper lamps we found $l = \frac{1}{3}L$ nearly; hence $\lambda = \frac{2}{3}L$ nearly.

The lamps used were as follows:—

No.	Kind of Carbon.	Size of Carbon.	Approximate resistance when cold.
580	Paper	Large	147 ohms.
201	"	"	147 "
850	"	Small	170 "
809	"	"	154 "
817	Fibre	Large	87 "

The capacity of the calorimeter was obtained by adding to the capacity of the water the copper of the calorimeter and the glass of the lamp and thermometer. The calorimeter and cover weighed 0.103 kil., and the lamps about 0.035 kil.

First experiment, No. 201 in calorimeter and No. 580 in photometer; capacity of calorimeter = 1.153 + 0.009 + 0.007 = 1.169 kil. The temperature rose from 18.28° C. to 23.11° C. in five minutes, or 1.75° F. in one minute. Taking the mechanical equivalent as 775.0, which is about right for the degrees of this thermometer, this corresponds to an expenditure of 3486 foot-pounds per minute. The photometric power of No. 580 was 17.5 candles maximum, or 13.1 mean, λ .

When the lamps were reversed, the result was 3540 foot-pounds for No. 580, and a power of 13.5, or 10.1 candles mean.

Lamps used in Calorimeter.	Photometer.	Photometric Power Measured perpendicular to paper L.	Average, λ .	Capacity of Calorimeter in pounds.	Rise of temperature in degrees F.	Energy per minute in foot-pounds.	Mean number of candles per horse-power of electricity.	Mean number gas jets of 16 candles each per h.p. of Electricity.	Mean number of gas-jets per indicated horse-power.
201	580	17.5	13.1	2.57	1.75°	3486.0	109.0	6.8	4.8
580	201	13.5	10.1	2.82	1.62°	3540.0			
580	201	38.5	28.9	2.74	2.41°	5181.0			
201	580	44.6	33.5	2.76	2.20°	4898.0	204.3	12.8	8.9
850	809	19.0	14.3	2.81	1.14°	2483.0			
809	850	12.2	9.2	2.79	1.54°	3330.0			
817			17.2	2.73	1.28°	2708.0	209.6	13.1	9.2

The mean of these two gives, therefore, a power of 3513 foot-pounds per minute for 11.6 candles, or 109.0 candles to the horse-power.

To test the change of efficiency when the temperature varied, we tried another experiment with the same pair of lamps, and also used some others where the radiating area was smaller, and, consequently, the temperature had to be higher to give out an equal light.

We combine the results in the following table, having calculated the number of candles per indicated horse-power by taking 70 per cent of the calculated value, thus allowing about 30 per cent for the friction of the engine, and the loss of energy in the magneto-electric machine, heating of wires, &c. As Mr. Edison's machine is undoubtedly one of the most efficient now made, it is believed that this estimate will be found practically correct. The experiment on No. 817 was made by observing the photometric power before and after the calorimeter experiment, as two equal lamps could not be found. As the fibre was round it gave a nearly equal light in all directions, as was found by experiment.

The increased efficiency, with rise of temperature, is clearly shown by the table, and there is no reason, provided the carbons can be made to stand, why the number of candles per horse-power might not be greatly increased, seeing that the amount which can be obtained from the arc is from 1000 to 1500 candles per horse-power. Provided the lamp can be made either cheap enough or durable enough, there is no reasonable doubt of the practical success of the light, but this point will evidently require much further experiment before the light can be pronounced practicable.

In conclusion, we must thank Mr. Edison for placing his entire establishment at our disposal in order that we might form a just and unbiassed estimate of the economy of his light.—*American Journal of Science*, vol. xix., April, 1880.

ON THE CONDITION IN WHICH SULPHUR EXISTS IN COAL.

By Dr. W. WALLACE, Glasgow.

It has been assumed that sulphur exists in coal chiefly, if not exclusively, in the form of bisulphide of iron, the presence of which can be detected in almost all varieties of the mineral. Crace-Calvert asserted many years ago that in some cases, at least, sulphur existed in coal in the form of a sulphate, and he gave a process for the estimation of the sulphur existing in this form. Some analyses which I have recently made have shown conclusively, however, that in some coals the greater part of the sulphur exists as an organic compound. These coals contain but a small proportion of iron—not nearly enough to form bisulphide with the iron that exists in them—and one well known description known as the Ell coal was found not to contain a trace of sulphuric acid when tested by Crace-Calvert's method. The others were not tested.

The following table shows the relative quantities of total sulphur and that existing as pyrites, as calculated from the iron found in the ash of the coal, assuming that all the iron exists as bisulphide, which is not always the case, as some coals contain carbonate of iron.

	Total Sulphur.	S. as Bisulphide.
Ell coal (Lanarkshire)	0.91	0.11
Main coal „	0.60	0.42
Splint „	0.46	0.14
Pyotshan „	0.68	0.17
Soft coal from Fife	0.93	0.49

The estimations of sulphur were made by Pattinson's method, and also by fusion with sodic carbonate and

potassic nitrate. When carefully performed and blank experiments made to eradicate sources of error, these methods give concordant results. It is well known that boiling with nitric acid does not give the whole of the sulphur, and I find that the amount obtained usually corresponds closely with that existing as pyrites.

The following analyses of the coals referred to may be useful for comparison with English varieties, of which the composition is given in several works on chemical technology.

	Ell.	Main.	Splint.	Pyotshan.	Fife.
Carbon	71.37	67.35	70.05	72.21	61.88
Hydrogen	5.13	5.03	5.24	4.79	4.89
Oxygen	8.88	12.66	12.37	9.77	12.78
Nitrogen	1.44	1.40	1.36	1.51	1.14
Sulphur	0.91	0.60	0.46	0.68	0.93
Ash	0.91	3.60	3.80	2.74	3.92
Water at 100° C. ..	11.36	9.36	6.72	8.30	14.46
	100.00	100.00	100.00	100.00	100.00
Spec. grav.	1.255	1.293	1.282	1.280	1.250
Heat units, C... ..	7190	6628	6929	7062	6132
„ F... ..	12,942	11,932	12,472	12,711	11,037
Lbs. of water evapd. by 1 lb. of coal (theoretical)—	13.20	12.18	12.74	13.00	11.21

In calculating the last figure allowance has been made for the evaporation of the water contained in the coal. Some authorities give the results of analyses of coals in the dried state, but such statements are of little practical value and are apt to mislead consumers of fuel. The water in coal, although readily expelled by heat, varies very little in samples taken from the pit at different times, and, in fact, is as constant as the composition of the coal itself, which varies sensibly in different portions of the field.

THE ATOMIC WEIGHT OF ANTIMONY. PRELIMINARY NOTICE OF ADDITIONAL EXPERIMENTS.*

By JOSIAH P. COOKE.
Erving Professor of Chemistry and Mineralogy.

IN our previous paper on this subject,† we gave our reasons for the opinion, since fully confirmed, that the bromide of antimony is the most suitable compound of this element as yet known for determining its atomic weight; and the results of fifteen analyses of five different preparations of the bromide were published, which gave for the atomic weight in question the mean value 120.00 with an extreme variation between 119.4 and 120.4 for all the fifteen analyses, and between 119.6 and 120.3 for the six determinations in which we placed most confidence. The antimonious bromide used in these determinations was purified first by fractional distillation, and secondly by crystallisation from a solution in sulphide of carbon. In the crystallised product thus obtained, the bromine was determined gravimetrically as bromide of silver in the usual way. Although it seemed at the time that the results were as accordant as the analytical process would yield under the unfavourable conditions, which the presence of a large amount of tartaric acid in the solution of the bromide of antimony necessarily involved; yet it was obvious that the agreement was far from that which was desirable in the determination of an atomic weight, and our chief confidence in the accuracy of the mean value—independently of its remarkable agreement with previous results—was based on the fact that the known sources of error tended to balance each other. Hence our conclusions were stated with great caution, and the hope was

* Contributions from the Chemical Laboratory of Harvard College. Presented to the American Academy of Arts and Sciences, March 10, 1880.

† *Proceedings of the American Academy of Arts and Sciences*, vol. xii, p. 1.

expressed that after a more thorough investigation of the subject we might be able "to return to the problem with such definite knowledge of the relations involved as will enable us to obtain at once more sharp and decisive results than are now possible." Unfortunately this investigation has been delayed by causes beyond our control.

In our previous paper we described a simple apparatus which we devised for subliming iodide of antimony; and in a note to the paper we stated that we were applying the same process to the preparation of the bromide of antimony, and that it promised excellent results. Our expectations in this respect have been fully realised, and the product leaves nothing to be desired either as regards the beauty or the constancy of the preparation. The fine acicular crystals are perfectly colourless, and have a most brilliant silky lustre. With ordinary precautions they can be kept indefinitely without change, and it is easy therefore to determine the weight of the material analysed to the tenth of a milligramme.

We have carefully studied the causes of error involved in the analytical process of determining bromine in an aqueous solution of bromide of antimony and tartaric acid by the usual gravimetric method. These causes we propose to discuss in a future more extended paper. In this preliminary notice, we have only space to state that we have satisfied ourselves that the small differences between the results previously obtained arose wholly from the analytical process, and not from any want of constancy in the material analysed; and further that these sources of error are to a very great extent under control. Moreover, we have found that the volumetric determination of bromine by silver was not materially affected, if at all, by the same causes. We have thus been led to devise a mode of testing the atomic weight of antimony, which, while it has all the advantages of the gravimetric method previously employed, is free from its sources of error.

If the atomic weight of antimony were 122.00, it would require 1.7900 grms. of pure silver to precipitate the bromine from a solution of 2.0000 grms. of antimony bromide, while if the atomic weight of antimony were 120.00 it would require 1.8000 grms. of silver. Now it is easy to estimate volumetrically 1-100th of this difference with great certainty. We therefore prepared with great care a button of pure metallic silver, which we annealed and rolled out to a thin ribbon. We then weighed out from 2 to 4 grms. of bromide of antimony, prepared by sublimation as described above, and dissolved this salt in an aqueous solution of tartaric acid, which we then transferred to a litre flask and diluted to about 500 c.c. We next very accurately weighed out a quantity of silver slightly less than that which calculation showed was required for complete precipitation. This silver was dissolved in nitric acid, and the solution having been evaporated to dryness over a water-bath, the silver salt was washed into the flask containing the bromide of antimony. As soon as the supernatant liquid had cleared, the small additional amount of a normal silver solution required to produce complete precipitation was run in from a burette, and measured with the usual precautions. We used no extraneous indicator, because it was important not to introduce any possibly new disturbing element into the experiment, and in the titration of bromine with silver the normal and familiar phenomena, which mark the close of the process, furnish a very sharp indication. The details of one of the determinations were as follows:—

The weight of the bromide of antimony used amounted to 2.5032 grms. To precipitate the bromine from the solution of this material 2.2404 grms. of silver would be required if $Sb = 122.00$ and 2.2529 if $Sb = 120.00$. We weighed out, with as much accuracy as if we were adjusting a weight, the smaller of these two quantities of metallic silver, and after dissolving the pure metal in pure nitric acid, evaporating the solution to dryness, and redissolving in water, we added at once the whole of this silver solution to the litre flask containing the solution of bromide of antimony, in the manner described above. It

was then found that 12.4 c.c. of a normal silver solution (1 grm. of silver to the litre) were required to complete the precipitation. It will be seen that the weights of the bromide of antimony and silver used could be thus determined with the most absolute precision, and we have the greatest confidence in these values to the one-tenth of a milligramme. Moreover, it will be noticed that the volumetric method is only used to estimate the difference in the atomic weight which has been in question, and that if the method were only accurate to the one-tenth of the quantity to be measured it would give us the value of the atomic weight within two-tenths of a unit; while if, as we had reason to believe, the process was accurate within 1 per cent, it would fix the atomic weight within two-hundredths of a unit.

By the method just described, the following results were obtained. The letters *a* and *b* indicate different preparations.

	Wt. of SbBr ₃ taken.	Total Wt. of Ag used.	Percent of Br, Ag=108, Br=80.	Corresponding value of Sb.
(a) 1.	2.5032	2.2528	66.6643	120.01
(a) 2.	2.0567	1.8509	66.6620	120.02
(a) 3.	2.6512	2.3860	66.6644	120.01
(b) 4.	3.3053	2.9749	66.6606	119.98
(b) 5.	2.7495	2.4745	66.6653	120.01
Mean value	66.6651	120.01
Mean value of 15 gravimetric determinations previously published	66.6665	
Theory Sb 120 requires	66.6666	
" Sb 122	66.2983	

In order still further to control the work, we collected the bromide of silver formed in the last two determinations, washing the precipitate with the precautions which experience had shown to be necessary, and determining its weight, first, after drying at 150° C., and, secondly, after heating to incipient fusion. In *b* 6 there was a loss of one-tenth of a milligramme; in *b* 7 a loss of two-tenths of a milligramme only at the second weighing. This is an absolute proof that there could be no sensible occlusion of any tartaric acid or any tartrate by these precipitates, and, as stated in our original paper, the same test was frequently applied, although not always, in our previous determinations. It is also evident that these last experiments give us two essentially distinct determinations of the atomic weight, although the materials employed were identical with those of *b* 4 and *b* 5.

	Wt. of SbBr ₃ taken.	Wt. of AgBr determined.	Percent of Br, Ag=108, Br=80.	Corresponding value of Sb.
(b) 6.	3.3053	5.1782	66.665	120.01
(b) 7.	2.7495	4.3076	66.667	120.00
Mean value..	66.666	120.00

Lastly, it is obvious that these gravimetric determinations, taken in connection with the corresponding volumetric results, give us the most conclusive evidence of the purity, both of the metallic silver used, and also of the bromide in the bromide of antimony, which is the basis of this atomic weight investigation. By comparing *b* 6 and *b* 7 with *b* 4 and *b* 5 respectively, we obtain the following data:—

1. 2.9749 grm. of silver gave 5.1782 grm. bromide of silver.
2. 2.4745 " " " 4.3076 " " "

Hence it follows that, as shown by these experiments, the proportions of the silver to the bromine were respectively:—

1.	108.00 silver to 79.99 bromine.
2.	108.00 " 80.01 "
Mean value,	108.00 " 80.00 "

This is the ratio of the atomic weight of silver to that of bromine, and corresponds to the second decimal place

with the determinations of Stas as well as with those of Dumas.

In conclusion it gives us pleasure to express our obligations to Mr. G. De N. Hough and Mr. G. M. Hyams, two students of this laboratory, who have greatly aided us in the experimental work of this investigation.—*Proceedings of the American Academy of Arts and Sciences.*

A NEW METHOD OF TAKING THE SPECIFIC GRAVITY OF LIQUIDS.

By H. SOMMERKORN.

THE apparatus required is a thin-sided glass tube, divided into millimetres, and of from 3 to 4 centimetres in diameter, and a thin circular plate of exactly the same diameter, held by a string. The tube is closed with the plate, and the apparatus plunged into the liquid to be examined, pulling the plate against the tube with the thread. If the tube is plunged deep enough the hydrostatic pressure causes the plate to adhere. If the tube is then slowly raised upwards in a vertical position, we reach a point where it merely hovers, and at the next it sinks. This point is easily observed. In the same moment the depth to which the tube is immersed in the liquid is read off on the scale.

If the specific gravity of the liquid = s , the superficies of the plate of glass or platinum = a , and its weight = G , and the depth to which the tube was immersed in the liquid = h . At the moment when the plate is about to sink its weight, G , must be equal to the weight of a column of liquid of the height, h , the area, a , and the specific gravity s , or a, h, s , which pressure, being equal to the weight of the plate,—

$$s = \frac{G}{a \cdot h}.$$

The factor $\frac{G}{a}$ is the same for all measurements, h only being a variable quantity.

ON THE HISTORY OF ATOMISTIC PERIODICITY.

By LOTHAR MEYER.

THE author is induced to refer to the origin of the doctrine of the periodicity of atomic weights in order that his share in the matter may not be forgotten. As long as the so-called equivalent weights of Gmelin were made use of a universal periodicity could not be detected. After Cannizzaro had established the correct principles for the determination of atomic weights, the regularities which had been observed up to that time took shape in the first edition of my "Modern Theories," in 1864.

A candid scrutiny there will be found in these tables, something more than a "simple collocation of groups of analogous elements"—namely, the endeavour to arrange bodies according to the magnitude of their atomic weights, and to show that chemical value varies regularly in accordance with atomic weight. Only the group Cu, Ag, Au was placed at the end of the second table because it contained elements of unequal value, concerning which I then doubted whether they belonged together. Otherwise the two tables contained about the half of what we now regard as the natural system of the elements. It likewise did not escape my notice that the second table with the group Zn, Cd, Hg could be joined to the left side of the first, and thus united with it to form a whole. In consequence of this observation I attempted to introduce all the other elements into the table. I could not succeed, as I was misled by the erroneous atomic weights, Mo=92, Nb=97.6, Vd=137, Ta=136.6, instead of Mo=96, Nb=94,

Vd=51, Ta=182. After these errors were corrected I had no difficulty in inserting all the elements in a single table arranged according to the magnitude of the atomic weights.

Before I made this public there appeared in the *Zeit. für Chemie* (New Series, vol. v., p. 405) an extract from a memoir by Prof. Mendeleeff containing a tabular arrangement of the elements. In the text accompanying the table it was remarked that the elements, when arranged according to the magnitude of their atomic weights show a serial modification of their properties; that the atomic weights determine these properties; that some atomic weights require correction; and that the discovery of new elements was to be foreseen, besides other less important observations. These points of view were published by Prof. Mendeleeff earlier than by me, and probably than by anyone, unless the claims of priority of Mr. Newlands—which I have hitherto been unable to examine—should be justified.

We see at the same time that Prof. Mendeleeff's table does not contain a single series advancing according to the magnitude of the atomic weights, but three series, which cannot be connected together and are very unequal in length. This is the more striking, as such discontinuity was produced merely by the introduction of seven uncertain, and, as we now know, inaccurately, determined atomic weights (Er, Y, In, Ce, La, Di, Th). If Mendeleeff had then laid any weight upon the establishment of a single series he would doubtless have assumed these weights differently.

My treatise, "The Nature of the Chemical Elements as a Function of their Atomic Weights" (*Ann. Chem. Pharm.*, 7, Supplement, p. 354), which was written in December, 1879, and published soon after, contained the first conspectus of all the atomic weights then accurately known arranged in a single series, that of indium being corrected in accordance with the periodic law, as was soon afterwards confirmed by Bunsen.

This arrangement is derived from a completion of my former attempt, from which it is externally distinguished merely by the circumstance that the successive atomic weights are not placed horizontally, but, as is done by Mendeleeff, vertically. It is distinguished from the tables of this chemist by the removal of the inaccurate atomic weights above mentioned. I showed, at the same time, by occasion of my graphic representation of the atomic volumes, that these elements which I had introduced possess only a part of the properties of the elements to which they were placed as corresponding so that certain properties do not recur before the next period but one, and the periodicity must be regarded as twofold. Prof. Mendeleeff subsequently, without making mention of me, utilised this fact as the distinction between his "even" and "odd" series.

Prof. Mendeleeff has, in his justly celebrated memoir, "The Periodic Regularity of the Chemical Elements," adopted as correct all that I had added to his work, but without the least mention of my researches. He gives a second table, which resembles mine much more closely than did his first, only that, like my first, the series are arranged horizontally. He introduced a number of new and weighty considerations which greatly extend our knowledge of the subject. I include here, above all, the introduction of the chemical value of the elements as determined from the composition of their non-volatile compounds, especially the oxides, and the no less bold than fortunate prediction of the properties of undiscovered elements such as gallium and scandium.—*Berichte der Deutschen Chemischen Gesellschaft.*

Behaviour of Phenanthren-quinon with Ammonia.—E. v. Sommaruga.—The reaction is not the same as in the case of isatin, and the number of the products is much greater. Among the compounds obtained and described are diphenanthren-oxytriimide and isodiphenanthren-oxytriimide.—*Wiener Anzeiger*, 1880, 20.

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Ordinary Meeting, April 24, 1880.

Prof. W. G. ADAMS in the Chair.

NEW members:—The Marquis of Blandford, Mr. J. Marshall.

Prof. G. C. FOSTER read a note by Prof. ROWLAND, of Baltimore, U.S., on the discovery of Mr. Hall that a magnet exercises an electromotive force on a current in a conductor crossing its field, as well as a force on the conductor itself. This fact will render it necessary to apply a correction to equations which assume that only the latter force acts. The electromotive force in question is at right angles to the direction of the current and to the lines of magnetic force. Prof. Rowland expresses it mathematically in this note, and bases a new method of determining the value of v , the ratio of the electrostatic to the electromagnetic unit of electricity, which gives v almost identical with the velocity of light, thus confirming Clerk-Maxwell's theory of the nature of light.

Dr. J. HOPKINSON, F.R.S., suggested an expression for one of Prof. Rowland's results.

Prof. FOSTER also read a note by Prof. WILD, of the Central Russian Meteorological Observatory, on a mode of correcting the bifilar magnetometer for torsion of its fibres, &c., and a method for finding the horizontal component of the earthy magnetism by its aid.

Mr. RIDOUT, F.C.S., described an improved thermo-electric apparatus of his construction. The author has followed the idea of combining the thermo-pile, and galvanometer in one instrument on the same base-board. The defects of the apparatus as ordinarily made are a too great disparity between the resistance in the pile and in the galvanometer; the junctions of the pile are too deep, and short circuit the current; the bars too long and resisting, as well as too numerous; the junctions too slender; the mass of matter to be heated too great. These defects are remedied by placing the bars in glass tubes connected with thin plates of copper, making the bars half the usual length and using only a single pair. The defects in the galvanometer are that the wire does not come near the needle; the needles are not of the best form; and the suspension is troublesome. Mr. Ridout makes the wire a flat ribbon mounted on one bobbin; the needles are flat oblong plates from the same piece of steel and magnetised in one piece; they are mounted on a pivot turning in an agate cup. The several parts of the apparatus are mutually adapted to each other; and in using it the galvanometer is not joined to the pile till the latter has been exposed to the heat, so as to prevent the current generated abstracting heat from the hot side. As made by Mr. Browning the pile consists of a pair of elements $\frac{1}{2}$ in. long, the copper connections being circular plates $\frac{1}{10}$ in. thick, and $\frac{3}{8}$ in. diameter. The pile is supported by thick copper terminals above the galvanometer, which consists of a copper ribbon, making some 20 turns round a pair of astatic needles, 1 in. long and $\frac{1}{8}$ in. broad, pivotted in an agate cup. A contact key, placed on one side, and the whole is enclosed in a glass shade perforated opposite the pile. A glass cone protects the front from extraneous heat, and a glass case the back. A directing magnet is fixed above the pile. Contact between the galvanometer and pile is made after, say, 30 secs. exposure to the heat. The pile is affected by a person standing 6 feet from it, and the radiation from stellar space is evident in clear weather. Half a minute is sufficient to put the instrument ready for use.

Mr. RIDOUT also exhibited laboratory experiments, showing cohesion in mercury by causing it to overflow up an inclined trough; electrolysis of water by a single Grove or bichromate cell through diminishing the pressure

in the flask containing the water by boiling it and condensing the vapour on cooling; a differential thermometer showing absorption of heat on liquefying solids; and the production of musical notes in glass tubes by contracting the bore smoothly to about one-fourth of the diameter at one part.

Prof. FOSTER remarked that the cohesion experiment might show the surface tension.

Prof. GUTHRIE and Prof. HUGHES offered remarks on the electrolytic experiment, the latter stating that he finds the resistance of an iron cell he has constructed to depend on the electrodes rather than the liquid; when the negative plate is tempered iron the resistance is low, when soft iron it is high.

Prof. STONE exhibited photographs of König's new tonometer, described by him at the last meeting, and further mentioned that König had devised a thermometer based on the principle that changes of temperature produce corresponding changes in the vibration-rate of a tuning-fork. The temperature is formed from the rate of the fork by bringing it to a zero rate by means of a rider.

Prof. MICHIN then described his experiments to solve the problem of transmitting light by photo-electric action. Two years ago he conceived the idea of employing for this purpose the fact that light falling on a sensitised silver plate disengages electricity. He forms a sensitive cell composed of two silver plates immersed in a conducting solution; one plate is coated with a sensitive emulsion of chloride or bromide of silver. When chloride is used a solution of salt in water forms the liquid, when bromide a solution of bromide of potash. A current is set up in this cell even in the dark, but when exposed to the magnesium light the current is very powerful, and flows within the cell from the uncoated to the sensitised plate. Prof. Michin also conducted this current to wire to a second cell in a dark chamber, and found that it effected a decomposition of the sensitive plate in that cell, as shown by a distinct darkening of the plate when "developed" by pyrogallic acid. The same effect was produced whether the current was reversed or not. Prof. Michin is continuing his experiments, and has provided a cable containing a number of separate conductors, insulated from each other, in order to convey the currents from several cells.

Prof. PERRY feared that the effect would not be strong enough; but

Prof. MICHIN said the light of a match produced a decided photo-electric effect in the cell.

Prof. PERRY alluded to the selenium plan proposed by himself and Prof. Ayrton, and said that Mr. Willoughby Smith had observed selenium to be sensitive to the shadow of a flying swallow.

Prof. ADAMS testified to the sensitiveness of selenium, and its power of being directly excited by light, a fact first proved by the experiments of Mr. Day and himself.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 13, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"The Radiograph," by D. WINSTANLEY, F.R.A.S. I have described already one of the several arrangements which I have devised for the automatic registration of the solar radiance.* The instrument in question places a lead pencil on a sheet of paper and writes down there-with when, and for how long, the sun may chance to shine, but it makes no record of the intensity of his rays. I will now ask your attention to the description of another and much more perfect apparatus, one which continuously

* Proceedings of Manchester Lit. and Phil. Soc., November 18th, 1879.

records the intensity of thermal radiation to which it is exposed. This instrument I have called the "Radiograph." It consists essentially as follows:—A differential thermometer of which the stem is circularly curved is mounted concentrically upon a wheel of brass in a groove cut with that object for its end. This wheel is supported with its plane in a perpendicular position by a knife edge of hardened steel which passes through its geometric centre and rests on agate planes. The tube of the thermometer is partly filled with mercury—preferably through half its curve—and for the reason given in my description of the simple sunshine recorder, to which I have alluded, a little sulphuric acid is introduced as well. If we now arrange it that the centre of gravity of the solid portion of the system here described shall be below the surface of the planes on which it turns (and the apparatus is provided with adjustments by means of which the point in question can be moved) it is clear that the arrangement may be made to swing in pendulous oscillations, notwithstanding the presence of the liquids it contains, for these remain substantially at rest whilst the tube which holds them does, in fact, slide over them (and with very little friction too) in swinging to and fro through arcs of the circles of which its parts are curves. Both bulbs of the thermometer are closed. It is obvious, therefore, that the tension of the air or gas which they contain will be uninfluenced by the barometric variations of the outer air, the temperature of which latter being experienced equally in each bulb will also leave the equilibrium of the apparatus undisturbed. When, however, one bulb is more heated than the other, the air contained therein will press more strongly on the heavy liquid piston in the tube and wheel the swinging portion of the system round until a fresh position of equilibrium is gained, and this will be (providing that the centre of gravity of the system has previously been made coincident with the point on which it turns) when the tension of the gases is equal in both bulbs. In fact, in so far as now described, the instrument is a differential thermometer, and is that alone, differing in this from Leslie's, that it is a solid and accessible portion of the thing which moves, and not the liquid it contains. When, however, one of the bulbs is blacked and the other one is silvered or left clear, the apparatus becomes a "Radiometer" in the proper meaning of the term, that is to say, a measurer of the thermal radiance to which it is exposed and the intensity of which it indicates by variations in the angular positions of a needle prolonged from one or other of the radii of the wheel.

It is only needful now to so arrange it that this needle shall make a tracing of its curves on a cylinder driven by clockwork at an even speed, and the "Radiograph" is complete.

Concerning the actual instrument I use, its wheel is 7·3 inches in diameter, and the weight thereof a trifle more than two pounds. The other portions of the apparatus are of the same dimensional proportions as are indicated in the sketch. Of course some delicate method of recording has to be employed, and I have thus far used the smoked paper process so much adopted in the observatories of France. In this way the "Radiograms" which illustrate this paper were obtained.

When using the instrument to record the radiance of the sun I have hitherto exposed it in a box of copper surmounted by a dome of glass into which the bulbs of the thermometer project. The line which joins them is in the plane of the meridian of the place and the black bulb to the north. The box itself is supported at an elevation of four feet or thereabouts upon a stand of wood, the legs of which are firmly embedded in the ground. The stand itself is located at the extremity of a garden which overlooks a valley and the sea. A small window in the box permits the movements of the train to be seen and the promptness with which the apparatus acts to be observed. If a cloud "no bigger than a man's hand," and "light as a feather" in its texture, floats before the sun, and occupies but three or four seconds in its transit, its presence,

the duration of its passage, and the degree of thermal obscuration it effects are at once set down.

The cylinder of the radiograph passes over a space of 0·875 of an inch per hour, a somewhat open range, but, as will be seen on reference to the tracings, the needle often moves for some considerable distance in both directions along the same thin line, thereby showing a practical instantaneity of action under very ordinary thermal changes in the radiance from the sky. The influence of the sun's rays at daybreak is almost always shown, for some minutes at any rate, before the sun himself is seen, and occasionally it would seem even for hours before his time to rise.

It is not, however, now my purpose to dwell upon the interesting changes which take place in the intensity of the thermal radiance from the sky, my present object being to describe an instrument by means of which they may be recorded or observed. Doubtless in several of its details the "Radiograph" may be improved, notably in the condition of its bulbs, and it would unquestionably be better if it computed for itself the areas included by its curves. This, I dare say, I shall presently enable it to do. Meanwhile, as a recorder of the duration and intensity of radiant heat the instrument, so far as I have seen, is the only one whose readings are uninfluenced by the temperature or the pressure of the air.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 19, 1880.

C. BAILEY, F.L.S., President of the Section, in the Chair.

"Bog Butter (*Butyrellite*), from Co. Galway, Ireland."

MR. JOHN PLANT, F.G.S., exhibited a piece of a mineral resin familiarly known in the west of Ireland as Bog Butter. The lump weighed exactly 14 ozs. It came from a good depth in a bog in County Galway. A few years ago, when in that part of Ireland, he had been unsuccessful in meeting with a sample of this curious substance, although he was informed that it was not unfrequently met with by the turfcutters during each summer. He heard of its origin and of some of the uses to which it was said to be put by the poor people, if they got any of it, from a farmer at Killkee, but he could hardly credit the statement that in hard times it was melted down and actually used as a dripping to the potatoes; he rather concluded that the greasing was limited to the axles of the potato cart. The Irish have a widespread belief that bog butter was hidden by the fairies in the bogs long ages ago; and it is affirmed that the butter is sometimes found in small wooden kegs in bogs along the coast. These kegs they say have been hastily buried by smugglers running a cargo of contraband, though when bog butter was declared an illegal article of trade in Ireland they are unable to say. Unfortunately, Mr. Plant was not shown a keg, or even a staver of a keg, but he was informed that specimens of veritable kegs of bog butter are to be seen in the Museum of the Royal Irish Academy and in the museums at Edinburgh. The fairy origin of the bog butter he thought might be ascribed to the active imagination of the Celtic brain, many of the inexplicable things in nature being readily put down to the good or evil doings of the indigenous fairies of Erin.

By the aid of scientific analysis the substance called bog butter can be shown to be a perfectly natural production arising from the decomposition of the vegetable matters forming the peat or bog, and to belong to the numerous family of mineral resins, or hydrocarbon compounds, of which Dana describes the composition of seventy species.*

Many of these are very well known under the names of marsh gas, petroleum, ozocerite, asphaltum, naphtha, paraffin, bitumen, amber, torbanite, coal, and its varieties.

Some of these singular minerals are obtained only from bog and peat beds.

* Dana, "System of Mineralogy," 5th edition, 1875, pp. 720–760.

Some time ago Mr. Plant showed to the Section a quantity of one of these resinous minerals, which occurred under the bark of pine logs found in a moss at Handforth by Mr. P. G. Cunliffe. It proved to be known in Germany as Fichtelite, but had not before been known to occur in Great Britain. Afterwards it was found in pine logs in the peat on Lindow Common. A waxy, greasy, or butter-like character is distinctive of these bog products. The one now exhibited was described first by Brazier in 1825, and was analysed by Williamson in 1845, its composition being given as

Carbon	73.78
Hydrogen	12.50
Oxygen	13.72

When fresh from the bog it is soft and like butter, but hardens in drying. The mass is dirty and bogstained on the outside, but inside pure white and free from impurities. It melts at 50° C., and becomes a yellow greasy resin, dissolves in alcohol or in ether, and then crystallises in beautiful needles. When heated it gives off a peculiar odour like acroline. By saponification with potash it yields an acid which Brazier proves to have a composition similar to palmitic acid.

There is a mineral waxy resin called Guyaquillite, which is found in extensive deposits in the marshy plains near Guyaquil, in South America, which has a similar composition to bog butter.

Johnson gives it as—

Carbon	76.67
Hydrogen	8.17
Oxygen	15.16

It has been proved that the slow decomposition or change in the vegetable peat or moss will produce elements of which these hydrocarbons are made.

NOTICES OF BOOKS.

Water Analysis for Sanitary Purposes, with Hints for the Interpretation of Results. By E. FRANKLAND, Ph.D., D.C.L., F.R.S. London: Van Voorst.

PROF. FRANKLAND'S experience in connection with the analysis of water for sanitary purposes has been exceptionally great, and the present work, as affording a clear and well-arranged summary of the method which he has been led to prefer, will be highly valued by chemists. We may state in the outset that we have grave doubts whether any of the known procedures for the analysis of water can be received as perfectly satisfactory or as affording more than an approximation to the truth. Were this otherwise a much greater degree of unanimity would prevail amongst chemists, for scarcely anything, in these days, finds its level more rapidly than an analytical method. Yet it is only a short time since one and the same sample of water was submitted to three chemists, all men of standing and of admitted skill, and all specialists in this department of analytical chemistry. Yet what was their verdict? Professor A. declared it totally unfit for use; Professor B. pronounced it fit for drinking, but not fit for washing; and Dr. C. held it fit for washing, but not fit for drinking. Surely, then, water-analysis has not reached that perfection which might reasonably be demanded.

Prof. Frankland has been considered by some speakers and writers too nice, too exacting, in his views of what constitutes a safe drinking-water. Surely, however, in a matter of so much importance it is better to err on the side of safety. It is surely wiser to destroy all the stray dogs of a city rather than let one rabid cur escape, and in

like manner it is better to condemn a suspicious water than to recommend it for use. It may be true, as a certain chemist asserts, that 1 part of human excrement mixed with 20,000 parts of water may be drunk with safety, but, loathing apart, we should lack courage to make the experiment. Perhaps some day we shall learn to detect the disease-germs, if such exist, in water.

Prof. Frankland lays little weight on the gases held in solution in a water, and consequently he does not recommend the "hydrosulphite" method for the determination of the oxygen which has recently come into some note in France. Nor does he refer as a preliminary guide to the organisms, animal or vegetable, which are to be met with in streams and wells. For the determination of ammonia, existing in the free state or as ammoniacal salts, he adopts the same process as does Prof. Wanklyn, *i.e.*, distillation along with sodic carbonate, and subsequent Nesslerising. The albuminoid ammonia process, it scarcely need be said, the author does not recognise.

For the determination of nitrogen in nitrates and nitrites Prof. Frankland recommends the "mercury process," but for those who do not practise gas analysis he considers the "zinc-copper couple" of Gladstone and Tribe, and the aluminium process of Chapman, the most convenient. The "indigo method" without certain precautions is apt to give very fallacious results.

As regards the determination of poisonous metals we find no instructions for chromium, which, now potassium dichromate is so extensively employed in dyeing, is liable to find its way into the rivers of the manufacturing districts.

As regards organic impurities, an account is given of the methods of Dittmar and Robinson, of Dupré and Hake, and of what is generally known as the permanganate process. Though admitting that the indications afforded by this last process are true only when the organic matter present is substantially alike in composition, and that there is "no relation between the absolute or relative weight of different organic matters and the oxygen which such matters abstract from permanganate," he yet holds that when the analyst "shrinks from" the combustion-process he will be more likely to rate a water at its proper value by this process than by any other.

The combustion-process is described in full, with the aid of the necessary illustrations. In the evaporation of strongly ammoniacal waters, boracic acid or borax is substituted for sulphurous acid; but as boracic acid does not completely destroy carbonates, a separate carbon determination, with sulphurous acid as the acidifying agent, becomes necessary.

No additional evidence on the accuracy of the combustion-process is adduced, nor do we find any reference to the source of error recently pointed out as common to this and to Prof. Wanklyn's process, *i.e.*, the possible escape of volatile organic matter during evaporation or ebullition.

An Appendix contains a list of the reagents necessary for water-analysis, a selection of useful tables, and certain remarks taken from the Reports of the late Rivers' Pollution Commissioners.

The author evidently prefers spring and deep-well waters for dietetic purposes. But as it cannot be shown that they are more salutary than mountain waters, and are very ill-fitted for boiling vegetables, for washing, and for manufacturing uses, the palm should surely be awarded to the soft mountain and moorland waters, which are preferable for every purpose save drinking and brewing.

We quite agree with Prof. Frankland that the Lea and the Thames should be abandoned as sources of potable waters, and indeed that rivers flowing through populous and cultivated districts should be entirely rejected. With his commendations of the water of the Kent Company we cannot fully agree. To wash the person with it is, to anyone who has lived in the north of England or in Scotland, simply misery, and the waste of soap involved is very serious.

Glasgow Sewage. Reports by Dr. W. WALLACE, F.R.S.E F.C.S., &c., from May 20 to November 7, 1879.

DR. WALLACE has reports here on experiments made to determine the action of the purified sewage of Glasgow upon the water of the Clyde. For this purpose a portion of sewage was treated with lime alone in the proportion of one ton per million gallons, and another portion with sulphate of alumina and lime, of course in succession. The two effluent waters, after filtration through pounded coke, were mixed with eight times their bulk of clean (?) Clyde water, such being the proportion which the sewage of Glasgow bears to the ordinary flow of the Clyde. The samples were then kept both in closed and open vessels for six weeks. During the whole of this time they remained clear and transparent, and free from odour. On analysis nearly the whole of the ammonia had disappeared, whilst nitrites were present in considerable quantity. Hence it appears that, even without contact with the air, the admixture with eight times its bulk of Clyde water is sufficient to oxidise and render harmless the organic matter in the purified sewage. The crude sewage is not oxidised in the same manner. It is remarkable that the organic ammonia remaining in the sewage after treatment with sulphate of alumina and with lime respectively is alike 0.28 grain per gallon.

Dr. Wallace also reports on the sewage-systems now in operation in a number of English towns. He makes the very judicious remark that wherever macadamised roads exist, in a drainage the quantity of sludge and the proportion of useless material in it are comparatively large. He finds, from observations in Leeds, that lime is incapable of removing the deep brown colour due to the waste waters of the tanneries. This colour we always found was very readily destroyed by sulphate of alumina, especially if containing a little free acid.

Concerning the quality of the sulphate of alumina used at Coventry, the author remarks that a material of the cheapest kind may be successfully employed. With this, we must, in the main, agree. The presence of salts of iron, within any moderate proportion, and of free sulphuric acid are quite unimportant. The question simply is, what does the soluble alumina cost per unit?

In describing his visit to the Aylesbury works, the author remarks that it would be impossible to obtain sufficient refuse carbon for treating the sewage of a large town. This is a difficulty which we admit cannot be overlooked. Dr. Wallace, however, seems to overrate the facilities for the disposal of sewage sludge which are to be found at Aylesbury. There are few districts where there is a smaller proportion of land under tillage.

With the results of irrigation as seen at Croydon Dr. Wallace was not fascinated. He found "the ground quite sodden, the vegetation by no means luxuriant, and the ground in some places swarming with a small species of fly."

The author is not an advocate of the costly system of double sewerage recommended by some authorities. At least he very rightly considers that soapsuds, the water in which vegetables have been boiled, &c., as objectionable and as unfit to be admitted into a river as the outflow from water-closets.

This little pamphlet may be recommended to municipal authorities, as being fairly and impartially written, and free from that spirit of partisanship by which the solution of the sewage question has been so much retarded.

Animals Injurious to the Sugar-canes in Brazil — E. Guignet.—An account of M. Capanoma's process for destroying the terrible Saüba ants. Some litres of carbon disulphide saturated with sulphur are poured into the galleries of these depredators, and the solution is fired by means of a fuse. The ants are killed by the explosion or by the fumes evolved.—*Moniteur Scientifique*.

CORRESPONDENCE.

VOLUMETRIC ESTIMATION OF MANGANESE.

To the Editor of the Chemical News.

SIR,—May I ask permission to point out that while Mr. Pattinson's three experiments (CHEMICAL NEWS, vol. xli., p. 180) sufficiently show that CaCl_2 is not equally effective with Fe_2Cl_6 in preventing MnO_2 carrying MnO down with it when the solution containing the Mn is treated by Mr. Pattinson's method,—or, rather, by Mr. Pattinson's modification of the method which I introduced more than ten years ago,—they prove nothing as to whether or not all the Mn is precipitated as MnO_2 when the solution containing the Mn is treated by that form of the method which I had introduced into nearly all chlorine works years before Mr. Pattinson's experiments with respect to the precipitation of Mn by solution of bleaching-powder were begun. In the three experiments referred to, Mr. Pattinson added, to a solution of MnCl_2 , calcium carbonate and bleaching-powder solution, "under the conditions," he says, "which I have found to give the best results." We can only suppose, therefore, that the precipitation was made at the temperature recommended in his paper in the *Journal of the Chemical Society* for June, 1879, namely, at about 160°F . By my method, however, no calcium carbonate is used, and the solution of MnCl_2 is boiled with the bleaching-powder solution, the latter being added gradually to a boiling solution of the former until a faint rose colouration, due to permanganate, appears.

Mr. Pattinson has not published any results stated to have been obtained by precipitating the Mn exactly in this way. He speaks of "two precipitations made when the solutions were boiling," but he gives no particulars with respect to them, except the fact that all the Mn present was not obtained as MnO_2 ; and for all that appears he may have used too great an excess of bleaching-powder solution, and so have brought into solution as permanganate more of his Mn than the mere trace which alone becomes permanganate when the instructions for the determination of manganese which I have given to nearly all works' chemists having to do with the Weldon process are exactly followed. Moreover, he probably employed a neutral manganese solution; and I long ago found that the manganese solution must be acid, or the result would be too low. The reason is that ordinary bleaching-powder solution contains an appreciable quantity of CaO , which precipitates MnO from a solution of MnCl_2 not containing any free acid, which MnO at once combines with MnO_2 to form Mn_3O_4 , the MnO in which compound cannot be completely peroxidised by boiling with solution of bleaching-powder.

Pressure of other work has as yet prevented any complete examination in my laboratory of the effect of the presence of ferric chloride with a solution of manganese treated by solution of bleaching-powder; but I cannot doubt that one effect of it is to hinder the precipitation of any Mn as MnO . I could not have doubted this, even in the absence of the experimental results of so accurate a worker as I well know Mr. Pattinson to be. All I do doubt in the matter is, whether the result got by Mr. Pattinson's method is more accurate than that got by boiling a slightly acidified dilute solution of MnCl_2 with enough bleaching-powder solution to produce a faint permanganate colouration. In the only comparative experiment I have yet been able to have made, my method gave $\text{MnO}_2 = 100$, and Mr. Pattinson's method gave $\text{MnO}_2 = 100.26$.

I hope soon to determine which result is the more accurate. In the meantime the question between Mr. Pattinson's method and my own is merely a question of 0.26 per cent.—I am, &c.,

WALTER WELDON.

Rede Hall, Burstow,
April 20, 1880.

CATALYTIC FORCE AND THE NASCENT STATE.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 176, I see that Dr. D. Tommasi has referred to my prize essay on Catalytic Force, and has quoted a passage from it. I have already quoted *other passages* from it in recent numbers of your journal, and therefore shall not discuss the matter any further. I thank Professor Tommasi for the complimentary terms in which he has thought proper to speak of this work of my youthful days, and regret that my views do not correspond with his own.

Allow me to inform you that a copy of my memoir exists in the library of the Royal Society, and another in that of the Chemical Society of London, where those who are interested in the question may consult the *original text* of my "Force Catalytique ou Études sur les Phénomènes de Contact," besides which many of our English Scientific Societies possess the complete collection of the Transactions of the Société Hollandaise.—I am, &c.,

T. L. PHIPSON.

Putney, April 24, 1880.

AMMONIA FROM ATMOSPHERIC NITROGEN.

To the Editor of the Chemical News.

SIR,—In this week's issue of the CHEMICAL NEWS (vol. xli., p. 195) there is a note, signed by J. H., respecting Rickman and Thompson's process for the production of ammonia (in the form of chloride) from atmospheric nitrogen and nascent hydrogen, wherein J. H. says that he has obtained a copy of the patent, filed March 15th, and he finds that it is only a "preliminary specification" (provisional ?), and that he in consequence has some difficulty in arriving at facts; but, judging from what he can ascertain, he is of opinion that the *bulk* of the ammonia they obtain comes from the nitrogen in the coal.

Now with regard to the provisional specification filed March 15th. It is clear that somebody must have made an error somewhere, because, if a provisional specification were filed March 15th, it would not yet be published for four months to come. Moreover, a provisional specification is never published apart from the complete specification, unless the patent is carried no further than the provisional stage; but this patent is carried further, and the complete specification can be obtained if in print, and if not can be inspected at the Patent Office.

Secondly, J. H. is of opinion that the *bulk* of the ammonia is produced from the nitrogen of the coal,—that is to say, practically, the whole. If that were so it would be folly to think of making it a direct manufacture. As a by-product, of course, it counts as supplementary profit; as a direct manufacture, nothing, the amount of ammonia from the nitrogen of a ton of coal being so small. As a matter of curiosity they always ascertain the amount of nitrogen in each batch of coal or coal-dust used, but the quantity is so inconsiderable that it has no practical effect. Of the different kinds of coal with which they have experimented they have found Hetton coal to contain the most, being 0.39 p. cent N—but it is a coking coal, therefore not so suitable as a non-coking coal, if even the contained nitrogen had been of any importance; Derby Brights contain 0.36 per cent, and Hard Steam 0.33 per cent N; but coal-dust, which they prefer to use and are now using, contains only 0.36 per cent N. This amount of nitrogen will produce but a very small amount of ammonia per ton of coal,—only about 7 lbs., equal to about 21 lbs. of chloride; but this is double the amount produced from the nitrogen of the coal in actual work in gas-factories. A ton of coal gives 10,000 cubic feet of gas, and each cubic foot of gas contains about 2.4 of ammonia, which is for the ton of coal 54½ ozs., or about 3½ lbs. Now it is not uncommon to produce 50 ozs. NH₄Cl from less than 20 lbs. of coal-dust, which is equal to about 350 lbs. to the ton; and this is more than 16 times that which the nitro-

gen in the coal would produce if it were all utilised, and 33 times that actually produced in gas-factories. So that it would appear that the *bulk* of the ammonia produced by Rickman and Thompson's process is really from the nitrogen of the atmosphere, and not from the nitrogen in the coal.

I begin to doubt whether any of the ammonia produced in gas-factories is from combined nitrogen in the coal, about which I may have something more to say.—I am, &c.,

J. B. T.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Justus Liebig's Annalen der Chemie,
Band 200, Heft 1 and 2.

Researches from the Chemical Institute of the University of Strasburg.—These consist of a paper by R. Fittig and H. Liepmann on fluoranthren, a new hydrocarbon in coal-tar; researches also by R. Fittig on the non-saturated acids, such as the mono-basic acids, with six atoms of carbon, the ethyl-crotonic, the hydrosorbic, and the pyroterebic, also on methyl-acrylic acid; on the decomposition of the substitution products of the lower fatty acids by water. The experiments in these various instances have been performed by students under Prof. Fittig's direction. These Strasburg researches further include a memoir on phenyl-lactimide, by Ed. Posen; a notice by H. Schmidt on the preparation of the neutral glycerin-ether acetate, and a paper by Fr. Jourdan on the synthesis of normal nonylic acid, and of an acid isomeric with the palmitic.

Various Nitriles from Hydrocyanic Acid and Ethyl-aldehyd-ammonia, according to the Experiments of Dr. S. C. Passavant.—E. Erlenmeyer.—An extensive memoir, not adapted for useful abstraction.

The Chemical Constitution of Organic Bodies with Reference to their Density and their Power of Transmitting Light.—Dr. J. W. Brühl.—The author expresses the law which he seeks to establish thus:—(1.) The atomic refraction of the univalent elements is independent of their position in a compound body. It is constant, no matter with what other atoms the former class are combined. (2.) The atomic refraction of carbon and of the polyvalent elements in general is variable, and depends on their mode of combination. If carbon binds a separate atom with each valence, its atomic refraction for that part of the spectrum independent of the wave-length=4.86; but in case of double catenation with other atoms of carbon it=5.86.

$$rAC' = 4.86.$$

$$rAC'' = 5.86.$$

(3.) The molecular refraction of organic bodies is determined by the kind and number of their atoms, and is affected also by their number. It can be calculated only when the chemical constitution is known, using the refraction peculiar to each atom according to its manner of combination. The molecular refraction is then the sum of the specific refractive power of the constituent atoms.

Studies on Quinic Acid, Quinon, and on Connected Bodies.—O. Hesse.—The author remarks that quinic acid forms in all cases anhydrous crystals. He examines its behaviour with acetic anhydride, bromine, concentrated hydrochloric acid, and soda. Quinon is readily soluble in boiling petroleum ether, and better still in boiling ligroin, from both of which it is chiefly deposited on cooling in fine yellow crystals. He considers this body as the aldehyd of quinic acid. The memoir contains also ob-

servations on hydroquinon, diacetyl-hydroquinon, dipropionyl-hydroquinon, quinhydron, phenoquinon, and methyl-quinhydron.

Alleged Non-existence of Pentathionic Acid.—F. Kessler.—A reply to W. Spring (*Annalen*, 199, 97). The author, in this preliminary communication, asserts that the acid which he obtained by the decomposition of sulphurous acid and sulphuretted hydrogen had the composition S_5O_5 , and was consequently a true pentathionic acid.

Postscript on the Nature of Pyroterebic Acid.—Jul. Bredt and R. Fittig.—The body hitherto regarded as pyroterebic acid, *i.e.*, the product of the dry distillation of terebic acid, is, in spite of its constant boiling-point, a mixture, and consists chiefly, not of an acid, but of lacton.

Moniteur Scientifique, Quesneville.
February, 1880.

Industrial Society of Mulhouse: Sessions of the Chemical Committee.—At the meeting, November 12, M. Prud'homme exhibited specimens of woollen cloth dyed with the "alizarin-carmine" of Przibram and Co. This colour is the potassium or sodium salt of sulpho-alizaric acid. It was first accidentally produced by the oxidation of anthraquinone-sulphurous acid. Alizarin-carmine is a deep violet powder, and dissolves readily in water with a magenta shade.

Review of Toxicology.—A. Vernon.—Extracts from the memoirs of M. F. Selmi, as originally published in the *Transactions* of the Lyceum Academy.

Narcotin, Cotarnin, and Hydrocotarnin.—G. H. Beckett and C. R. Alder Wright.—A lengthy translation from the English.

Volumetric Determination of Active Oxygen in Barium Binoxide and in Oxygenated Water.—Armand Bertrand.—Inserted at length.

Dimethyl-naphthyl-amin and Naphthoquinon.—P. Monnet, F. Reverdin, and E. Noelting.—The authors have not succeeded in obtaining colouring matters by the oxidation of dimethyl-naphthyl-amine. Naphthoquinon yields with aniline and dimethyl-aniline in presence of sulphuric acid fine red and violet colourations, and with phenol and resorcin compounds, which form with alkalies red and fluorescent solutions.

Action of Heat upon Metals in a Vacuum.—T. A. Edison.—A paper read at the Saratoga Meeting of the American Association for the Advancement of Science.

Falsifications of Alimentary Substances.—A summary of the English and German laws on the adulteration of food.

NOTES AND QUERIES.

Electro-deposition of Lead.—Can you tell me where I can find any information about the electro-deposition of lead? I can succeed in getting a thin coating upon copper, but cannot increase the thickness. It is very much wanted for machinery where sulphuric acid is used.—M.B.

Keeping Salts of Ammonia.—(Reply to H. J. Bland.)—I beg to state that muriate of ammonia kept in dry sheds does diminish in weight, and that to a considerable extent: the loss is increased when the muriate is stocked through a hot summer. Sulphate of ammonia loses weight also. This statement is the result of absolute experience, and is made with reference to a large quantity.—H. RAYNER.

Distillation of Hydrocarbons.—Will some chemist kindly account for the following?—In the distillation of coal in a small iron retort, I have found the results of illuminating hydrocarbons vary to the extent of 20 per cent with other iron retorts, the test being made with the same coal and under precisely similar conditions. I have gathered from this that the iron of the retorts must have varied in composition, and that a chemical combination must have occurred with one kind of iron more than another, the temperature of the iron being in all cases about 2000° F. If someone will kindly give me the benefit of their experience or judgment I shall esteem it a favour.—E. W.

ERRATUM.—P 191, col. 1, line 6, for "E. Rawson" read "C. Rawson."

MEETINGS FOR THE WEEK.

- MONDAY, May 3rd.—Medical, 8.30.
— Society of Arts, 8. Robert W. Edis, F.S.A., "The Decoration and Furniture of Town Houses."
— Royal Institution, 5. General Monthly Meeting.
- TUESDAY, 4th.—Civil Engineers, 8.
— Zoological, 8.30.
— Pathological, 8.30.
— Royal Institution, 3. Mr. R. H. Scott, "Wind and Weather."
- WEDNESDAY, 5th.—Society of Arts, 8. John C. Morton, "Agricultural Experiences: the Lesson of Forty Years."
- THURSDAY, 6th.—Royal Institution, 3. Prof. Tyndall, "Light as a Mode of Motion: Theories of Light and Colour."
— Chemical, 8. Dr. Hodgkinson, "On the Action of Sodium on Etherial Salts of Phenyl Acetic Acid." C. E. Groves, "Estimation of Nitrogen in Carbon Compounds." Mr. M. P. Muir, "On Sage Oil." Dr. W. Ramsey, "On the Electrical Conductivity of Solutions."
- FRIDAY, 7th.—Royal Institution, 8. Prof. Flower, "Fashion in Deformity."
— Geologists' Association, 8.
— Society of Arts, 8. Mr. W. Robertson, M.R.C.A., "The Present Condition and Prospects of Agriculture in South India."
- SATURDAY, 8th.—Royal Institution. Prof. Morley, "The Dramatists before Shakespeare," 3.
— Physical, 3.

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THE CHEMICAL NEWS.

Vol. XLI. No. 1067.

MEASUREMENT OF THE ACTINISM OF THE SUN'S RAYS AND OF DAYLIGHT.*

By Dr. R. ANGUS SMITH, F.R.S.

WHEN examining the air of towns and the effect of smoke and fogs, I have often wished for a very simple chemical method of measuring the total light absorbed by these gases, vapours, and floating solids. I do not undervalue the work of others, but I think I have obtained a process promising good results with great simplicity, although I dare say it introduces its own class of difficulties.

The fundamental fact is that when iodide of potassium in solution is treated with nitric acid, so small in quantity as to cause no change of colour in dull diffused light, a change takes place when the same mixture is brought into clear light; iodine is set free and the solution becomes yellow.

2. The amount of iodine freed can be titrated with great exactness by the use of hyposulphite, as is well known.

In these two facts lies the whole process, the first is the new part, the second makes the first quantitative, and its use is of course part of the novelty.

3. It is known that strong acid liberates iodine. Weak acid does so after a long time, but the process is hastened by light.

4. Heat even to the boiling point does not act so well as light (experiments being made in sealed tubes to prevent loss of iodine).

5. Heat assists the action of light.

6. A solution may be exposed day after day so as to give the accumulated effect of sunlight, in a measurable condition at the end of the time.

7. The solution of iodide of potassium as hitherto obtained is subject to change. An old solution, that is, one nearly a month old, was found more sensitive than a new one in all cases tried.

8. The result of No. 7 is, that a certain allowance may require to be made for this, in those cases where the periods of observation with one solution are long.

9. The amount of allowance to be made for temperature is not made out. It is not certain that any is required in the cases when weak acid is used. The weather has not allowed any combined action of great light and heat, but with heat and light in the rays from an electric light with a parabolic reflector, the action was very rapid.

10. Specimens of experiments (prospective at first). It was found convenient to use a solution of 2 grms. of iodide of potassium, afterwards changed to 1 grm., in 100 of water, and to use half of this for an experiment, *i.e.*, 50 c.c. of the solution which may be called A.

A nitric acid solution having an acidity equal to 1 per cent of sulphuric anhydride was made; this may be called B. Only very small portions of B were added to A.

The experiments were made in wide test-tubes, and here are early trials:—

13th February.

50 c.c. of A with 0.2 c.c. of B; no action in three hours.
50 c.c. " 0.4 " action; } not looked at
50 c.c. " 0.8 " decided action; } before the
end of 3 h.

No colour in a shaded part of the room in 420 hours in liquids of the same strength.

49.0 c.c. of A with 1.0 c.c. of B; colour after 40 mins.

48.8 " 1.2 " " 35 "

48.5 " 1.5 " " 30 "

(Sunshine and cloud alternately.)

48.5 c.c. of A with 1.5 c.c. of B; in the diffused light of the laboratory; no colour after 4 hours.

Here we have action in sunlight in proportion to the acid. The acid gives delicacy. The light commences the chemical action.

Examples in which the decomposition was measured by a solution of hyposulphite of sodium, which may be called solution C=0.1 grm. per litre of iodine (or as convenient). I shall extract experiments made with B solution 0.8 c.c., because it is an intermediate one (0.2, 0.4, 0.8, 1.6, and 3.2 have hitherto been the favourites).

1880.	B sol.	Hours.	Measure by C solution (hyposulphite).
Mar. 3.	Sunshine and cloud alternately.	0.8 After 2½	8.1, 1st colour in 20'
" 4.	Sunshine	0.8 ..	1st colour in 30'
" 5.	Dull all day	0.8 " 4	0.9
" 8.	Sunshine	0.8 " 2½	7.5, colour in 20'
" 9.	A little sunshine	0.8 " 2½	4.8
" 10.	Foggy, with a gleam of sunshine	0.8 " 6	1.5
" 11.	Bright	0.8 " 2½	7.2
" 12.	Dull and wet	0.8 " 3	0.6
" 13.	Dark and dull	0.8 " 2½	Faint trace
" 15.	Changeable	0.8 " 2½	1.8
" 16.	Changeable	0.8 " 2½	1.6
" 18.	Sun through haze	0.8 " 2½	5.8
" 19.	Bright	0.8 " 2½	11.5
" 20.	Fog till 11.30	0.8 " 2½	3.2
April 1.	Sun and showers	0.8 " 2½	1.6

Example of one day's observations showing the measured amounts after 2½ and 7 hours. C solution gives the proportion of light effect.

8th March. Sunny.

B sol.	C sol. required after 2½ hours.	C sol. required after 7 hours.
0.2 c.c.	1.25 c.c.	5.2 c.c.
0.4 "	4.1 "	11.1 "
0.8 "	5.8 "	17.5 "
1.6 "	9.8 "	27.0 "
3.2 "	12.8 "	33.3 "
6.4 "	17.8 "	34.8 "

The rate of increase of decomposition requires to be learned, also the most convenient solutions of B for every light and perhaps temperature.

Effect of temperature.	Tubes exposed to daylight during a dull day.
B sol.	C sol. required.
0.2	0.15
0.4	0.90 .. Temperature = 12.8° C.
0.8	1.00
1.0	3.20
1.6	6.00

These were in duplicate to begin with, but finding after an hour and a half that almost no action had taken place one-half were put into the dark. At the end of the day the result was:—

B solution.	C solution required.
0.2	0.0
0.4	0.0
0.8	0.1 .. Temperature = 11.1° C.
1.0	0.1
1.6	0.2

It is seen that with an almost equal temperature the action is very decided in light on a dull day, but scarcely measurable in darkness.

* A Paper read before the Royal Society, April 29, 1880.

To further the effect of heat the following were tried:—

Temp. 12° 8' C. in light.		Temp. 25° C. in darkness.	
B sol.	C sol. required.	B sol.	C sol. required.
0·4	0·4	0·4	0
0·8	1·3	0·8	0
1·0	1·9	1·0	0
1·6	5·2	1·6	?

It was suspected that not only did the iodide solution change by keeping, but the nitric acid also, weak as it was. Indeed I had once a mode projected of measuring light by the decomposition of nitric acids or nitrates, but I did not expect this to take place in such weak solutions.

To avoid this change, sulphuric acid was tried with the following result. This trial serves also as a test for the effect of light separate from heat.

2½ hours exposure to not very bright clouds:—

Temp. 12° C. in light.		Temp. 20° in dark.	
Sulphuric acid used, same acidity.	C sol. required.	Sulphuric acid.	C sol. required.
0·4	0·5	0·4	0
0·8	3·9	0·8	0
1·6	4·9	1·6	0
3·2	6·1	3·2	0

11. There seems, therefore, no reason to doubt that this is a true photometric process, with special capacities to be developed in time. I may add that I did obtain better results at the window of my house than at the laboratory at the same time, the latter being nearer the centre of the town; thus the process has done the duty it was intended for, although only once tried for this special purpose. I am looking to it as an agent specially for the examination of climate, but of course it may have many uses. This process does not aim at delicacy, but at accumulation of effect. I have not spoken of a standard; the results are only comparative, but the process may be made to supply its own standard.

12. Since writing the above, it appears that by using sulphuric acid some of the fears at first entertained may be avoided, as is shown by the following extract:—

B sol.	C sol. required after 2½ hours' exposure of A to light.	C sol. required after 50 hours' exposure of A to darkness.
0·2	7·6	0·3
0·5	15·1	0·6
1·0	23·4	0·6
2·0	30·4	0·7
4·0	43·6	0·7
6·0	53·8	1·3

The temperature of the solutions exposed to light = 13° C., kept in darkness = 22° C.

The strength of solutions and the kind of acid to be used may vary. Similar results may be got by using bromide of potassium, but it is less delicate.

REVISION OF THE ATOMIC WEIGHT AND VALENCE OF ALUMINIUM.*

By J. W. MALLET, F.R.S.

AFTER remarking that there is probably no one of the so-called chemical elements equally abundant in nature with aluminium, and occurring in as numerous compounds, with regard to the atomic weight of which our knowledge has long rested upon so slender a foundation of accurate experiment, the author gives a brief statement of prior determinations, noticing in succession the researches of Berzelius, Sir Humphry Davy, Thomson, Mather, himself, Dumas, Charles Tissier, and Terreil, and quotes the numbers given in several of the more recent chemical handbooks, reports, &c.

He then proceeds to describe a re-determination of this atomic weight, which occupied a large part of his leisure time during the last three years. In this investigation he spared no pains to attain as precise a result as possible, and aimed especially at the discovery, and as far as possible removal, of sources of error connected with the methods employed. The following general principles were kept in view:—

1. That each process should be as simple as possible, and should involve as little as possible of known liability to error.
2. That different and independent processes should be resorted to as the means of checking each other's results, even though it may fairly be assumed that one is more advantageous than another.
3. That each process should be carried out with quantities of material differing considerably from each other in successive experiments.
4. That only such other atomic weights should be involved as may be counted among those already known with the nearest approach to accuracy.

The most scrupulous care was taken in the purification and examination of all the reagents used, and as far as possible, vessels of platinum or of hard porcelain were substituted for those of glass.

In the author's first series of experiments the alumina was determined which resulted from the ignition of a known weight of ammonium alum.

In the second series a solution was made of a known weight of bromide of aluminium, and was very exactly precipitated by a solution of nitrate of silver, and the weight of bromide of aluminium compared with that of the silver required for forming the solution of nitrate.

In the third series bromide of aluminium was used for the preparation of pure aluminium, by reduction by sodium; a carefully weighed portion of the pure aluminium was dissolved in a solution of caustic soda, and the hydrogen given off was estimated by volume or else burnt, and the weight of resulting water determined.

In the following epitome of the results A, B, C denote groups of experiments under each series, the experiments of each group differing only in the quantity of material operated on, while the different groups differ in the particular preparation used, or in some feature of the method. Thus in Series I. A was made with alum dried by exposure to air for two hours; B with alum dried by exposure for twenty-four hours. In Series II. A, B, C were made with successive portions of a distillate of bromide of aluminium. In Series III. A was made by estimating the hydrogen by volume, B by estimating by weight the water resulting from its combustion. Only the number of experiments in each group, the mean result from the group, and the probable error of the mean, are here given.

SERIES I.

	A.	B.
No. . . .	5	5
Mean . . .	27·040	27·096
P. e. . . .	±0·0073	±0·0054

SERIES II.

	A.	B.	C.
No. . . .	3	5	3
Mean . . .	27·034	27·023	27·018
P. e. . . .	±0·0049	±0·0052	±0·0069

SERIES III.

	A.	B.
No. . . .	6	3
Mean . . .	27·005	26·990
P. e. . . .	±0·0033	±0·0046

In view of the gradual loss of water which crystallised ammonium alum has been shown to undergo on exposure to the air, the author considers that of these various sets of experiments Series I., B, is entitled to least confidence

* Abstract of a Paper read before the Royal Society, April 22, 1880.

He is inclined to attach most weight to Series III., A, since the method used was very simple in principle, the determination of one of the two quantities concerned was rendered very exact by the great volume occupied by the hydrogen, the comparison was made directly with the standard element in our system of atomic weights, and not through the intervention of any other substance whose atomic weight must be assumed, and the agreement of the results among themselves is particularly good.

The general mean from all the thirty experiments, if all be included is $Al = 27.032$, with a probable error for this mean of ± 0.0045 . If Series I., B, be excluded, the mean of all the remaining twenty-five experiments is $Al = 27.019$, with a probable error of ± 0.0030 .

The general result adds, the author hopes, aluminium to the unfortunately still limited list of those elementary substances whose atomic weights have been determined within the limits of precision attainable with our present means of experiment.

This result also adds one to the cases already on record of the numbers representing carefully determined atomic weights approaching closely to integers, and leads the author to say a word on the re-consideration of "Prout's Law." Taking the following eighteen elements as the only ones of which the atomic weights may be fairly considered as determined, with reference to hydrogen, with the greatest attainable precision, or a near approach thereto, namely oxygen, nitrogen, chlorine, bromine, iodine, sulphur, potassium, sodium, lithium, silver, thallium, aluminium, carbon, phosphorus, barium, calcium, magnesium, and lead, and making a reasonable allowance for the errors of the determinations, he calculates the probability that nine of those numbers should lie, as they are found to do, within 0.1 of integers, supposing the value of the true numbers to be determined by chance, and finds it only as 1 to 235.2. The exact figure for the chance will of course depend upon the limit of error taken; but the above example seems sufficient to show that not only is Prout's law not as yet absolutely overturned, but that a heavy and apparently increasing weight of probability in its favour, or in favour of some modification of it, exists, and demands consideration.

THE APPLICATION OF THE ELECTRIC CURRENT IN ANALYTICAL CHEMISTRY.

By C. LUCKOW.

THE electric current is capable of varied application both in quantitative and qualitative analysis. It can effect the solution of metals and alloys at the + pole, and the precipitation of certain metals from acid solutions, and of others from alkaline solutions, or from neutral solutions mixed with acetate of soda. This may take place either at the - pole in the metallic state or at the + pole as peroxide. It also serves for separating metals precipitable out of an alkaline solution from such as cannot be thrown down from such solutions. Thus, copper can be precipitated as a metal and lead as peroxide, being simultaneously separated both from each other and from all the metals of the first four groups, except manganese. The author recommends Meidinger's battery, as giving a constant current for two or three months, and as constantly available, and points out many advantages of the electric separation of metals as compared with ordinary analytical processes. A Meidinger battery of four elements costs about £1, and consumes yearly 25 lbs. sulphate of copper. In acid solutions the electric current has a reducing action, but in alkaline solutions it is an oxidiser. If a current is passed through solutions of the compounds of chlorine, bromine, iodine, cyanogen, ferro- and ferri-cyanogen with hydrogen, the above-named electro-negative constituents are separated out at the + pole and the hydrogen at the - pole.

Cyanogen undergoes a further decomposition, the final products being carbonic acid and nitrogen. The solutions of ferro- and ferri-cyanogen deposit Prussian blue at the + pole.

In dilute solutions of metallic chlorides there is formed merely hypochlorous acid, accompanied in stronger solutions by chlorine.

In the solutions of the chlorides of the alkaline and earthy alkaline metals a chlorate is formed as soon as the solutions have become alkaline, in consequence of the escape of the hypochlorous acid and of the chlorine. If dilute solutions of chlorides contain little free hydrochloric acid hypochlorous acid alone is formed, and the liquid in course of time becomes alkaline.

From the solutions of the iodides and bromides iodine and bromine are deposited at the + pole, and in case of the metals of the two first groups, especially in concentrated solutions, there are formed iodates and bromates.

Potassic cyanide is decomposed by the current into potassic and ammoniac carbonates.

If the solutions of the chlorides, iodides, and bromides contain free alkali, the chlorates, iodates, and bromates alone are formed. From the insoluble compounds of chlorine, iodine, bromine, cyanogen, ferro- and ferri-cyanogen with the metals in presence of dilute sulphuric or nitric acid, the metal is eliminated at the - and the halogen at the + pole. Hence, for this purpose the - pole is a platinum wire, and the + pole a platinum capsule, in which the metallic compound before the addition of the diluted acid is moistened with a little water, which is evaporated to prevent the adhesion of the metallic compound to the bottom of the capsule.

Concentrated nitric acid is decomposed with the formation of nitrous acid at the - pole. In acid of 1.2 sp. gr. this decomposition does not ensue, at least with a weak current.

Dilute nitric acid between two platinum poles passes into ammonia neither when alone nor in the presence of sulphuric acid. But if dilute nitric acid is mixed with a solution of copper sulphate, and submitted to electrolysis, metallic copper is deposited and ammoniac sulphate is formed, all the nitric acid present being thus transformed if there is sufficient copper salt. Ammonia is also formed in the electrolysis of nitrates, except a free alkali is present.

Concentrated sulphuric acid is decomposed with separation of sulphur, but the dilute acid is not, nor is the acid decomposed during the electrolysis of the aqueous solutions of its salts.

Sulphurous acid in aqueous solution is resolved into sulphur and sulphuretted hydrogen; in its salts it is gradually converted into sulphuric acid. The hyposulphites gradually pass into the corresponding sulphates, with separation of sulphur.

The alkaline sulphides are decomposed with or without liberation of sulphur according to the proportion which they contain, and a sulphate is simultaneously formed.

In the solutions of the alkaline sulphites and hyposulphites there is a transitory formation of polythionates along with the production of sulphides.

Phosphoric acid is not decomposed by the current, whether in dilute solution or in the dilute solutions of its salts.

Carbonic acid is very incompletely liberated at the + pole from the solutions of the bicarbonates.

Silicic acid is separated from concentrated solutions as a white mass, and boric acid in dendritic crystals, both at the + pole.

In order to separate the above-mentioned acids from their insoluble compounds with the metals of groups 4, 5, and 6, the insoluble salt is dissolved in sulphuric or nitric acid, in an alkali, or in some suitable salt, or we proceed as laid down for the insoluble chlorides.

We now turn to the behaviour of the electro-positive constituents under the action of the current.

The metals of group 6 are all separated from their solutions in the reguline state.

(1.) From the solutions of the chlorides, also, in presence of free hydrochloric acid, which, if abundant, is neutralised with ammonia, there are separated:—

(a.) Antimony from the trichloride or the chlorine compound corresponding to antimony oxide as a dark brown or light grey metallic deposit according to the concentration of the solution. The precipitate is hardly soluble in hydrochloric acid, readily in nitric acid, especially if previously moistened with hydrochloric acid. Antimony is easily and completely thrown down in a reguline state from the solution of emetic tartar.

(b.) Arsenic from the trichloride, as dark brown or black graphitic deposit, according to concentration, insoluble in hot hydrochloric acid, readily soluble in concentrated nitric acid and in a solution of sodic hypochlorite.

(c.) Tin from the stannous and stannic chlorides as a white deposit with a dull metallic lustre, easily soluble in dilute acids, especially in contact with platinum. In the electrolysis of arsenic and antimony chlorides some arsenic and antimony hydride escapes at the - pole. If all these three metals are present simultaneously, arsenic is deposited first, then antimony, and lastly tin.

(d.) Platinum from the dilute solutions of the chloride, to which some solution of sodium chloride may be advantageously added. The deposit is at first reguline; afterwards, when the solution has become very dilute, it takes the form of platinum-black. The deposit from the insoluble double chlorides takes the latter form.

(e.) Gold, like platinum, is easily deposited from its soluble and insoluble compounds. From solutions in potassic cyanide it is deposited in a reguline form. Platinum vessels in which the precipitation of gold or platinum is conducted should be previously coated internally with a thin stratum of copper or silver.

(2.) From the solutions of the sulphides in alkaline sulphides:—

If care be taken that an excess of alkaline sulphide remains in solution the precipitation of arsenic and tin in the metallic form is complete; that of arsenic incomplete.

(3.) From alkaline solutions:—

Stannic and antimonie acids dissolve readily in concentrated soda or potassa-lye from which metallic deposits are obtained. The precipitation is very imperfect, and is only rendered complete by the introduction of sulphuretted hydrogen into the alkaline solution, or its acidulation with hydrochloric acid.

Of the metals of group 5 the current throws down:—

(a.) Copper completely from solutions containing free sulphuric, nitric, or acetic acid, provided the weight of such free acid, calculated as anhydride, does not exceed 8 per cent of the weight of the solution. If the solutions contain some free hydrochloric acid all the copper is thrown down if sal-ammoniac, common salt, or sodic acetate be added. If ammonia, ammoniac carbonate, or potassic cyanide is added to the solutions of neutral copper salts, all the copper is thrown down as a metal.

(b.) Silver, from solutions containing not above 8 to 10 per cent of free nitric acid, is deposited in a very bulky metallic state. Some peroxide is also deposited at the + pole, the formation of which is prevented by the addition of a little glycerin, milk-sugar, or tartaric acid. From the solutions of silver-salts in ammonia or ammoniac carbonate, the metal is also thrown down in a very bulky form, peroxide being separated out at the + pole, but it is soon reduced to metallic silver. If potassic cyanide is added to neutral or ammoniacal solutions of silver, or if silver-salts, insoluble in water, are dissolved in potassic cyanide, the silver is deposited from such solutions in the reguline form with a dull metallic lustre.

(c.) Mercury is completely deposited in the form of drops from mercurous and mercuric solutions. The metal is also readily separated from its insoluble compounds. If other metals are also present amalgams are formed.

(d.) Lead, from neutral solutions, is thrown down partly at the - pole as metal, and partly as peroxide at the + pole. A complete metallic precipitation is effected

only in presence of easily oxidisable bodies, which hinder the formation of peroxide. From the alkaline solutions of lead, the current precipitates the metal alone in a bulky condition, a slight deposit of peroxide at the + pole disappearing in the sequel. The complete separation of lead as peroxide is effected in a pure solution of lead only in the presence of more than 10 per cent of pure nitric acid. If the solution contains copper also, then even in presence of small proportions of nitric acid all the lead is deposited at the + pole, even though small quantities only of copper were present in solution. Other metals, such as silver and mercury, behave in a similar manner, but include part of the lead in their metallic precipitates.

(e.) Bismuth is thrown down in the reguline form from solutions containing some free nitric acid, a little peroxide being deposited at the positive pole. The metal is easily separated as a black mass from the insoluble compounds of bismuth.

(f.) Cadmium is completely deposited from the dilute neutral solutions of its sulphate, nitrate, and acetate in the metallic state with a zinc-grey colour. The quantity of the acid liberated in the solution of the sulphate may rise to $1\frac{1}{2}$ to 2 per cent before the precipitation of the cadmium is hindered. It is also completely deposited from solutions which have been mixed with an excess of ammonia, or ammoniac acetate.

If all the above-named metals of group 5 are present together in a solution containing free nitric acid, mercury and silver are thrown down first, and copper and bismuth not until the greater proportion of the two former metals has been precipitated.

Of the metals of group 4, zinc, nickel, and cobalt are imperfectly precipitated from the solutions of their neutral sulphates, but manganese and uranium are not thrown down at all in a metallic form. If, however, such solutions are mixed with a solution of an alkaline acetate, tartrate, or citrate, zinc, nickel, and cobalt are completely precipitated, and uranium to a small extent. The metallic zinc deposited from such solutions has a grey colour, and usually a metallic lustre, and is readily soluble in acids and alkalis. Nickel is deposited of a yellowish grey, and cobalt of a reddish grey colour and metallic lustre, and both dissolve with difficulty in cold dilute sulphuric and nitric acids. The original reddish colour of the cobalt solution turns to a dark brown soon after the current begins to operate; a part of the cobaltous oxide is transiently converted into cobaltic oxide. From solutions containing all the three metals the zinc is deposited first.

During the electrolysis of solutions of neutral sulphates of the above metals hydrates are soon formed, in consequence of the conversion of the nitric acid into ammonia, whence such solutions should be previously acidified with acetic acid.

On the electrolysis of the ammoniacal solutions of the above metals, and of the solutions of their cyanides in potassic cyanide, all three metals are completely deposited.

Zinc may also be completely precipitated in a reguline state from the potassic solutions, to which a little potassic cyanide may be advantageously added.

On dissolving in acids the zinc which has been deposited upon platinum there generally remains upon the latter a dark grey coating, rough to the touch, and not soluble even in concentrated acids. On ignition it shows a play of colours which, on subsequent treatment of acids, disappear, whilst some zinc is dissolved. Sometimes it is not practicable to remove the whole of this deposit except by the action of melting acid potassic sulphate. This coating is formed very slightly in potassic solutions, and not at all in the nitrate solutions of zinc.

Manganese is thrown down from its solutions, whether neutral or containing free acid, only as hydrated peroxide, and not at all as a metal. If it is desired that the manganese deposit should adhere firmly to the positive surface the proportion of free acid must be small. In very dilute manganese solutions, containing much nitric acid or a

mixture of nitric and sulphuric acid, permanganic acid is formed, and gives the solution a characteristic red colour.

Uranium is deposited even from perfectly neutral solutions of its oxide only in small quantities and as a yellow-grey metallic precipitate, which dissolves in hydrochloric acid with liberation of hydrogen. In acid solutions uranic oxide is converted into uranous oxide.

Iron is completely deposited in the metallic state from the neutral solutions of ferrous salts. The ferrous oxide is partly converted into ferric oxide in consequence of the oxidising action of the current. If to a neutral solution of ferrous sulphate there is added a solution of ammoniac citrate, and if care is taken that a little free citric acid always remains in the solution, the iron is completely thrown down in a shining reguline form, even if a part of it was originally present in the form of a ferric salt. Iron thus deposited resembles bright platinum, but to preserve its lustre it must be washed first in water, then in alcohol, and dried quickly.

From potassic ferrocyanide no metallic iron is precipitated, but Prussian blue at the — pole.

From the solution of ferrous oxide in sodic hyposulphite all the iron is deposited, chiefly as sulphide.

Metallic iron of a blue-grey colour is deposited from the solution of ferrous fluoride in sodic fluoride.

The solutions of the metals of the three first groups present little that is characteristic on the transit of an electric current. If the two poles are arranged separately in the limbs of a U-tube filled with such solutions the hydrates of the metallic oxides appear at the — pole; chromic oxide and aluminic hydrate in a voluminous form; calcic and magnesian hydrates as white incrustations; baric, strontic, potassic, and sodic hydrates in a soluble form.

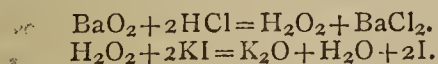
In the solutions of baric and strontic salts there appear white turbidities on the surface, in consequence of the formation of carbonates. — *Zeitschrift für Analytische Chemie*, Vol. xix., Part i., p. 1.

DETERMINATION OF ACTIVE OXYGEN IN BARIUM BINOXIDE AND IN OXYGENATED WATER.

By M. ARMAND BERTRAND.

FOR some years the peroxide of barium has become an industrial product, as the dyers employ it in large quantities for bleaching wild silks (as Tussah silk, &c.), and, as well as oxygenated water, it serves for decolourising human hair.

The peroxide is sold in the form of a grey powder, and readily admits of sophistication. The value of a sample is proportionate to the quantity of active oxygen which it contains. This method of analysis is based upon the following reactions:—



Hence, 1 grm. iodine corresponds to 43.8355 c.c. of active oxygen at 0° and at the pressure of 760 m.m. The iodine is determined with hyposulphite, without having recourse to starch liquid as indicator.

The standard solutions are:—Iodide of potassium made by dissolving 200 grms. of the pure salt in a quantity of water sufficient to make up a litre. No iodate should be present or the results will be rendered too high. The hyposulphite solution is prepared by dissolving 60 grms. of the pure salt in water and making up to a litre. It is standardised with pure iodine.

Into a large test-glass provided with a foot there are gradually poured 4 to 5 c.c. of pure hydrochloric acid, not containing a trace of free chlorine, which is diluted with water (about 100 to 150 c.c.). Half a gramme of the peroxide of barium, weighed very exactly, is allowed to dissolve in the acid, and 10 c.c. of the solution of potassic

iodide is added. Iodine is at once set at liberty. After waiting four or five minutes for the reaction to be complete the hydrochloric acid is saturated with an excess of pure bicarbonate of soda, and the standard solution of hyposulphite is added until the liquid turns from a yellow to a perfect white. This final reaction is very well-defined, so that if the determination is repeated ten or fifteen times on the same sample we obtain constantly the same number of c.c. of hyposulphite, to about 1-10 c.c. Having read off on the burette the number of c.c. of hyposulphite which have been employed, the corresponding weight of iodine is calculated, and thus there is found the value of the sample in free oxygen.

To determine oxygenated water a known volume is taken (a few c.c.), mixed with a dilute solution of hydrochloric acid, and the process is conducted as above directed. — *Moniteur Scientifique*.

ON THE QUANTITATIVE DETERMINATION OF OXYGEN DISSOLVED IN WATER.

By J. KÖNIG.

IN No. 14 of the *Berichte* for last year, F. Tiemann and C. Preusse communicated comparative examinations on the methods of determining the free oxygen in water according to the methods of Bunsen, Mohr, and Schützenberger-Risler, and arrived at results opposed to those which the author obtained in concert with L. Mutschler (*Berichte*, x., 2017). They conclude that next to Bunsen's method that of Schützenberger is to be preferred, and that Mohr's method gives too low results, whilst we formerly came almost to the opposite result.

Having been for some years engaged with extensive experiments on the changes undergone by irrigation-water when repeatedly used, I required an exact but speedy method for the determination of the dissolved oxygen, which I felt justified in assuming could not be inoperative in the irrigation of meadows. I was, therefore, along with C. Krauch, engaged with a repetition of the former experiments when the memoir of Tiemann and Preusse came into our hands. Our new results will be given in full in the *Zeitschrift für Analytische Chemie*.

In our former researches we set out with the assumption that the determination of oxygen by Bunsen's method gave absolutely accurate results. For the expulsion and collection of the oxygen dissolved in water we used Reichardt's apparatus. This, however, as we are now convinced, involves an error, and yields too much air and consequently oxygen.

As we then obtained by the Bunsen, Reichardt, and the Mohr process results closely agreeing, and as we were not aware of the source of error above mentioned, the circumstance that Mohr's process yields too much oxygen escaped us. Hence, we must now admit that this method yields uniformly too much oxygen. The result obtained by Tiemann and Preusse we have never been able to confirm in many hundred experiments. The only distinction between our manner of operating and theirs is that we used ammonia instead of soda-lye. As little have we been able to confirm the result of the same chemists that the Schützenberger process is absolutely accurate. On closely following out the method as described by Tiemann and Preusse we find that the results thus obtained fall too low by from $\frac{1}{3}$ to $\frac{1}{4}$.

After numerous determinations we find that illuminated water (*i.e.*, such as has been exposed to day- or sun-light, like rain-water, or even distilled water which has been aerated by shaking), always yields more oxygen by Mohr's process, as compared with well-water which has been for a length of time withdrawn from the action of light. This may possibly be due to the presence in the former

of oxygenated water (hydrogen peroxide), which has an oxidising effect upon ferrous oxide, but does not act upon sodic hydrosulphite. We must also remark that distilled water, repeatedly shaken up and saturated with air, gives different and rather higher figures for air and oxygen than are admissible according to the coefficients of absorption as calculated by Bunsen. It would therefore seem that the latter cannot be regarded as normal under all circumstances. Otherwise both the Mohr and the Schützenberger process yields results which, though not absolutely, are relatively accurate and respectively comparable, those of the former method being too high and those of the latter too low.

Mohr's process has the advantages that its standard solutions are more permanent, whilst the standard of the hydrosulphite varies from day to day. Mohr's method is therefore recommended where single determinations are required periodically. Schützenberger's is preferred if a large number of samples are to be examined in rapid succession. In the latter case the solution should be standardised not by means of solutions of copper oxide, but with water which is saturated with air by repeated shaking.—*Berichte der Deutsch. Chem. Gesellschaft.*

ON THE PRESERVATION OF SOLUTIONS OF PALMELLINE.

By Dr. T. L. PHIPSON, F.C.S., &c.

THE solution of Palmelline obtained by allowing cold water to stand for a day or two over the air-dried plant (*Palmella cruenta*), as described in my account of this substance last autumn, like all solutions of albuminoid substances, is very subject to decomposition, and at temperatures of 75° to 80° F., putrefaction sets in rapidly. The beautiful rose and yellow dichroic tint of the solution becomes paler and finally disappears, whilst the liquid takes a strong ammoniacal odour and swarms with *bacterium*, *vibrio*, and *spirillum*. The latter are not easily to be distinguished (except by their small size and that their motion is more rapid) from the spirillum which is present in the blood in cases of relapsing fever, during the pyrexia only, disappearing as the temperature of the body falls.

I have tried various methods of preserving the liquid in question without altering its composition and optical properties. Exclusion from air and light were only partially successful for short periods. The addition of a little salicylic acid modifies the delicate purple-rose tint and destroys the dichroism, so that the orange-yellow is no longer seen by reflection; moreover, it only preserves the liquid for a week or two, after which the phenomena alluded to set in as above described. Finally, I found that ether, which has no solvent action on Palmelline and does not affect its composition nor coagulate it, may be used with success to preserve the liquid for an indefinite period. It is sufficient to add a small quantity of ether to the solution in a tube, cork it, and turn it over once or twice so as to dissolve as much ether as possible in the liquid, to preserve it with all its properties for several months. As long as the contents of the tube have a strong odour of ether no decomposition sets in, and the optical properties of the palmelline remain intact.

This simple method of preservation may be found applicable to many other organic substances upon which ether exerts no chemical action.

Putney, April 24, 1880.

NOTICES OF BOOKS.

The Tar-Colour Works of Messrs. Meister, Lucius, and Brüning, at Höchst on the Main, in a Sanitary and Social Point of View. (Die Theerfarben-Fabriken der Herren, Meister, Lucius, und Brüning, zu Höchst am Main, in Sanitärer und Socialer Beziehung.) By Dr. GRANDHOMME. Berlin: L. Schuhmacher. (Reprinted from *Eulenberg's Vierteljahrschrift für Gericht Medicin und Effentl. Sanitäts wesen*, N. S., xxxii.)

WE have here a most valuable contribution to sanitary science. The manufacture of coal-tar colours has brought into use on the large scale a number of chemical compounds whose physiological action has been very imperfectly known. In consequence the most contradictory and highly-coloured statements have been circulated in political and literary organs. Many of the new colours have been denounced as exceedingly dangerous, not merely to the makers and to the dyers and printers who apply them to the textile fabrics, but even to the wearers of such tissues. There has been no due distinction observed between the properties of the raw materials and those of the finished products, and dyes have been condemned as poisonous in virtue of impurities resulting from imperfect and now obsolete methods of manufacture.

Under these circumstances all persons connected with the tinctorial trades may congratulate themselves on the appearance of the present work. The far-famed establishment of Messrs. Meister, Lucius, and Brüning offers opportunities for the study of the question not to be met with elsewhere. It employs over 1000 workmen, besides 40 foremen and managers, 25 chemists, 1 engineer, and 30 clerks. Everything is conducted in the most systematic manner. Each class of colours is prepared in separate work-rooms: thus we read of the blue house, the violet house, the eosin- and the naphthol houses, &c. A stringent rule provides that no workman may enter any part of the establishment save his own department. A very close watch is kept upon the health of all persons employed, and—a very important feature—the use of arsenic in the manufacture of magenta has been abandoned in favour of Coupier's process. There is consequently no danger that the physiological action of magenta and its derivatives may be complicated by arsenical impurities. Every precaution is taken to obviate dangers; the arrangements for ventilation are very complete. Any apparatus giving off noxious gases is placed in connection with the chimney. Rooms containing combustible and explosive materials are illuminated solely by the electric light on the principle of Siemens and Halske.

Among the raw materials benzol, naphthalin, and anthracen have been carefully examined as regards their action upon human health. Benzol is admittedly poisonous. It has repeatedly proved fatal when experimentally administered to animals. If the vapour is inhaled by man it produces irritation of the nervous centres, dizziness, ringing in the ears, nausea, and drowsiness. Only one serious case is recorded in medical literature, which, however, terminated in recovery.

Naphthalin is not to be feared, except in the state of very hot vapour. If taken internally it irritates the stomach, but none of the experiments made upon animals have had a fatal result.

Anthracen has an irritating action upon the mucous membranes, but it has not proved injurious to the workmen.

Nitrobenzol has been made the subject of extended experiments, as well as of clinical observations. The symptoms produced are difficulty of breathing, dizziness, drowsiness, tonic and clonic convulsions; but death often occurs from general paralysis. These morbid phenomena sometimes do not commence until after twenty-four hours have elapsed. Medical literature records forty-four cases of acute poisoning in human subjects, fourteen of which

Physical Society.—Sir William Thomson will preside at the meeting on Saturday afternoon, the 8th, and will make some brief communications.

proved fatal. The possibility of a chronic or cumulative action is not decided. No case of sickness from nitrobenzol has occurred at Höchst, though of the eight men in this department one has been employed for six years, one for eight, two for nine, and one for ten years.

The poisonous nature of aniline, including both the pure base and the so-called aniline-oil of commerce, is indisputable. Many recorded clinical observations are untrustworthy, as it does not appear whether the poison was raw aniline or some of its derivatives contaminated with arsenical or mercurial compounds. Three cases of "anilism" have been observed among the thirteen workmen employed in this branch.

Contrary to a very general opinion, rosaniline and its salts prepared without arsenic have been found harmless. The urine of the men employed in the magenta house has been specially tested for albumen, but with a negative result. Eczema and erythema do not occur among the workmen. Dr. Grandhomme considers that the irritation of the skin said to have frequently arisen from the use of stockings, &c., dyed with magenta is doubtless a consequence of the presence of arsenic. The derivatives of rosaniline, aniline-blue, violet, green, &c., are also harmless.

Eosin and erythrosin produced no injurious symptoms when taken by rabbits. The workmen in the eosin house, however, did not enjoy good health. Of seventeen persons employed in this department sixteen are annually unwell, chiefly from *hyperidrosis localis*.

The naphthol colours are not to be regarded as dangerous, the cases of illness in this department being chiefly due to slight wounds.

In the alizarin house the most prevalent diseases were affections of the skin, concerning the origin of which the author is not yet satisfied.

In the entire establishment the cases of illness which could in any way be traced to the nature of the employment formed only 3 per cent of the total cases. Hence it may well be questioned if the manufacture of coal-tar colours can be regarded as a specially dangerous employment.

This work is strongly to be recommended to the notice of medical practitioners, especially in the manufacturing districts. If writers and lecturers who wish to organise a crusade against poisonous colours will consult Dr. Grandhomme it will enable them to add knowledge to their zeal.

Agenda du Chimiste. Paris: Hachette et Cie.

THIS work, which appears with a recommendatory preface from no less eminent an authority than Prof. Würtz, seems to have taken its origin from a manuscript collection of notes and memoranda which had been gradually formed in his laboratory. Being continually consulted by his pupils and assistants, the thought occurred to the latter, some of whom have already made themselves very honourably known, that it might be found no less useful to chemists in other laboratories,—in fact, to the profession in general. The result of this happy idea was the publication of the present *multum in parvo*, in the form of an annual pocket-book. It would be difficult indeed to condense a larger amount of information, especially useful to the chemist, into so small a compass.

The first chapter is devoted to "Physical and Mathematical Documents," comprising the conversion of old or foreign measures and weights into the modern French standards; tables of circumferences, circles, squares, cubes, square roots, and cube roots, from 1 to 100; comparison of different thermometric scales; reduction of the indications of the mercurial thermometer to the temperatures shown by an air thermometer, and correction of thermometers. We have next a table of the coefficients of the linear expansion of certain solids, from 0° to 100°; of the cubic expansion of mercury and of glass; and of the expansion of certain liquids. Upon this follow sections on barometry, on vapour tension, the liquefaction of gases, and their vapour tension in that state. Next we

come to specific gravity, where are comparisons of the different hydrometric scales, tables of the specific gravities of inorganic compounds and minerals, of gases and vapours, and of solutions. We have next a series of tables not generally to be met with in manuals, such as the specific gravities of mixtures of alcohol and ether, of methylic alcohol and water, of bromine water, of solutions of hydrobromic and hydriodic acids; of fluosilicic, iodic, periodic, phosphoric, and arsenic acids; of the formic, tannic, oxalic, tartaric, and citric acids; and of series of saline solutions which we have not even room to particularise.

Under the head of Heat we find not merely refrigerating mixtures, but tables of the calories liberated in the mutual reactions of the non-metallic bodies; formation heats of salts, and other thermo-chemical data.

Under Light we find tables of indices of refraction, and of the rotatory powers of solids and liquids.

The second chapter, which is devoted to pure chemistry, deals with qualitative analysis in the moist way; by means of the blowpipe; spectral analysis; quantitative analysis, with table of factors for the calculation of results; tables of the formulæ of the most important compounds, with their solubilities in cold and boiling water and in concentrated alcohol.

The third chapter relates to industrial and applied chemistry, and is no less rich in well-selected facts, figures, and condensed instructions than are the two preceding.

The principal deficiency of the book is the want of a good index, a feature especially needful in a work of reference, and which the brief table of contents can by no means supply.

In the instructions for the analysis of superphosphates the reader is directed to determine the *sulphuric* acid in the aqueous solution by means of uranium. This is doubtless a clerical or typographical error for the *phosphoric* acid. In the preparation of "magnesia-mixture" we would venture to recommend magnesium chloride as preferable to the corresponding sulphate.

In the table of the percentages of tannin in different astringent materials we should suggest a re-consideration of the very low figure—12 per cent—ascribed to divi-divi. Its comparative action in dyeing blacks along with a salt of iron would place it above 30 per cent.

We may best sum up our opinion of this little Manual by declaring that no chemist ought to be without it, especially as a very slight knowledge of the French language will enable the reader to make use of its contents. The student, the commercial analyst, and the investigator engaged in purely scientific researches will all find it a valuable guide and a great saving of time and trouble, and all of them have good reason to be grateful to the compilers, and especially to M. Salet, upon whom the editorial duty has, we believe, exclusively rested.

Ceylon Coffee Soils and Manures. A Report to the Ceylon Coffee Planters' Association. By JOHN HUGHES, F.C.S. London: printed by Straker Bros. and Co.

THE planters of Ceylon have to contend with difficulties not few. The *Hemileia vastatrix* is not unworthy to rank with the *Phylloxera* and the *Pteronospora infestans*; white bugs and Termites commit depredations on the coffee-trees; the estates, lying often on steep declivities, are robbed of the richest portion of their soil by the heavy tropical rains, and judicious manuring of land regularly under crop has been the exception rather than the rule. It is therefore not surprising that the Coffee Planters' Association should become convinced of the necessity of obtaining scientific advice, and should engage a competent agricultural chemist to examine and report upon their soils and the means for their improvement.

Mr. Hughes appears to have inspected the coffee-growing districts, noticed their deficiencies and their capabilities, and examined the articles chiefly used as manure. As a matter of course, in a country where roads are bad and

transport necessarily costly, only the most concentrated fertilisers can be recommended. Mr. Hughes further points out that, in a tropical climate like Ceylon, manures, and even lime, should be applied in small successive doses. He finds Australian bone-dust contains a larger percentage of organic matter than that of Indian origin, but such matter is in the former case poorer in nitrogen. The Ceylon coffee-soils he finds generally deficient in phosphoric acid as well as in lime. Of the latter requisite of plant-food the coral-beds on the coasts of the island yield a satisfactory supply, which will become more available as the roads are improved. As a source of phosphoric acid he recommends steamed bones and superphosphates; but he does not seem to have thrown any light upon the possible presence of phosphatic minerals in Ceylon. Such deposits are of the greater moment as the bone-exporting countries of the world must, with the progress of cultivation, become fewer and fewer. If Australia and the mainland of India advance in the direction that is to be hoped, their bones will be needed for return to the soil whence they were drawn.

Nitrogen, Mr. Hughes thinks, should be supplied to the soil rather in the state of organic insoluble matter than as ammoniacal salts, or as nitrates, which are liable to be washed away before they can be absorbed by the roots of the trees. For the same reason he contends that potash, notwithstanding its predominance in the ash of the coffee-berry, should not be supplied in large quantities. He argues, also, that plants possess the power of abstracting potash from the soil to a much greater extent than they do the other important mineral elements. Admitting this power, still so long as a soil contains merely a finite supply of potash it must ultimately become exhausted by crops which absorb it to a considerable extent. The proportion of 4 per cent of potash, which Mr. Hughes would allow in coffee manures, is, however, one which we should not feel disposed to exceed.

Coffee cannot, however, be considered an exhausting crop. Taking 7 cwt. per acre as a very full average crop, and making the necessary additions for the pulp and the leaf, Mr. Hughes finds that the total amount of mineral matter removed yearly from the soil is but 60.2 lbs., as against 364 lbs. in the case of turnips, and 189 lbs. in the case of wheat.

"Carbonate of lime," the author remarks, "as a constituent of manufactured manures in England, would have decidedly a low value." Should not it be rather regarded as a negative quantity?

It has, we believe, been objected that Mr. Hughes, though pointing out many errors to be avoided, does not state with sufficient distinctness what is the best course to follow. We can scarcely admit this censure. He does not, indeed, lay down one universal formula to be applied under all circumstances, for the sufficient reason that no such universal remedy is possible; but those who mix their manures "with brains," as a certain eminent artist said of his colours, will find here most useful indications.

Incidentally the author notices a kind of tea made from the leaves of the coffee tree. It had a "somewhat strong flavour and slight medicinal taste."

rule, coal contains on the average $1\frac{1}{2}$ per cent of N, some even over 2 per cent. I cannot do better than refer J. B. T. in this respect to a series of analyses by Dr. Lyon Playfair, given in "Muspratt's Chemistry."

If Messrs. Rickman and Thompson have been working with coal containing as little as 0.36 to 0.39 N and have obtained the splendid result stated, such facts are all the more to the credit of their patent, but still I am puzzled and unable to understand how they obtain sufficient atmospheric nitrogen when in the provisional specification the following sentence occurs: "We expose the mixture to moderate heat in a slow burning furnace, supplying the furnace with a small amount of air," &c.—I am, &c.,

J. H.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 13, March 29, 1880

Determination of High Temperatures.—H. Sainte-Claire Deville and L. Troost.—One of the authors announced in September, 1878, that the measurement of elevated temperatures had been effected by means of the apparatus which they employed for the determination of the coefficient of expansion of porcelain simplified by the use of Sprengel's pump, which renders it possible to remove and measure when desired the thermometric matter (nitrogen) contained in the reservoir, and to calculate its temperature. The instrument and the manner of using it are described at length.

Researches on Diffusion.—L. Joulin.—These researches refer to the condensation of gases by porous bodies, the solution of gases in liquids, either on direct contact or when separated by a membrane, and the equilibria established between gases condensed or dissolved and a limited ambient atmosphere. In case of each of these phenomena the author has studied the influence of the pressure of gases varying from a few centimetres of mercury to 4 atmospheres, and at temperatures varying from 0° to 100°.

A New Property of the Vanadiates.—P. Hautefeuille.—The acid vanadiates of potassium, sodium, and lithium, when passing slowly from a liquid to a solid state, behave like silver under the same circumstances, emitting fine bubbles of gas and producing a sound resembling the decrepitation of common salt. The gas emitted is oxygen which has been absorbed from the atmosphere. If the true vanadic acid, the isomorph of phosphoric acid, is a body capable of combining without loss of weight with bases, and in particular with lithium, it contains per gramme 8.3 milligrms. less oxygen than vanadic acid which has been melted or dried in the air. As the equivalent of vanadium has been determined by the loss of weight which vanadic acid undergoes in passing to the state of vanadium tetroxide, the author is preparing for a re-examination of this method.

Certain Properties of Mixtures of Methyl-cyanide with Common Alcohol and Methylic Alcohol.—MM. Camille Vincent and Delachanal.—In order to separate methyl-cyanide from vinic alcohol the mixture is first submitted to fractional distillation in order to classify the products. The largest possible quantity of calcium chloride is then made to dissolve in the liquid boiling at the lowest possible temperature, so as to absorb the alcohol. The whole is then distilled in the water-bath, and the product once more submitted to fractionated distillation

CORRESPONDENCE.

AMMONIA FROM ATMOSPHERIC NITROGEN.

To the Editor of the Chemical News.

SIR,—I thank J. B. T. for pointing out to me that I can now obtain a copy of the complete specification of Rickman and Thompson's patent, and I have written for it.

Meanwhile I beg to call J. B. T.'s attention to some of his statements, which are evidently inaccurate as far as percentage of N usually contained in coal is concerned. The instances he quotes are the exceptions, while, as a

No. 14, April 5, 1880.

Determination of High Temperatures.—H. Sainte-Claire Deville and L. Troost.—The authors have studied the boiling-points of zinc and cadmium by means of the air-thermometer, and their determinations agree closely with those published by E. Becquerel. The boiling-points of zinc increase successively when hydrogen, air, and carbonic acid are used as the thermometric material. Although carbonic acid gives a boiling-point much higher than the other gases, the authors do not feel as yet warranted in ascribing the excess of volume to dissociation. One of them is about to undertake experiments with a view of finding the causes of the differences observed on employing the vapour of iodine instead of air as a thermometric material.

The Formation-heat of the Oxides of Nitrogen.—M. Berthelot.—For the protoxide the author gives the number -10.3 ; for nitric oxide, -21.6 ; nitrous acid, -11.1 ; hyponitric acid, -2.6 ; liquid, $+1.7$; nitric acid, -0.6 ; liquid, $+1.8$; solid, $+5.9$; dissolved, $+14.3$.

Continuous Gyratory Movements Produced by a Rotative Induction-machine.—W. de Fonvielle and D. Lontin.—The magnetic gyroscope of the authors cannot be intelligibly described without the accompanying figures.

Measurer of Energy.—Marcel Deprez.—Among the uses of this instrument the author states that it will show the quantity of heat necessary to effect a chemical decomposition. The graduation may show either calories or kilogrammetres or horse-powers. If applied to an electric lamp it shows at each instant the work absorbed by the production of light.

The Specific Heat and the Conductibility of Bodies.—M. Morisot.—A mathematical paper, not susceptible of useful abstraction.

Sulphides and Selenides of Chrome.—H. Moissan.—The author has obtained the sesqui-sulphide by exposing chromic sesqui-oxide, anhydrous but not calcined, to a current of dry sulphuretted hydrogen at the temperature of 440° . It is of a dark maroon colour, and is not readily attacked by acids, except the nitric and *aqua regia*, which dissolve it. If kept at a high temperature in a current of hydrogen it is converted into the monosulphide. The corresponding selenides are obtained in an analogous manner.

Thermo-chemical Study of the Earthy Sulphides.—P. Sabatier.—The sulphides in question are those of magnesium, aluminium, and silicium.

Crystalline Oxalic Acid.—A. Villiers.—The author has succeeded in obtaining the normal acid, $C_4H_2O_8$, in very fine octahedral crystals, by dissolving small quantities of ordinary oxalic acid in concentrated sulphuric acid, the latter being in large excess. Oxalic acid is almost as greedy of water as is sulphuric acid, and can even take away moisture from the latter when in a dilute solution.

Amidic Acids of α -Oxy-caproic Acid.—E. Du villier.—An account of the methyl-amido- α -caproic acid and of the corresponding ethylic compound.

Proportion between the Sugar and the Mineral and Nitrogenous Substances in Beet-root.—H. Pellet.—The order of utility of manurial elements for the beet-root is as follows:—Phosphoric acid, magnesia, lime, then follow potash and soda, and lastly nitrogen. The formulæ calculated from the author's researches agree very closely with those indicated fifteen years ago by M. G. Ville.

Simultaneous Reproduction of Orthose and Quartz.—P. Hautefeuille.—Phosphates employed along with fluorides render it possible to reproduce minerals belonging to different species, not only separately, but associated together as they are found in their natural beds.

Chemisches Central-blatt.

No. 6, 1880.

The matter in this number has been noticed elsewhere.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 1, 1880.

On a Series of Aromatic Bases Isomeric with the Oils of Mustard and the Sulphocyanates.—A. W. Hofmann.—This long and important paper is not susceptible of useful abstraction.

Synthetic Formation of Formic Acid.—V. Merz and J. Tiberica.—Various syntheses of organic bodies from inorganic materials are known, but at present it would be scarcely possible to prepare in this manner, on the large scale, any organic substance capable of an industrial application. As a first step in this direction, experiments with carbon oxide or dioxide seem to offer an especial interest. As it is well known these oxides have both been transformed into the first member of the series of the fatty acids, *i.e.*, formic acid. The earlier syntheses of this acid, by H. Kolbe, R. Schmidt, Maly, and Berthelot, are not capable of yielding the acid to any great extent. The authors caused carbon oxide to pass slowly over ordinary soda-lime at temperatures from 180° to 200° , and obtained formiates in abundance. The salt must be loose in texture, the gas moist, and the temperature must not exceed 220° .

A New Base.—E. F. Smith.—The base in question, $C_{21}Cl_{26}$, was obtained by the complete chlorinisation of toluol. When pure it forms large colourless prisms, fusible at 152° to 153° .

Products obtained by Nitrising Metachlor-salicylic Acid.—E. F. Smith and G. K. Peirce.—On nitrising metachlor-salicylic acid the authors obtained two products which they distinguish as A and B. The former is a crystalline mass which separates in long reddish needles, and is resolvable into two compounds. B is an acid, probably identical with that described by Hübner.

On Normal Propyl-alcohol from Glycerin—A. Fitz.—The author has obtained this compound as a product of fermentation.

On the α and β Position in Naphthalin.—F. Reverdin and E. Noeltling.—Already noticed.

Products of the Sulphurisation of Isomeric Nitramido- and Diamido-benzols.—Jul. Post and E. Hardtung.—Among the compounds obtained and examined are ortho-nitramido-sulphi-benzol with some of its salts, ortho-diamido-sulphi-benzol, and meta-nitramido-sulphi-benzol.

Detection of Alizarin, Isopurpurin, and Flavopurpurin when they occur simultaneously, and on the Quantitative Determination of Alizarin.—E. Schunck and H. Roemer.—Already noticed.

On Certain Azo-Compounds.—J. H. Stebbins.—The author obtains azo-benzol-trinitro-oxybenzol by the action of 1 mol. picric acid in alcoholic solution upon 1 mol. diazo-benzol nitrate. Azo-benzol-pyrogallol is obtained by the action of 1 mol. of pyrogallol in an alkaline solution upon 1 mol. of diazo-benzol nitrate.

A Lecture Apparatus.—H. Schulze.—This paper requires the accompanying illustrations.

A Synthesis of Cumol.—A. Liebmam.—The author obtains cumol by the action of benzol-chloride diluted with six volumes of anhydrous ether upon zinc methyl at a gentle heat.

The Third Anthracen Mono-carbonic Acid.—C. Liebermann and A. Bischof.—Not suitable for abstraction.

Composition of Weldon Mud and Similar Compounds (Second communication).—Jul. Post.—The author gives several analyses of the mud in different states. He does not consider the question of the existence of a man- ganous acid as decided.

Contributions to a Knowledge of the Spontaneous Oxidation of Manganous Hydroxide with Reference to the Regeneration of Manganese.—Jul. Post.—A continuation of the memoir begun in *Berichte* (xii., 1537), and not susceptible of useful abstraction.

Action of Sulphuric Acid upon Phosphates, especially Calcium Phosphate, with Reference to the Manufacture of Superphosphate.—Jul. Post.—The author's experiments, which are still in progress, lead him to the opinion that on the action of 2 mols. sulphuric acid upon 1 mol. tricalcic phosphate the equivalent proportion of phosphoric acid is first set free, and then acts upon the unattacked tricalcic phosphate.

On Mono-phenyl-borchloride and the Valence of Boron.—A. Michaelis and P. Becker.—The authors consider the pentavalence of boron highly probable, though not formally demonstrated.

Carbon Acids of Pyridin.—S. Hoogewerff and W. A. van Dorp.—Since the discovery of pyro-cinchomeionic acid the three monocarbo-pyridic acids which are possible, in accordance with Körner's formula for pyridin, are known, since both nicotic and picolic acid are known as such.

Studies on the Compounds obtained from Animal Tar.—H. Weidel and G. L. Ciamician.—In this communication the authors describe the non-basic compounds. In their general summary of the bodies they obtained they mention, as the chief products, nitriles of the butyric, valerianic, capronic, isocapronic, caprinic, palmitic, and stearic acids; further, pyrrol, homo-pyrrol, dimethyl-pyrrol, and hydrocarbons of the composition C_9H_{14} , $C_{10}H_{16}$, $C_{11}H_{18}$. As by-products there appear pyridine, picoline, lutidine, chinoline, phenol, propionitril, valeramid, toluol, ethyl-benzol, and naphthalin.

Action of Potassium Ferricyanide upon Morphine.—Karl Polstorff.—The author has obtained by this reaction oxydimorphine, $C_{34}H_{36}N_2O_8 + 3H_2O$.

On Schützenberger's Oxymorphine.—K. Broockmann and K. Polstorff.—Both the free base and its salts agree in their properties with the oxydimorphine described in the foregoing paper.

Action of Potassium Permanganate upon Morphine.—K. Broockmann and K. Polstorff.—Oxydimorphine was here also obtained.

Action of Atmospheric Oxygen upon Morphine in an Ammoniacal Solution.—K. Broockmann and K. Polstorff.—Here also oxydimorphine was obtained.

Action of Potassium Ferricyanide upon Methylmorphine Iodide.—K. Polstorff.—The author obtained basic methyl-oxydimorphine iodide, a corresponding neutral salt, and methyl-oxydimorphine hydroxide.

On Methylmorphine Hydroxide.—K. Broockman and K. Polstorff.—The authors have isolated this compound in a state of purity.

Action of Benzoyl-chloride upon Morphine.—K. Polstorff.—Three benzoyl groups are introduced into the morphine.

On Acridin.—C. Graebe and H. Caro.—This memoir does not admit of abstraction.

On Artificial Alkaloids.—A. Ladenburg.—The identity of natural and artificial atropine is demonstrated both by their chemical and physical attributes and by their physiological effects.

Revue Universelle des Mines, de la Metallurgie, &c.,
Tome 6, No. 3, Nov. and Dec., 1879.

Electric Lighting.—H. de Backer.—An account of the rise and progress of electric lighting, with especial notices of the machines of Meritens, Deprez, Lontin, Gramme, Hofener-Altenneck; the regulators of Jaspas, Gaiffe, Carré, Serrin, Lontin, Mersanne; the Jablochkoff and the Wilde candles; the lamps of Regnier and Werdermann, &c. We do not find any notice of the improvements of M. Tommassi, which is surely an omission.

The Royal School of Mines, of London.—A. Ronna.—The author denounces the removal of the chair of Metallurgy from Jermyn Street to South Kensington, and the resignation of Dr. Percy, and gives at length the solemn protest of this distinguished metallurgist.

The "Pantelephone" of Loch.—The author figures and describes a new instrument which he has constructed, and which is free from the defects of previous telephones.

The Influence of Air in the Reduction of the Ores of Zinc.—J. Binon.—The author shows that the presence of air is mischievous, by decreasing the points of contact between the ore and the fuel, and thus impeding reduction.

Suppression of Animal Charcoal in the Sugar Manufacture.—V. Francken.—M. Meyer substitutes for bone-black silica prepared in a peculiar manner, which is not described, but which is said to give very satisfactory results.

Moniteur Scientifique, Quesneville.

February, 1880.

A New Process for the Separation of Nickel and Cobalt.—Ph. Dirvell.—Inserted at length.

The Extraction of Perfumes by means of Methyl Chloride.—Camille Vincent.—Already noticed.

Methylated Products and the Preparation of Pure Methylic Alcohol.—M. Bardy.—A report presented to the Société d'Encouragement on the result of the author's researches on the applications of methylene in the manufacture of coal-tar colours, and on a new process for obtaining methylic alcohol, as also on methods for the analysis of mixtures of vinic and methylic alcohols.

NOTES AND QUERIES.

Portland Cement.—Will Mr. Watson please to give the formula of the double silicate of alumina and lime which the materials for Portland cement should be proportioned to produce, and oblige.—**PORTLAND CEMENT.**

Condensation of Ammonia.—Can any of your readers assist me in the following? In submitting to heat a mixture of sulphate of ammonia or sal-ammoniac and chalk, there result sesquicarbonate of ammonia, and also a d-terminate quantity of free ammonia and water, which latter (NH_4O) it is of course necessary to preserve, in order to conduct the process economically, and on this point none of the textbooks I have read give any detail. Any information you could give me as to a ready, efficient, and inexpensive method of collecting this free ammonia in water to be subsequently reconverted into sulphate or chloride will be highly appreciated.—**A CONSTANT READER.**

MEETINGS FOR THE WEEK.

- MONDAY, 10th.**—Society of Arts, 8. Robert W. Edis, F.S.A., "The Decoration and Furniture of Town Houses."
Royal Geographical, 8.30.
- TUESDAY, 11th.**—Civil Engineers, 8.
Royal Medical and Chirurgical, 8.30.
Photographic, 8.
Anthropological Institute, 8.
Royal Institution, 3. Mr. R. H. Scott, "Wind and Weather."
- WEDNESDAY, 12th.**—Society of Arts, 8. Mr. Charles Wood, "Utilisation and Properties of Blast Furnace Slag."
Geological, 8.
Microscopical, 8.
- THURSDAY, 13th.**—Royal Institution, 3. Prof. Tyndall, "Light as a Mode of Motion: Theories of Light and Colour."
Royal Society Club, 6.30.
Royal, 8.30.
Society of Arts, 8. Prof. W. G. Adams, F.R.S., "The Optical Properties of Crystals and some of their Practical Applications."
- FRIDAY, 14th.**—Royal Institution, 8. Lord Reay, "Certain Aspects of Social Democracy in Germany," 9.
Astronomical, 8.
Quekett, 8.
Clinical, 8.30.
Society of Arts, 8. A. Terrien de la Couperie, "China and the Chinese: Their Early History and Future Prospects."
- SATURDAY, 15th.**—Royal Institution. Prof. Morley, "The Dramatists before Shakespeare," 3.

TO CORRESPONDENTS.

Joseph Hemingway.—Mr. A. H. Allen's "Introduction to the Practice of Commercial Organic Analysis" is the most suitable. We do not know of any work which deals with oils and soaps alone.

Will Messrs. Rickman and Thompson, who have just taken patents for the production of ammonia from the nitrogen of the atmosphere, kindly forward their address to our Office.

THE CHEMICAL NEWS.

VOL. XLI. No. 1068.

ON THE DETECTION OF STARCH SUGAR MECHANICALLY MIXED WITH REFINED CANE SUGAR.*

By P. CASAMAJOR.

ABOUT a year ago we were often entertained by the daily press with accounts of the adulterations practised by sugar refiners, and, among these adulterations, the one most generally used, as we were then told, was the mixing of refined sugar with starch glucose. I must confess that I never believed in such a practice; for, although I had tried to procure specimens of such sugars, I was not able to find any, and one or two specimens which were given to me, as of sugar so adulterated, turned out to be pure, as far as the presence of starch sugar was concerned. The idea that these sugars were so adulterated very likely originated in the imperfection of the processes used to detect the presence of starch sugar.

This week, however, I have had the good fortune to come in possession of a sample of refined sugar largely adulterated with starch glucose. This sample was sent to Messrs. Havemeyer and Elder from St. Louis, and a slip of paper in the box gave the information that the barrel from which this sugar was taken was marked "Powdered Sugar, Manhattan Sugar Refinery, New York;" a refinery of the existence of which I have not been able to find any proof.

The incredulity which I previously maintained on the existence of adulteration by starch sugar was based on the following considerations:

From a sugar solution you can only obtain, by the ordinary processes of a refinery, a quantity of crystallised sugar representing very nearly the difference between the cane sugar present and the soluble impurities. If you have, for instance, a sugar solution whose co-efficient of purity is 90 per cent the soluble impurities will represent 10 per cent of the total of the substances in solution, and you can obtain, at the utmost, only $90 - 10 = 80$ parts of sugar from 100 parts of substances dissolved. Processes have been published whereby a greater yield may be obtained, but such processes require the use of alcohol or of large quantities of salts of magnesia, and they have never been used in any extended scale. As starch sugar in solution does not act otherwise than any other impurity in solution, the addition of it in a dissolved state to a sugar solution could not have any other effect than to diminish the yield of sugar and increase that of molasses.

The only manner in which starch glucose can be mixed with refined cane sugar, so as to give a profit, instead of a loss, to the person effecting this mixture, is to mix the two substances in a solid state. Now, the starch sugar must either be added in large quantities—and then it seemed to me that it could be easily detected by the eye or by the taste; or it must be added in very small quantities, and then the difference between the price of sugar and that of starch glucose would not leave a profit proportional to the trouble. My experience with sugar refiners does not lead me to believe that the refiner exists who would adulterate his products by adding to them only one or two per cent of starch glucose. Such a thing would not pay.

The object of this communication is to give a few easy

processes for the detection of starch glucose in commercial sugar.

By the use of the optical saccharometer, the presence of starch sugar may be easily detected, when in quantities as large as in the sample in question. It is a dextro-rotate substance, and no other dextro-rotate substance could be used to adulterate sugar with efficiency in such large quantities.

The direct test by the optical saccharometer gives 97 per cent. After inversion, the reading of the saccharometer is 9.8 per cent to the left at 21° C. If we add these numbers we find by Clerget's table that they correspond to 80 per cent of cane sugar. As the sugar is dry, the balance, 20 per cent, nearly represents the quantity of starch sugar, as from the appearance of the sugar there must very little inverted sugar present.

I need not now consider the subject of inversion as a means of ascertaining the actual quantity of cane sugar present in a sample of commercial sugar, as I have already had occasion, in a paper read before this Society at our regular meeting of February of last year, to discuss this subject at great length.* I may, however, say that if this sugar had been what we call in the sugar business a *straight sugar*, 97 per cent of sugar, after inversion, would have given at 21° C. -32.5° , instead of -9.8° . I call attention to this to show of what precious help the process of Clerget is in cases of this kind. It is a sad truth that very many persons, whose occupation is the analysis of sugars, are either entirely ignorant of the process of Clerget, or do not know enough about it to use it when they need it.

This sugar, tested by the alkaline copper solution, gives 17 per cent of reducing substances calculated as glucose, which shows that the starch sugar mixed with refined sugar contained $\frac{17}{100} = 85$ per cent of these reducing substances.

The process which I had the honour of describing before this Society at our June meeting† gives unsatisfactory results with sugar adulterated by starch glucose. This is due to the imperfect solubility of starch glucose in methylic alcohol. After grinding the sugar under examination with the standard solution for three minutes, the process gave 85 per cent of pure sugar—a discrepancy from the result by the saccharometer (80 per cent) which never takes place with straight sugars.

Besides these processes, which can only be applied by persons provided with scientific appliances, the presence of starch glucose, when used in such large quantities, may be readily detected by very simple processes, which may be applied by any body who wishes to use such means as are always at hand.

One process for the detection of starch glucose consists in adding to the suspected sugar somewhat less than its own weight of cold water and stirring for a few seconds. If starch sugar is present, it will be seen floating in the solution as white specks which resemble crushed wheat. This appearance is due to the comparative insolubility of starch glucose in cold water, which allows it to remain undissolved quite a long time, and also to the fact that as the cane sugar present is crystalline, and its refrangibility is not very different from that of a sugar solution, the portion of it which remains undissolved is not so distinctly seen as the specks of starch sugar. These specks are best seen by using a beaker glass, and putting only enough sugar and enough water that we may be able to see light through the flat bottom of the glass. If a flat-bottomed glass is not at hand, the observation may be made on a pane of glass.

This process for the detection of mixtures of starch

* See *Journal of the American Chemical Society*, vol. i. p. 26; also *CHEMICAL NEWS*, vol. xxxix., pp. 212–234, *Sugar Cane*; vol. xi., p. 296; *Moniteur Scientifique*, Juin, 1879, p. 647; *Stammer's Zeitschrift*, Juli, 1879, p. 683.

† See *Journal of the American Chemical Society*, vol. i., p. 205; also *CHEMICAL NEWS*, vol. xl., pp. 74, 97, 107, 131; *Sugar Cane*, vol. xi., pp. 531, 588, 693; *Zeitschrift des Vereins*, October, 1879, p. 957; *Ann. de Chimie et de Phys.*, 5th series, vol. 18, p. 559.

* A Paper read before the American Chemical Society, March 4, 1880.

glucose with cane sugar is so simple and satisfactory that I might well be excused from giving another, but the one I am about to give is useful for the detection of other foreign substances in the products of refineries. This process is based on this, that the taste of sugar has a tendency to dull our perceptions of the taste of other substances mixed with sugar. So as to neutralise this effect of sugar, we may, before tasting a suspected sample, put a pinch of pure sugar in the mouth. If after this sugar is dissolved, but while we still perceive its sweet taste, we put on the tongue a pinch of sugar containing starch glucose we distinctly perceive the bitterish taste of the glucose.

This process is very useful for detecting other foreign matters besides starch sugars. The presence of chloride of tin in molasses or sugars, even when used in very small quantities, can be easily detected by its unpleasant bitter taste if, before tasting the suspected products, we fill the mouth with the pure sweet taste of refined cane sugar.

VOLUMETRIC DETERMINATION OF PHENOL.

By P. DEGENER.

ON adding bromine water to a liquid containing phenol there is precipitated tribromo-phenol, which, under the circumstances of the experiment, is not attacked by an excess of bromine. Upon this reaction the author bases the following volumetrical process:—Standard bromine water is gradually added to a solution of phenol sufficiently diluted, preferably containing less than 1 per cent. The end of the reaction—that is, the moment when the liquid contains a trace of free bromine—is determined by means of a starched potassium iodine paper. This final reaction is very distinct; and if the liquid under examination is colourless, the least excess of bromine may be detected by the yellowish tint which it communicates.

The author employs a solution containing 40 grms. bromine and 20 grms. potassium bromide in a litre of water. It is standardised by means of a solution of pure phenol, an operation which must be repeated daily, as bromine water quickly loses strength. The solution of phenol itself is not permanent, so that it is preferable to determine the value of the bromine solution by an iodometric process.—*Journal für Praktische Chemie*.

ON THE DETERMINATION OF CHROMIUM.

By THEODOR WILM.

By occasion of numerous determinations of chromium, the author has repeatedly satisfied himself of the existence of the important error (see Souchay, *Zeitschrift Anal. Chem.*, 1865, iv., 66) arising on the precipitation of chromic oxide by ammonia in glass vessels, as during the operation—by the action of the alkaline liquid, and the subsequent washing with hot water—constituents of the glass pass into the precipitate.

Souchay shows in his memoir, by comparative analyses, that this error is avoided if the precipitation of the chromium is effected not in an alkaline solution, e.g., by means of mercurous nitrate, and that the result is strictly accurate if all the operations are conducted in platinum vessels, thus proving that the glass is the source of the foreign matter which occasions the difference.

The author finds that the error is greater than was determined by Souchay in his two analyses of potassium bichromate. He has also been unable to confirm the statement that on re-dissolving the washed precipitate in hydrochloric acid, and re-precipitating with ammonia, the error was doubled. The excess of chromic oxide found over the calculated quantity was in some experiments 4 per cent.—*Berichte der Deutsch. Chem. Gesell.*

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

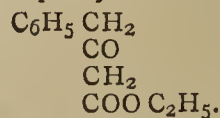
Thursday, May 6, 1880.

Prof. H. E. ROSCOE, President, in the Chair.

AFTER the minutes of the previous meeting had been read and confirmed, the following certificates were read for the first time:—H. Brown, G. H. Hughes, H. Aubrey Lawrence, R. S. Marsden, E. A. Reilly, Brenton Symons, H. Kneebone Tompkins, A. Wingham.

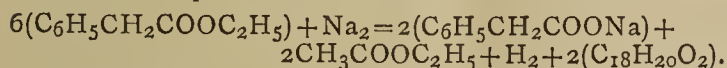
The PRESIDENT then called on Dr. HODGKINSON to read a paper "On the Action of Sodium on Phenyl Acetate," by W. H. PERKIN, jun., and W. HODGKINSON. One of the authors has already published some results as to the action of sodium on ethereal salts containing mixed fatty aromatic groups. In the present paper they first give an account of the action of sodium on phenyl acetate, prepared by heating dry phenol with acetyl chloride: the acetate boiled at 193° C. A most violent action took place with considerable rise of temperature. Hydrogen and much acetic ether were evolved, the silver salt from the latter being analysed. After the sodium had disappeared a solid body remained in the flask, which, when treated with water, formed a dark coloured solution and a small quantity of a thick oil. The aqueous solution yielded acetic acid, phenol, and salicylic acid. The thick oil gave on distillation some undecomposed phenyl acetate and a crystalline mass, which was pressed between filter-paper and crystallised from petroleum. Two forms of crystals were observed, and were separated by crystallisation from alcohol. The substance most soluble in alcohol forms glistening white needles melting at 48°, giving a red colouration with potash, and having the composition $C_{15}H_{12}O_3$. The other substance crystallises in yellow needles, melting at 138°, giving a yellow crystalline compound with alcoholic potash, and having the composition $C_{18}H_{14}O_4$. Experiments were now made by heating cresylic acetate with sodium. Acetic ether and salicylic acid were obtained as in the previous reaction; probably, therefore, these two substances are constant products of the action of sodium on acetyl phenols.

Dr. HODGKINSON then read a paper entitled "Preliminary Notice on the Action of Sodium on some Ethereal Salts of Phenyl-acetic Acid." In a contribution to *Liebig's Annalen* by Max Conrad and the author of the present paper it has been shown that the influence of the presence of a phenyl group in ethers of fatty acids results in the substitution of one or more of the hydrogen atoms in combination with the carbon immediately joined to the carbonyl group by the group containing the phenyl, giving a synthesis of so-called aromatic fatty acids. From phenyl acetate the benzylic ether of β -phenyl-propionic acid was obtained as a primary product, and, secondarily, by the action of sodium on this substance, dibenzyl-acetic and β -phenyl-acrylic acids. Benzylic propionate, butyrate, &c., gave analogous results. The author of the present paper has experimented as to the effect of a phenyl group in the acid or negative portion of the ethereal salt. For this purpose α -toluic acid seemed likely to give results, as from its ethyl ether might be formed ethyl-phenyl aceto-acetate,—



2.5 grms. of sodium were warmed with 50 grms. of the ether. A condenser was fitted to the retort, so that the liquid produced was removed, as fast as it was formed, from contact with the sodium. In one experiment 10 grms. of a liquid distilled over, which, after re-distillation, boiled between 75° and 78°. On analysis it proved to be ethyl acetate. The residue in the retort forming a thick mass

was treated with water, and gave a solution in which sulphuric acid caused the formation of a white precipitate and an oily body; the latter was separated and distilled. On shaking the acidified aqueous solution with ether, phenyl-acetic acid was obtained, crystallising from water in long needles, melting at 76° ; a small quantity of acetic acid was also obtained. The oily product on distillation separated into some unaltered ethylic phenacetate and an oil which, after purification, boiled at 250° at 60 m.m., having the composition $C_{18}H_{20}O_2$. A residual oil gave on analysis numbers indicating $C_{18}H_{16}O$. The decomposition may thus be represented—



The action of sodium on other ethers of similar constitution seems to be quite analogous, but more secondary products are formed. Experiments were carried out with propylic phenyl acetate: propylic acetate was formed. From the residue in the retort were obtained phenyl-acetic acid, a substance crystallising from petroleum in white needles, having a composition indicated by the formula $C_{24}H_{20}O_3$, insoluble in acids and alkalis, and a yellow oil boiling at 335° at a pressure of 50 m.m., giving on analysis $C_{22}H_{20}O_2$. The reaction has also been extended to a primary isobutylic phenyl acetate. It appears, therefore, that the first products of the action of sodium on the ethers of phenyl-acetic acid, containing ethyl and its homologues, are the corresponding ethylic, &c., ethers of acetic acid. The phenyl group of the phenyl-acetic acid being replaced by hydrogen, it further reacts with sodium on another portion of the original ethereal salt, forming the above liquid and solid bodies whose constitution is as yet undetermined, hydrogen being at the same time evolved. With benzylic phenyl-acetate the action of sodium appears to be analogous with that on the benzylic ethers of the normal fatty acids.

After a few remarks from Dr. FRANKLAND as to the importance of the results,

The PRESIDENT proposed a vote of thanks to the authors, and expressed his gratification that a son of the Secretary had already commenced to contribute papers to the Society.

The next paper was "*On the Determination of Nitrogen in Carbon Compounds*," by C. E. GROVES. The author described and exhibited an apparatus for facilitating the collection and measurement of nitrogen evolved during the combustion of a substance according to Dumas's method. It consists of two vertical glass tubes, in size and shape somewhat resembling burettes. One, *a*, is graduated, and is closed at its upper end by a glass stopcock. Into its lower end is fixed a T-tube, one leg of which is attached to the combustion-tube, a bulb U-tube containing a globule of mercury being interposed. To the other leg of the T-tube is attached a long india-rubber tube, the other end of which is connected with the bottom of the second vertical glass tube, *e*. The combustion-tube is charged in the ordinary way. The tube *a* is filled with potash solution (sp. gr. 1.25), and *e* is lowered until the opening of the T-tube which leads to the combustion-tube is free: carbonic acid is then evolved, and the air in the combustion-tube swept out as usual. The graduated tube *a* is then filled with potash, and the stopcock closed as soon as the bubbles of gas are completely absorbed. The tube *e* is then lowered until the level of the potash in it is a little below the T-tube. The combustion is conducted in the ordinary manner. The advantages claimed for the apparatus are its simplicity and the fact that as the combustion is carried out under diminished pressure there is no tendency for the tube to blow out. The author also gives details of a method for using the same tube for six to twelve combustions. The mixture of CO and CO₂ obtained by the action of strong sulphuric acid on oxalic acid is recommended as preferable to hydrogen for reducing the oxidised copper gauze. The author also exhibited a simple arrangement for collecting or delivering small quantities of gas.

Mr. PERKIN remarked that he had never yet been able to get all the air out of a tube by CO₂; he always had to use the Sprengel pump.

Mr. KINGZETT had experienced the same difficulty as the previous speaker, and had obviated it in the same way.

Dr. FRANKLAND strongly recommended the use of the Sprengel pump, and suggested the application of a water-jacket round the measuring-tube.

Mr. GROVES, in reply, said that he had used a water-jacket round the tube, and that by passing the CO₂ for the same time at the same beginning as at the end of the combustion the error due to the minute bubble of air could be almost completely corrected.

Mr. M. M. P. MUIR then read a paper "*On Essential Oil of Sage*." The results of the author may be summarised as follows:—The composition of essential oil of sage varies with the age of the oil. When freshly distilled it contains comparatively small quantities of salviol, camphor, and cedrene. As the oil ages the quantities of these substances, especially of the first two, increase. The oil distilled from English sage contains much cedrene, boiling at 260° , with small quantities of $C_{10}H_{16}$, hydrocarbons, and traces of oxidised compounds. The terpene of sage oil is identical with that from French turpentine; probably an isomeride of terpene, boiling at 171° , is also present. Salvial has the formula $C_{10}H_{18}O$, not $C_{10}H_{16}O$. When oxidised with chromic or dilute nitric acid it yields camphor, melting at 174° , with oxalic and acetic acids. Salvial on distillation is slightly decomposed, water and a $C_{10}H_{16}$ compound being produced. Sunlight, in the presence of air, and in the course of time, forms from the $C_{10}H_{16}$ compounds of sage oil, salviol, and small quantities of camphor. The latter substance is also formed by the action of sunlight on salviol. The action of phosphoric anhydride on salviol is very complex, there being formed polymerides of $C_{10}H_{16}$, one boiling at 171° , a hydrocarbon of the benzene series boiling below 130° , and a paraffin boiling between 170° and 180° : no cymene is found if the action be continued for some time. Phosphorus pentachloride acts upon salviol at high temperatures, producing a chlorinated derivative, which is decomposed on distillation, forming a $C_{10}H_{16}$ hydrocarbon boiling at 157° , and probably the paraffin-like body mentioned above. Bromine acts energetically on salviol, hydrogen and carbon being liberated. A brominated oil may be separated, and under certain conditions small quantities of camphor melting at 174° . Camphor separates chiefly from the portions of sage oil distilling between 205° and 208° ; it is partially soluble in salviol, but separates out on cooling the solution to -15° . Sage camphor melts at 174° , boils at 205° ; chemically it appears to be identical with laurel camphor; it is, however, optically inactive. The compounds present in sage oil are stable when pure, but when mixed with small quantities of other bodies they decompose. Processes of oxidation, deoxidation, and polymerisation probably occur simultaneously during the ageing and fractionation of sage oil. In discussing his results the author expresses an opinion that the camphor group may be regarded as a link between the "closed chain" and the $C_{10}H_{16}$ series, just as the $C_{10}H_{16}$ group forms a link between the fatty and the closed chain compounds.

Dr. WRIGHT complimented the author on the completeness of his paper. Though some of the reactions had been already worked out, additional and more complete evidence was always very valuable. He thought the author's views as to the relation between the closed chain compounds and the camphors were open to discussion.

Mr. KINGZETT had been much interested in the paper, as it seemed that in many points the oxidation of sage oil was analogous to that of the terpenes. In one experiment he had obtained from turpentine colourless crystals closely resembling camphor. He should like to ask if Mr. Muir had observed during the oxidation of sage oil in the air any production of hydroxyl.

Mr. MUIR had not specially looked for hydroxyl during the present investigation, but had noticed its formation in a previous research. He also replied to the criticisms of Dr. Wright on the theoretical portion of the paper.

Mr. A. H. ALLEN then read a paper "*On the Presence of Nitrogen in Iron or Steel*," which will appear in our next issue.

The following papers were taken as read:—

"*On the Mode of Application of Pettenkofer's Process for the Determination of Carbonic Acid in the Expired Air*," by W. MARCET. The author describes the apparatus which he has used in upwards of 350 determinations during a series of experiments on the influence of altitude on the phenomena of respiration (*Proc. Roy. Soc.*, 1878 and 1879). The air from the lungs is collected into a large india-rubber bag, whose capacity under a certain pressure was known; two bags were used, one holding 39.3, the other 68.4 litres. For the analysis of the air the following apparatus is used:—A thick glass tube or cylinder, about 2 litres capacity, is supported on a light tripod stand; both ends are ground and are closed with thick ground glass discs, which by means of grease and two screwed brass caps close the cylinder air-tight. In the upper glass disc three holes are made, in which fit air-tight a thermometer and two brass stopcocks. The lower disc has attached to its inner surface a thick vulcanite disc, whose upper part is slightly funnel-shaped, so as to drain off completely any liquid from the cylinder. Perforations are made in the lower disc for attaching a tube from an air pump, and for drawing off liquid. The cylinder is first dried and exhausted and the air from the large bag allowed to enter; the cylinder is again exhausted and filled a second time, the pressure being noted by a manometer and the temperature observed. A measured quantity of standard baryta solution is now introduced, the increase of volume is diverted into a small empty india-rubber bag connected with the cylinder, so that the pipette can be emptied by closing its top and warming the bulb with the hand. All the stopcocks are now closed and the cylinder gently shaken; the milky liquid is then run into a bottle, which is corked and paraffined. Some of the clear liquid is decanted into a burette and titrated against a measured quantity of standard oxalic acid, turmeric being used as an indicator. The whole apparatus is portable, and the results are perfectly reliable. The details of an experiment with the calculations are given.

"*Note on an Improved Form of Oven for Heating Sealed Tubes and avoiding Risks of Explosions*," by WATSON SMITH. The author has constructed such an oven having the following advantages:—A temperature of 420°–440° can be obtained in a few minutes with one moderately large Bunsen. It ensures the safety (in case of explosion) of the experimenter and of the thermometer. It has a simple construction, and can be cooled down rapidly. It consists of a horseshoe-shaped inner tube of stout sheet iron, which contains thick iron tubes, surrounded by an outer D-shaped cover of sheet iron. A figure accompanies the paper.

"*Note on a Convenient Form of Lead Bath for Victor Meyer's Apparatus for Determining the Vapour Densities of High Boiling Substances, together with a few slight modifications*," by WATSON SMITH. The author advises the cutting off the capillary delivery tube about half an inch from the main stem, and connecting the cut off delivery tube to the main stem with a piece of caoutchouc tubing only when everything is adjusted for a determination. Detailed drawings with measurements accompany the paper. A piece of iron gas-tube, 9 to 10 inches long and 2 inches internal diameter, forms the lead bath; two screens of sheet iron are used, one to steady the gas flame, the other to protect the upper part of the apparatus from draughts. The glass bulb is well smoked in a luminous gas flame, and introduced into the lead very gradually.

After the thanks of the meeting had been given to the authors for their respective papers, the Society adjourned to May 20th, when a paper will be read "*On the Oxida-*

tion of Peaty Matter," by Miss Lucy Holcrow and Dr. E. Frankland, and the discussion on Dr. Tidy's paper (which will be printed this week) "*On River Water*," will take place.

PHYSICAL SOCIETY.

Ordinary Meeting, May 8, 1880.

SIR WILLIAM THOMSON, President, in the Chair.

NEW members: E. F. Bamber, Dr. E. Obach, R. D. Turner, E. Woods, H. E. Roscoe, H. Watts.

Prof. MINCHIN, of Cooper's Hill Engineering College, described his further researches in the subject of photo-electricity brought by him before the last meeting of the Society. He has found that the current in a sensitive silver cell does not always flow from the uncoated to the coated plate. It does when chloride or bromide of silver is used, but when the sensitive emulsion is iodide of silver and the liquid water tintured with iodide of potassium, the current is from the coated to the uncoated plate. He demonstrated that the current set up by the fall of light on the cell could be sent by wire to a receiving cell and made to produce a local effect on the sensitive plate therein. He also proved that electricity is developed in fluorescent bodies by the action of light, and hopes to show that it is also developed in phosphorescent bodies. Neither heat nor the red rays produce this electricity, but it is the blue and violet rays which do so. The fluorescent silver plates he employed were coated with an emulsion of eocine and gelatine, and had kept sensitive for twelve days. They would thus be a permanent source of photo-electricity did the eocine not tend to leave the gelatine. Mr. Wilson had suggested naphthaline red for eocine, as not apt to leave the gelatine, and he had found it give good results.

Dr. O. J. LODGE described certain improvements which he had made in his electrometer key designed for delicate electrical and especially electrostatic experiments. Assisted by the British Association he had made it more convenient, and fitted it into an air-tight case which could be artificially dried. The contact pins were now of phosphor-bronze, gilt, instead of platinum, and the contacts were made by press-pins from the outside. Dr. Lodge also exhibited a new inductometer or modified form of Prof. Hughes's induction balance, combining a Wheatstone balance, and expressly designed for comparing capacities and resistances, especially the resistances of coils having no self-induction. A telephone takes the place of a galvanometer in the bridge, and the current in the primary coil is interrupted by a clockwork make and brake. There is one primary coil of fine wire, $3\frac{1}{2}$ ohms in resistance, and two secondaries, one on each side of it, of fine wire, each about 270 ohms; these are fixed, but the primary is adjustable by a screw.

Prof. HUGHES remarked that he had pointed out in his paper to the Royal Society that the induction balance could be used in this way; and Dr. Lodge disclaimed any novelty in the apparatus beyond its arrangement.

Sir W. THOMSON added that it was satisfactory to see so serviceable an adaptation of the induction balance to research.

Dr. HOPKINSON, Prof. PERRY, and Sir W. THOMSON offered remarks on the element of time in comparing discharges from condensers of different dielectrics. Sir W. Thomson said that in 1864 he had made experiments on air and glass dielectrics and found the discharge about the same for the first quarter second.

Prof. ADAMS then took the Chair, and

Sir W. THOMSON made a communication on the elimination of air from a water steam pressure thermometer, and on the construction of a water steam pressure thermometer. He said it was a mistake to suppose that air was expelled by boiling water, because the water dissolved

less air when warm than when cold. The fact was due to the relations between the density of air in water and the density of air in water vapour. There was fifty times more air in the water vapour over water in a sealed tube than in the water below. If this air could be suddenly expelled only 1-50th part of air would remain, and of this only 1-2500th in the water, the rest being in the vapour. This suggested a means of eliminating air from water, which he had employed with success. It consisted in boiling the water in a tube, and, by means of a fluid mercury valve, allowing a puff of the vapour to escape at intervals. Sir W. Thomson also described his new water steam thermometer, now being made by Mr. Casella. It is based on the relations of temperature and pressure in water steam, as furnished by Regnault's or other tables, and will consist of a glass tube with two terminal bulbs, like a cryophorous, part containing water, part water steam, and the stem enclosed in a jacket of ice-cold water. Similar vapour thermometers will be formed, in which sulphurous acid and mercury will be used in place of water, or in conjunction with it. For low or ordinary temperatures they will be more accurate than ordinary thermometers.

SOCIETY OF ARTS.

SECTION OF APPLIED CHEMISTRY AND PHYSICS.

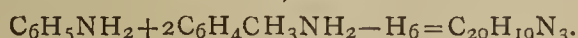
Thursday, April 8th, 1880.

Prof. CHARLES GRAHAM, D.Sc., F.C.S., in the Chair.

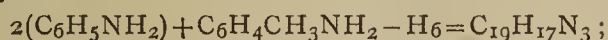
THE paper read was on "*The Newer Artificial Colouring Matters Derived from Benzene*," by R. J. FRISWELL, F.C.S., F.I.C.

Methylaniline Violets.—It is, no doubt, well known to many here that the earliest violets obtained by artificial means were those produced by the action of pure aniline or phenylamine on roseine, in the presence of an organic acid. A study of this reaction by Hofmann led to his discovery of the action of the iodides of the alcoholic radicles, methyl and ethyl, on roseine base, with the production of the well-known "Hofmann violets." These were found to be substitution-products of rosaniline, in which one, two, or three atoms of hydrogen in the molecule are replaced by the radicles methyl or ethyl, according as the iodide of either has been used.

Now roseine itself is, as is well known, produced by the action of arsenic acid or other oxidising agents on a mixture of aniline and toluidine. The chemical formula then adopted for it led to the conclusion that it was produced by the coalescence of the residues of two molecules of toluidine and one of aniline, thus:—

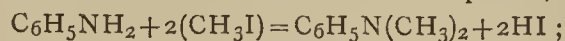


O. and E. Fischer have recently shown that this formula was partly erroneous, and that the reaction could also occur between two molecules of aniline and one of paratoluidine—



but this does not affect the inference drawn from what was, till recently, supposed to be the constitutional formula of the body in question, and this inference was that the methylated derivatives of roseine could be obtained by the oxidation of the methylated derivatives of aniline, just as roseine was by the oxidation of aniline and toluidine. The inference was somewhat rash; the methyl groups in dimethylaniline replace the hydrogens in the amido group, and on oxidation might, perhaps, be destroyed. However, on oxidation, dimethylaniline did, indeed, produce a very brilliant violet colour, which, having been discovered by M. Lauth, and improved and patented by Messrs. Poirrier and Chappat, was introduced into commerce under the name of Paris violet. This achievement led to a demand for the production of the methyl-

anilines on a large scale, and, in a very short time, this was attained. It was well known that methylaniline could be produced by the action of methyl iodide, or bromide, upon aniline, the dimethyl compound resulting from the use of two molecules of the alcoholic compound; thus—



but it was evidently necessary to produce the required compound in a cheaper way. This was eventually done by heating aniline hydrochloride and methylic alcohol together, under pressure, in strong cast-iron vessels, enamelled inside, and known as "autoclaves." Various proportions of the bodies have been employed—among others, the following giving good results:—Aniline, 33.6; hydrochloric acid, 37.9; methylic alcohol pure, 28.5; heat to 250° C. for eight hours. Messrs. Poirrier have also employed a mixture of 100 parts aniline and 250 methyl nitrate. In the latter case, the mixture requires only a temperature of 100° C.; but the alcoholic nitrate is an exceedingly dangerous compound to deal with; in all probability, it was the one that led to the lamented death of Mr. E. T. Chapman, and a very disastrous and fatal explosion at Messrs. Poirrier's works was also caused by it.

Methylaniline is now largely made by the action of methyl chloride on aniline. As is well known, the former body has, of late years, been obtained in immense quantities in France, from a product of the destructive distillation of residues obtained in the manufacture of beet sugar. This body reacts upon aniline just as the corresponding iodide or bromide does; it is cheap, the reaction takes place with ease, and a remarkably pure product is produced; in fact, dimethylaniline can now be obtained by the ton, free from unaltered aniline, and containing only 3 per cent of the mono-methylated compound.

From dimethylaniline the violet is obtained by oxidation; formerly, various oxidising agents were used, among them a mixture of iodine and potassium chlorate. It is, however, now well known that very gentle oxidisers will produce the colour if a metallic salt is present, the one preferred being copper. If I heat in this tube some copper filings with a mixture of dimethylaniline and chloral hydrate, the whole will shortly become a mass of semi-solid violet. It is, however, obvious that so costly a method could not be employed on a manufacturing scale, and accordingly the following process is in very general use:—20 parts pure crystallised cupric nitrate are dissolved in 20 parts of acetic acid; some common salt is now stirred in to the mixture, which is carefully cooled down to the ordinary temperature, and 50 parts of dimethylaniline are added; the whole is then thoroughly mixed with about 250 to 300 parts of white sand, and the stiff mass thus produced is moulded into large cakes, 2 feet long by 15 inches wide, and 4 inches thick; these arranged on copper plates, are placed in a chamber, and heated to a temperature of 60° C. for forty-eight hours. At the end of that time, they have become perfectly hard and brittle, and of a bright brassy colour. They are broken into a coarse powder and thrown into water, sulphide of sodium being added until the whole of the copper-salt has been decomposed. The mass is now washed with water and extracted with dilute hydrochloric acid at a boiling temperature. After partial cooling and filtration, to remove some resinous by-products, the colouring-matter is precipitated with common salt, and, after drying, it is ready for use. The sand, which simply serves to spread the mixture over a large surface, can be used for a fresh operation.

The product thus obtained is very brilliant in colour, and in shade is that known as 3 B, dahlia, &c.; it is, however, not the bluest that can be produced. The bluest shades are made by dissolving it in alcohol, converting it into base by the cautious addition of caustic soda, and then heating the alcoholic solution of the base with benzyl chloride—a body having the formula $\text{C}_6\text{H}_5\text{CH}_2\text{Cl}$, and produced by the action of chlorine on toluene. The spirit and unaltered benzyl chloride are

recovered, and the basic colour, on conversion into the hydrochloride, is ready for use. In a similar way, by the action of methyl chloride, the well-known methyl green was produced; it is now, however, replaced by the malachite green, discovered by Oscar Doebner, and produced by the action of one molecule of benzoyl trichloride, $C_6H_5CCl_3$, on two molecules of dimethylaniline or of benzoyl-hydride, or bitter almond oil, C_6H_5COH , on the same in the presence of zinc chloride or of sulphuric acid.

The latter colour much surpasses the former in fastness and power of standing rough treatment in the dyeing process. The methyl, and, still more, the iodine green obtained during the manufacture of blue shades of Hofmann violet, were fugitive and easily altered by heat, so that the latter could not be boiled without changing to a violet.

Before leaving the methylaniline colours I may briefly allude to a question of scientific interest in connection with them. It is well known that Hofmann himself, at one time, considered the violets obtained by the methylation of rosaniline and those from methylaniline to be identical. On the other hand, there was much evidence against this view, for the methylaniline colour was readily rendered bluer in shade by benzyl chloride, which was almost without action on the Hofmann one; it was also more brilliant, had a much greater affinity for animal fabrics, and was less permanent when exposed to light. For these reasons the Hofmann colour is still in demand, and, indeed, has recovered some of the ground it at first lost to the more brilliant colour. The researches of Brunner and Brandenburg, following those of Caro and Graebe, have shown that the methylaniline violets are not identical with those obtained by Hofmann's process.

Diphenylamine Blue and Alkali Green.—The ordinary aniline blues are obtained by the action of aniline upon roseine base, in the presence of an organic acid, at a temperature which ultimately approaches the boiling-point of the aniline used. The ultimate product of this reaction is an exceedingly intense blue, the hydrochloride of which is known as opal blue, and is really the hydrochloride of triphenyl-rosaniline. This, on dry distillation, yields diphenylamine, and the latter body, on oxidation, yields a blue which is identical with that obtained from rosaniline, but somewhat greener in shade, and is therefore in demand for certain uses. The diphenylamine is prepared by heating, under pressure, a mixture of aniline and dry aniline hydrochloride, when the following reaction occurs:—



Diphenylamine is readily oxidised if heated with oxalic acid, and the resulting melt, purified from unaltered oxalic acid, diphenylamine and resinous matters, is readily converted into either of the sulphonic compounds discovered by Nicholson. Blues of a redder shade can be also obtained by the oxidation of methyl- or ethyl-diphenylamine.

I have now to call your attention to a green obtained from this body, discovered by Mr. R. Meldola, and now under his investigation. It is obtained by the oxidation of a diphenylamine derivative. After oxidation the colour is obtained in a state corresponding to the well-known opal-blue, and, like that, forms sulphonic acids. It is remarkable as being the first green obtained having this property. The sodium salt of the sulphonic acid is soluble in water, and, if wool is immersed in this solution (which is nearly colourless), and kept warm, it apparently undergoes but slight change. I have here a piece of Berlin wool which has been thus treated, and subsequently dried. You will observe that it is apparently only rather dirtier than undyed wool. When, however, I immerse it in warm water, acidulated with sulphuric acid, a brilliant green is immediately developed. The colour is remarkably fast; and since it requires exactly the same dyeing process as do the Nicholson blues for wool, one would have supposed that it would have been much liked by the dyers; but this is not at present the case. It was exhibited at the late

Paris Exhibition by the firm of Brooke, Simpson, and Spiller.

As time is getting on, I must now leave this very interesting field—the colours produced by the oxidation of the secondary and tertiary amines—after a very brief and incomplete glance at a few of them, and pass on to the consideration of a totally distinct group of colouring-matters, which are now attracting much attention in colour-chemists' laboratories, and which have already taken an important place among artificial dyes, though, as yet, the range of shades is somewhat limited. These are obtained from substances produced by the action of nitrites on amido compounds, and are known as the azo yellows, oranges, and scarlets.

The effect of nitrites on organic compounds is very various, according to the subsequent treatment they undergo; thus, if nitrous gas is passed through a solution of diphenylamine in acetic acid, a mixture of nitroso-nitro-diphenylamines results, and this, on heating with an alkali, decomposes so far as the nitroso groups are concerned, and a mixture of mono- and dinitro-diphenylamine results, which was introduced as a yellow dye by Mr. R. Meldola. A somewhat similar reaction occurs if the sulphonic acid of alpha-naphthol is similarly treated, and dinitro-naphthol may be obtained; while, if a nitrite is added to an aniline salt, and the resulting compound boiled, or if rosaniline salts are similarly treated, the whole of the nitrogen is eliminated with effervescence, and phenol in the one case, rosolic acid in the other—both of them non-nitrogenous substances—result. Before this boiling takes place, there are, however, in the two latter cases, very different bodies in solution; these bodies, which behave in the manner just mentioned on boiling, are known to chemists as "azo compounds."

In the earlier days of organic chemistry, the prefix "azo" was applied by Mitscherlich, Laurent, Zinin, and others to many bodies containing nitrogen, such as azo-benzene, $C_6H_5N=NC_6H_5$, produced by the imperfect reduction of nitro-benzene, and also to others, like the compounds obtained by Laurent by the action of ammonia on bitter almond oil and other bodies. In 1864, however, P. Griess published a magnificent memoir, in which he described a number of bodies obtained by the action of nitrous acid on aniline, and various substitution-products obtained therefrom. In this paper, he proposed that the prefix "azo" should be held to mean that the compound to which it was applied contained one atom of nitrogen occupying the place of one atom of hydrogen. This definition is now generally accepted, and thus the term "azo" has obtained a definite signification.

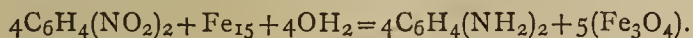
The azo compounds are, as a rule, very easily prepared; in most cases, it is only necessary to add a solution of metallic nitrite to an acid solution of a given amide, in order to obtain the diazo compound of the radicle contained in the amide; thus, if I take a solution of aniline hydrochloride, and add to it an equivalent quantity of sodium nitrite solution, the reaction at once takes place, and diazo-benzene is produced; it can be readily separated from its solution, and obtained in the solid state, one method being to add a solution of potassium dichromate, when the chlorochromate of diazo-benzene is produced. This salt when dry is terribly explosive, and at one time was suggested for use in warlike operations; however, it very rapidly decomposes when kept, losing nitrogen, and becoming no longer efficient.

This explosiveness is readily understood when we consider the constitution of the body which contains the group— $N=N$ —. It is manifestly in a state of unstable equilibrium, and a very slight disturbance is sufficient to bring about its decomposition; and, for the same reason, you will at once see that its chemical activity, as measured by its tendency to combine with other bodies, will be great; so that, if I add to it another molecule of aniline salt, or, what amounts to the same thing, if I add to two molecules of aniline only one of sodium nitrite, the diazo-benzene formed at once attacks the free aniline salt, and what is

known as diazo-amido-benzene is formed, which, in the presence of an aniline salt, becomes amido-azo-benzene, $C_6H_5N=NC_6H_4NH_2$. The oxalate of this body was once in pretty general use as a yellow dye, but, as it happened to be volatile at a very low temperature, it soon evaporated from the dyed article, and was, therefore, discarded.

A compound having a very similar constitution and mode of preparation has, however, long been in use, and is one of the most permanent of the aniline colours, under the name of "Bismarck brown." It is obtained as follows:—

Meta-dinitro-benzene is prepared by boiling ordinary nitro-benzene with nitric acid. The compound thus produced is added cautiously, with constant agitation, to coarse iron borings, kept boiling in a large quantity of water acidulated with hydrochloric acid. A violent reaction soon commences (I could readily show you the experiment, but for the steam and unpleasant odour produced); the four oxygen atoms contained in the dinitro-benzene are replaced by hydrogen, thus:—



We have thus produced diamido-benzene, and this, when purified from a little dissolved iron, is attacked with sodium nitrite solution; the reaction here is analogous to the one last described, the final product of the reaction being a triamido-azo-benzene, the hydrochloride of which constitutes the well-known colouring-matter. We have thus two terms of a possible series—first, the amido-azo-benzene (yellow, volatile, and fugitive), and triamido-azo-benzene (brown, and perfectly fast). Dr. Witt set himself the task of filling up the intermediate link, expecting an orange colour, and a moderate stability for the diamido-azo-benzene he sought. In this he was not disappointed. A study of the two compounds from a purely scientific point of view, led him to a perfectly accurate prediction; and the discovery of "chrysoidine," as the new colour was called, and its production—by the addition of diazo-benzene chloride to a solution of diamido-benzene—was one of the first of a series of researches which have in various hands enriched our science and our dyers with a number of magnificent colours.

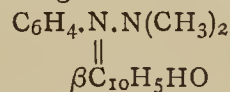
The further development of these colours commenced with the introduction of a sulphonic group into one of the amido compounds, the conversion of the sulphonic acid thus produced into a diazo body, and then using it as before; for instance, if sulphanilic acid—produced by the action of strong sulphuric acid on aniline—is converted into diazo-sulphanilic acid, and this added to a solution of diamido-benzene hydrochloride, the scarlet body produced is the sulphonic acid of chrysoidine. Again, the same body will act on resorcin to produce a colour only differing from the last in that it contains hydroxyl instead of amido groups; this body has been used as a dye, under the name of Tropæoline O. Witt also produced, by substituting diphenylamine for the resorcin, or diamido-benzene, another beautiful orange, known in commerce as Tropæoline OO. Other compounds were subsequently prepared, and have obtained greater prominence; these were those in which a naphthol was substituted for the phenolic or amido portion of the molecule, beautiful oranges being produced by the action of diazo-benzene sulphonic acid on both α - and β -naphthol; but these colours were much improved by the introduction of sulphonic groups into both portions of the molecule, α - or β -naphthol-sulphonic acid being, in fact, substituted for the naphthol only; still, so far, the improvement was in the direction of stability mainly, the shades still being yellow or orange; the red was yet to come.

Chemists will not be surprised to hear that the higher homologues of benzene are found, when converted into amido compounds, to give an increased redness of shade; thus, with a given phenol or amine, diazo-sulpho-toluydinic acid, which differs from diazo-sulphanilic acid by having an atom of hydrogen in the benzene ring replaced by

methyl, gives redder shades than does the latter, while the substitution of another hydrogen in the same way, as is the case with the diazo compound derived from sulphoxylic acid, produced a scarlet. Messrs. Meister, Lucius, and Bruning were among the first to produce a scarlet by this method, but they also introduced both the sulpho groups into one side of the molecule—that of the naphthol. In their patent they describe the preparation of two isomeric β -naphthol-disulphonic acids, the sodium salts of which are differently soluble in alcohol, the most insoluble one giving a redder colour than the other. On one or other of these they act with diazoxylene chloride, produced by the action of a nitrite on xylydine chloride. The most insoluble salt above mentioned gives a scarlet closely approaching cochineal, and perfectly fast.

Mr. R. Meldola has also taken out a patent for a scarlet, in which no less than three sulpho groups are engaged. He prepares diazo-sulphoxylic acid, and with this acts on β -naphthol disulphonic acid; on the addition of ammonia, the colour is immediately thrown down, as you perceive. This is, after a slight purification, ready for use. By certain modifications, a scarlet, closely approaching the scarlet obtained by dyeing cochineal in the presence of oxychloride of tin, is produced.

So far, we have only oranges and scarlets by these reactions. Whether other colours can be similarly produced remains to be seen; but I may mention that Mr. Meldola has recently communicated a paper to the Berlin Chemical Society, in which he describes a violet colour—unfortunately not a dye—obtained in a somewhat similar way. If we act on dimethylaniline—the body from which the violets described in the first part of my lecture are derived—with a nitrite, not an azo-, but a nitroso-dimethylaniline, is produced thus:— $C_6H_4NON(CH_3)_2$. This body is as ready to combine with others as an azo body is, and does so in a very similar way, the oxygen atom being eliminated in the process, so that if we act with it on β -naphthol the following combination takes place* :—



The oxygen of the nitroso group going off with two hydrogens from the β -naphthol, the place of which is taken by a group, which is equivalent to azo-dimethylaniline. The colour crystallises magnificently, but its dyeing powers are very feeble.

I must now conclude a very imperfect sketch of some of the recent advances of colour chemistry. If I have succeeded in showing some that practical results do come from purely scientific researches, I am content. Hard things are often said of the aniline dyes; they are looked on as poisonous and fleeting. The first accusation is untrue, properly used they are perfectly harmless. The second is only true of some few of the series, and many of the older dyes are equally evanescent, while none are so beautiful and brilliant—though this may be considered a fault, for some highly æsthetic persons look on the producer of brilliant colours almost as a criminal.

The colour chemist may contemplate all these hard sayings with an even mind. In the humble position of a scavenger, he takes some of the most noxious waste-products of our civilisation—those produced in the gas manufacture—and converts them into useful and beautiful colouring-matters. If these are improperly applied to the cloth or fabric that is not his fault; while if they are artistically used our æsthetic friends have the remedy in their own hands.—*Journal of the Society of Arts.*

Theory of Inconstant Galvanic Elements.—F. Exner. —The author shows the non-existence of a so-called galvanic polarisation in the elements, the phenomena in question being reducible to the action of the oxygen dissolved in the water.—*Wiener Anzeiger*, 1879, 292.

* This formula is given under reserve, as a complete investigation of the compound has not yet been published.

CORRESPONDENCE.

DEPHOSPHORISATION OF IRON.

To the Editor of the Chemical News.

SIR,—For many years it has been considered that crude iron should contain about 2 per cent of silicon in order to be suitable for the Bessemer process. In a letter which appeared in the *Times* of the 10th June, 1879, I endeavoured to point out that comparatively non-silicious iron could be more readily dephosphorised than iron containing a larger percentage of silicon, and I am pleased to find, from the papers read and the discussions that took place at the last meeting of the Iron and Steel Institute, that this is now an admitted fact. It is, however, phosphorus, not silicon, that is now thought to be a necessity in order to produce the requisite heat for the operation, and I venture to submit the following figures in order to demonstrate that this also is erroneous, and that there is no gain in available heat by increasing the percentage of phosphorus in the crude iron to be Bessemerised. A larger proportion of phosphorus means more air required for oxidation, therefore more blast and a slower operation, besides necessitating an additional quantity of basic material, with its attendant increased yield in slag as compared to the product of steel, and more wear and tear of the basic lining.

In Mr. R. Pink's paper, read last week before the Iron and Steel Institute, on "The Dephosphorisation of Iron in the Bessemer Converter," will be found the following analyses:—

ANALYSES OF METAL (Charge 67), composed of 2400 lbs. foundry No. 3, 3000 lbs. white forge, 1000 lbs. steel scrap.

	P	C	S	Mn	Si
As taken from cupola ..	1.04	2.58	0.22	1.35	1.08
After blowing 11.75 mins.					
on fall of carbon lines	0.82	0.08	0.19	0.39	0.09
At overblow of 100 secs.	0.08	0.06	0.15	0.39	0.007
Further blow of 15 secs.	0.045	0.04	0.14	0.37	0.005
After addition of 420 lbs. of spiegeleisen containing 17 per cent Mn ..	0.06	0.28	0.067	0.46	0.002

(6820 lbs. total weight of raw material; 6074 lbs. weight of steel produced.)

ANALYSIS OF SLAG (Charge 67).

Silica	9.50 per cent.
Phosphoric acid	9.76 "
Magnetic oxide, Fe ₃ O ₄	9.28 "
Peroxide of iron	— "
Oxide of manganese, Mn ₂ O ₄	6.16 "
Lime	59.35 "
Magnesia	5.01 "
Sulphate of Lime	0.87 "
Alumina	trace

99.93 "

The weight of the slag is not given, but calculated from the percentage of phosphoric acid in the slag formed it is equivalent to 23 per cent of the weight of crude iron operated on.

9.76 per cent : 100 : 0.02245 : 0.23.

On these analyses I have based the following calculations, which I think bear out the opinion I have above expressed:—

1.0 Crude iron × 0.1138 sp. heat of Fe (Regnault) × 1650° C.	{ Temperature at which it is assumed the iron is introduced into the Bessemer.	Heat Units. 187.7
0.0098 P burning with 0.01265 O = 0.02245 P ₂ O ₅ ∴ 0.0098 P × 5708 calorific value of P (mean of Andrews and Abria)		= 55.9
0.0254 C	0.03387 O = 0.05927 CO ∴ 0.0254 C × 2473 cal. val. of C (Favre and Silbermann) ..	= 62.8
0.0008 S	0.0008 O = 0.0016 SO ₂ ∴ 0.0008 S × 2220 cal. val. of S (Favre and Silbermann) ..	= 1.8
0.0098 Mn	0.00386 O = 0.01366 Mn ₂ O ₄ ∴ 0.0098 Mn × 1639 cal. val. of Mn (assumed to be the same as that of Fe)	= 16.1
0.0103 Si	0.01178 O = 0.02208 SiO ₂ ∴ 0.0103 Si × 7830 cal. val. of Si (Troost and Hautefeuille) ..	= 80.6
0.0168 Fe	0.0064 O = 0.0232 Fe ₃ O ₄ ∴ 0.0168 Fe × 1639 cal. val. of Fe (mean of Andrews and Dulong)	= 27.5
0.0729	0.06936	0.14226
0.06936 O × 76.3		
23.7	= 0.22329 N = 0.29265 air × 0.2375 sp. heat of air (Regnault) × 50° C.	{ Temperature at which the air is blown in } = 3.5
0.0254 C burning with 0.03387 O = 0.05927 CO × 0.245 sp. heat of CO (Regnault) = 0.0145		
0.0008 S	0.0008 O = 0.0016 SO ₂ × 0.154 sp. heat of SO ₂ (Regnault) = 0.0002	
0.22329 N	0.224 sp. heat of N (Regnault) = 0.0545	

Temperature at which it is assumed the gases escape, 1500° C. × 0.0692 = 103.8

Heat units 332.1

0.0098 P burning with 0.01265 O = 0.02245 P ₂ O ₅ + 0.02481 CaO = 0.04726 Ca ₃ P ₂ O ₈ × 0.205 sp. heat of Ca ₃ P ₂ O ₈ = 0.0097	
0.0098 Mn	0.00386 O = 0.01366 Mn ₂ O ₄ × 0.17 Mn ₂ O ₄ = 0.0023
0.0103 Si	0.01178 O = 0.02208 SiO ₂ × 0.23 SiO ₂ = 0.0051
0.0168 Fe	0.0064 O = 0.0232 Fe ₃ O ₄ × 0.17 Fe ₃ O ₄ = 0.0039
59.35 per cent CaO × 0.23 slag = 0.1365 CaO — 0.2481 CaO =	0.1117 CaO × 0.202 CaO = 0.0226
5.01 MgO × 0.23 slag =	0.0115 MgO × 0.266 MgO = 0.0031
0.87 CaSO ₄ × 0.23 slag =	0.002 CaSO ₄ × 0.2 CaSO ₄ = 0.0004

0.2314 slag, i.e., 23 p. c. slag per ton of metal.

1.0 Crude iron = 0.11 loss by oxidation, &c. =

0.89 Fe × 0.1138 sp. heat of Fe = 0.1013

0.1484

$\frac{332.1}{0.1484} = 2238^{\circ}$ centigrade.

From the foregoing it will be seen that—

$$\begin{aligned} 0.0098 \text{ P burning with } 0.01265 \text{ O} &= 0.02245 \text{ P}_2\text{O}_5 \\ \therefore 0.0098 \text{ P} \times 5708 \text{ calorific value of P} &= 55.9 \\ 0.01265 \text{ O} \times 76.3 &= 0.04072 \text{ N} \times 0.244 \text{ sp. ht. of N} \times \\ 23.7 & \\ \times 1500^\circ \text{ C. temperature, at which it is assumed} & \\ \text{the gases escape} &= 14.9 \\ &41.0 \end{aligned}$$

$$\begin{aligned} 0.02245 \text{ P}_2\text{O}_5 + 0.02481 \text{ CaO} &= 0.04726 \text{ Ca}_3\text{P}_2\text{O}_8 \times \\ \times 0.205 \text{ sp. heat of Ca}_3\text{P}_2\text{O}_8 &= 0.0097 \\ 5 \text{ per cent additional lime added for each 1 per cent} & \\ \text{of P contained in the crude iron } 0.05 \text{ CaO} \times 0.202 & \\ \text{sp. heat of CaO} &= 0.0101 \\ &0.0198 \end{aligned}$$

$$\frac{41.0}{0.0198} = 2071^\circ \text{ Centigrade.}$$

Although as shown by the above figures the oxidation of phosphorus will not supply the additional amount of heat that may be requisite, this can easily be obtained by burning the CO to CO₂ in a converter specially constructed for that purpose.

RICKMAN AND THOMPSON'S AMMONIA PROCESS.

To the Editor of the Chemical News.

SIR,—In reply to the enquiry in the CHEMICAL NEWS, vol. xli., p. 220, I beg to say that any correspondence on this matter should be addressed as follows:—Messrs. Rickman and Thompson, care of Messrs. Pellatt and Co., Falcon Glass Works, 17, St. Bride Street, E.C.—I am, &c.,

JAS. PELLATT RICKMAN.

CARBON IN STEEL.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 152, Mr. James W. Westmoreland, in an article on "The Estimation of Carbon in Steel," mentions as one of the objections to the direct combustion process, advocated by Mr. W. D. Herman, "the uncertainty as to the completeness of the reaction." Now, this is precisely one of the points in which the process proposed by Mr. Herman has an advantage over other methods of combustion formerly employed, as he points out in his paper describing the process (*Journal*

$$\begin{aligned} 0.0254 \text{ C burning with } 0.06774 \text{ O} &= 0.09314 \text{ CO}_2 \therefore 0.0254 \text{ C} \times 8080 \text{ calorific value of C} + \text{O}_2 &= 205.2 \\ 0.06774 \times 76.3 &= 0.21808 \text{ N} \times 0.244 \text{ sp. heat of N} &= 0.05322 \\ 23.7 & \\ 0.09314 \text{ CO}_2 \times 0.216 & \text{,,} \text{ CO}_2 = 0.0201 \end{aligned}$$

$$0.7332 \times 1500^\circ \text{ C., temperature at which it is assumed}$$

$$\text{gases escape} = 110.0$$

$$95.2$$

$$0.0254 \text{ C burning with } 0.03387 \text{ O} = 0.05927 \text{ CO} \therefore 0.254 \text{ C} \times 2473 \text{ calorific value of C} + \text{O} = 62.8$$

$$0.05927 \text{ CO} \times 0.245 \text{ sp. heat of CO} = 0.0145$$

$$0.03387 \text{ O} \times 76.3 = 0.10904 \text{ N} \times 0.244 \text{ sp. heat of N} =$$

$$0.0266$$

$$0.0411 \times 1500^\circ \text{ C.} = 61.6$$

$$1.2$$

$$\text{Heat units } 94.0$$

$$\frac{332.1 + 94.0}{0.1484} = \frac{426.1}{0.1484} = 2871^\circ \text{ C.}$$

This figure of 2871° C., however, is far too high, because it is not probable that complete combustion of the CO to CO₂ would take place, and it is certain that the escaping gases would carry away more heat than has been calculated. Without doubt, however, if the CO were burnt to CO₂ sufficient heat would be obtained to permit of white iron containing neither silicon nor phosphorus being Bessemerised.—I am, &c.,

JOHN HOLLWAY.

London, May 11, 1880.

PS.—The heat rendered latent in the melting of the slag has been ignored for want of the requisite data.

AMMONIA FROM ATMOSPHERIC NITROGEN.

To the Editor of the Chemical News.

SIR,—It may interest your correspondent "J. H." and others to know that Messrs. Rickman and Thompson's patent for the above process has been anticipated by Clarke and Smith's patent—No. 4650 for 1878; filed November 16, 1878; sealed February 7, 1879, a copy of which I beg to enclose.—I am, &c.,

E. S.

May 11, 1880.

Chem. Soc. [2], viii., 375), that the oxide of iron formed by the combustion has invariably the composition of Fe₂O₃, and agrees exactly with the amount calculated from the steel employed, after deducting the carbon found. Of course, when it is ascertained that the whole of the steel has been converted into Fe₂O₃, there can be no doubt that the combustion of the carbon is also complete, and Mr. Westmoreland's objection as to the uncertainty of the completeness of the reaction falls to the ground.—I am, &c.,

GEORGE E. DAVIS.

Royal School of Mines.—Mr. W. Chandler Roberts, F.R.S., Chemist to the Mint, has been appointed to the Lectureship of Metallurgy in the Royal School of Mines, rendered vacant by the resignation of Dr. Percy, F.R.S. Mr. Roberts will continue to hold his appointment at the Mint. Mr. Richard Smith, hitherto assistant metallurgist has been appointed instructor in assaying.

Researches on Picrotoxin.—L. Barth and M. Kretschy.—Picrotoxin is a mixture of three compounds; true picrotoxin, C₁₅H₁₆O₆, very bitter and highly poisonous; picrotin, C₂₅H₃₀O₁₂, bitter, but not poisonous; and anamartin, present only in small proportion, and neither bitter nor poisonous.—*Wein. Anzeiger*, 1880, 2.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Chemisches Central-blatt.
No. 7, 1880.

New Method for Recognising the Optical Difference of Transparent Bodies.—V. Dvorák.—The author describes a new method and a simple apparatus by which the differences of transparent bodies due to the inequality of the indices of refraction may be distinguished. The method has certain advantages as compared with that of Toepler (*Pogg. Annalen*, 131, 33), and the apparatus is extremely sensitive.—*Casopis pro Pěstování Mathem. a Fysiky*, ix., 23 to 31, Agram.

Contributions to Quantitative Spectral Analysis.—C. von Norden.—This memoir treats of a simplification introduced into Hüfner's spectro-photometer; on the determination of the degree of concentration of a normal solution, and certain observations with oxyhæmoglobin from the blood of dogs, guinea-pigs, and rats.

Examination of "Ferrum Hydrogenio Reductum."—G. Vulpius.—*Arch. Pharm.*, xv., 508, Dec., 1879.

Volhard's Volumetric Determination of Silver.—H. v. Jüptner.—The author finds the results of this process accurate to 1 part in 2000.—*Oest. Zeit.*, xxviii., 33.

Erroneous Test for Kirshwasser.—G. A. Berchemmann.—The blue colouration produced with guaiacum wood is merely due to the presence of traces of copper derived from the still, and throws no light upon quality.—*Arch. Pharm.*, xv., 517.

Analysis of Milk.—N. Gerber and P. Radenhausen.—The authors consider that the measurement of milk does not give accurate results, and that for the determination of total solids it is better to dry up after coagulation with alcohol or acetic acid than to dry along with sand. The determination of the albuminates is best effected by coagulation with copper sulphate, neutralisation with soda-lye, and removal of fatty matter from the coagulum. They determine milk-sugar by titration with Fehling's solution. Feser's lactoscope gives unsatisfactory results.—*Schweitz. Zeit. f. Pharm.*, 1879, 37.

No. 8, 1880.

The Reducing Properties of Potassic Ferri-oxalate.—J. M. Eder.—This compound is prepared by mixing a concentrated solution of ferrous sulphate with a neutral concentrated solution of potassic oxalate till the precipitate first formed re-dissolves to a clear deep red liquid. The solution acts more energetically than all other acid or neutral ferrous salts, and in its action approaches the alkaline pyrogallates.—*Wiener Anzeiger*, 1880, 15.

No. 9, 1880.

The Acid Reaction of Cryolite.—F. Stolba.—The author finds that powdered cryolite reddens moist litmus paper.—*Listy Chem.*, iv., 67.

No. 10, 1880.

The Source of the Force Exciting Electricity.—Every heat-phenomenon, emission as well as absorption, occasions under favourable circumstances an electric current. The current produced by the emission of heat has the opposite direction from that produced by absorption. If only one metal in a galvanic element is active, the electric force is proportional to the algebraic sum of the heat developed by the bodies acting upon each other within the element. If both metals are active the electric force is proportional to the difference of the algebraic heat-sums on the one and the other side. The power of polarisation in exciting electricity depends neither on the nature of the gas nor of the metal, but mainly on the chemical action springing from electrolysis. The power of two metals in one acid to produce electricity stands in a simple proportion to the heat which the metals in question evolve when they unite with the acid to form salts.—*Rad. Jugosl. Ak.*, L. 1 to 31.

Morphine Hydrochlorate.—H. Tausch.—Commercial morphine hydrochlorate if heated to 130° turns brown and undergoes a partial decomposition.—*Wiener Anzeiger*, 1880, 20.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 11, March 11, 1880.

This issue is chiefly taken up with an account of the prizes awarded by the Academy of Sciences.

NOTES AND QUERIES.

Tension of Dissociation.—Can any of your readers inform me what meaning Deville attaches to the term "tension of dissociation"?—H. G.

Sulphur in Coal.—In reference to Dr. Wallace's observation as to the existence of sulphur in coal more than sufficient to form FeS₂, it is stated by Wittstein in *Archiv. d. Pharm.*, 1876, that sulphur is generally present in coal in greater quantity than is necessary for the formation of pyrites.—F. I. C.

Blue-black Writing and Copying Ink for Hot Climates.—(Answer to "South Africa.")—The following prescription will make a good ink:—Aleppo galls, 4½ ozs.; bruised cloves, 1 drachm; cold water, 2 pints; best copperas, 1½ ozs.; pure sulphuric acid, 35 minims.; sulphate of indigo, as neutral as possible, ¼ oz. Digest the galls and cloves for a fortnight in the water, shaking frequently; filter through paper, add the copperas to the filtrate, and, when dissolved, the other ingredients.

MEETINGS FOR THE WEEK.

- TUESDAY, 18th.—Civil Engineers, 8.
— Pathological, 8.30.
— Royal Institution, 3. Mr. Fiske, "American Political Ideas."
— Society of Arts, 8. Rev. James Sibree, Jun., F.R.G.S., "The Arts, Commerce, Recent Advances, and Future Prospects of the Island of Madagascar."
WEDNESDAY, 19th.—Society of Arts, 8. Lieut. G. T. Temple, "Trade Routes between England, Norway, and Siberia."
— Meteorological, 7.
THURSDAY, 20th.—Royal Institution, 3. Mr. T. W. R. Davids, "Buddhist Sacred Books."
— Pharmaceutical (Anniversary), 8.
— Society of Arts, 8. Capt. Abney, R.E., F.R.S., "Some Recent Advances in the Science of Photography."
— Chemical, 8. Miss Lucy Holcrow and Dr. E. Frankland, "On the Oxidation of Peaty Matters." Discussion on Dr. Tidy's Paper on "River Water."
FRIDAY, 21st.—Royal Institution, 8. Mr. W. Spottiswoode, "Electricity in Transitu," 9.
— Society of Arts, 8. A. Terrien de la Couperie, "China and the Chinese: Their Early History and Future Prospects."
SATURDAY, 22nd.—Royal Institution. Prof. Morley, "The Dramatists before Shakespeare," 3.
— Physical, 3.

TO CHEMICAL MANUFACTURERS AND PARTNERSHIP AGENTS.

RE JOHN GELLATLY, IN LIQUIDATION.

TO BE SOLD as a Going Concern, by Private Treaty, the whole of the Stock, Plant, Utensils, Goodwill, and Possession of Hawkesbury Lane Chemical Works, near Coventry, lately carried on by Messrs. Gellatly and Farr. Or an advantageous partnership may be arranged with the present solvent Partner. Full particulars on application to the undersigned, at his Offices, 30, Hertford Street, Coventry.

CHARLES F. BARNARD, Trustee.

THE CHEMICAL NEWS.

VOL. XLI. No. 1069.

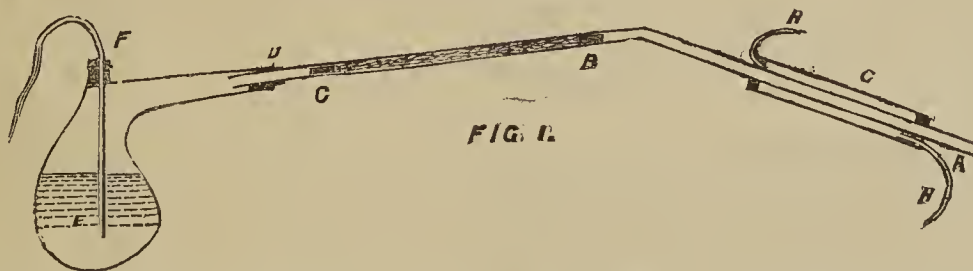
ON THE PRESENCE OF NITROGEN IN IRON AND STEEL.*

By ALFRED H. ALLEN, F.C.S.

ALTHOUGH numerous chemists have investigated the vexed question of the presence of nitrogen in commercial iron and steel, their results have been very contradictory, and the problem cannot be said to have hitherto received a definite solution.

As tests for or modes of determining nitrogen in iron and steel the following principles have been utilised:—

1. The metal has been ignited with soda-lime or potash-baryta, the object being to convert the nitrogen into ammonia. (Schafhäutl, Marchand.)
2. The metal has been heated to redness in a current of hydrogen, with the object of converting the nitrogen into ammonia. (Fremy, Stuart and Baker.)
3. The metal has been dissolved in acid, and the resultant ammonia distilled off after addition of an alkali. (Boussingault, Bouis.)
4. The metal has been converted into oxide by igniting it *in vacuo* with oxide of copper, the liberated nitrogen being measured as gas. (Schafhäutl, Marchand.)
5. The metal has been ignited in admixture with native sulphide of mercury, and the liberated nitrogen gas measured. (Boussingault.)
6. The metal has been ignited with potassium or sodium, whereby a cyanide was formed which admitted of easy and certain recognition. (Marchand.)



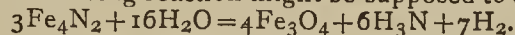
In 1865 Stahlschmidt prepared a definite nitride of the formula N_2Fe_4 , and concluded that nitride of iron existed in an irregular state of distribution in commercial steel.

Since 1865 I am not aware that the question of the presence of nitrogen in steel has been investigated from a chemical point of view, though the matter has been touched upon by Mr. John Parry, who, in a research on the gases occluded in iron and steel, incidentally found that nitrogen was not present to any appreciable extent in a specimen of grey cast-iron.

Among the most promising and convenient of the methods of searching for nitrogen in steel are those based on its combination with hydrogen, and consequent conversion into ammonia. As, however, the tendency of hydrogen to combine with nitrogen is not very great, and is greatly diminished at a red-heat, it is desirable to arrange the experiment so as to reduce these disadvantages to a minimum. This was not done by those chemists who sought for nitrogen by heating the steel to redness in a current of hydrogen; for, apart from the limited tendency to form ammonia under such conditions, the temperature employed was such as might not improbably lead to the establishment of a reversed action, and the consequent decomposition of any ammonia previously found.

* A Paper read before the Chemical Society. Communicated by the Author.

With a view of avoiding these difficulties it occurred to me that the tendency of any nitrogen in the steel to form ammonia would be much increased by presenting the hydrogen to it in a nascent state, and in a neutral atmosphere which should rapidly remove the products from the sphere of action. These conditions are exactly fulfilled if the hydrogen be obtained in a nascent state by heating the steel to be tested for nitrogen in a current of steam, when the following reaction might be supposed to occur:—



This reaction appeared to possess the additional advantages of requiring only one reagent (vapour of water), of allowing a complete expulsion of the air from the apparatus, and of affording a very convenient means of collecting any ammonia formed.

In accordance with these views I have made a series of experiments with the following arrangement of apparatus:—

A, B, C (Fig. 1) is a combustion-tube about seven feet in length. It is thoroughly cleaned; a plug of ignited platinum gauze is inserted at B, and the part of the tube from B to C is filled with the material to be operated upon, in as fine a state of division as possible. Another platinum gauze plug is inserted at C. At D, the tube is joined by a sound cork to the large retort E, containing water to which a few drops of hydrochloric acid and fragments of steel borings have been added. The object in making these additions is to retain any trace of ammonia in the water, and to facilitate the expulsion of the last traces of air by causing a slight evolution of hydrogen. The tubulure of the retort is fitted with a cork carrying a bent glass tube, F, tightly closed at the farther end by a piece of elastic tubing and a screw clip. Before heating the water in the retort this tube is entirely emptied of air by loosening the clip and sucking up the water till filled with it. A little beyond B the combustion tube is bent, and surrounded at its further extremity by the Liebig's condenser, G, through which a rapid current of cold water can be maintained by means of the tubes H H, connected with the source of supply.

A number of experiments have been made with this apparatus, but the following are those of the greatest interest and importance:—

Action at a Red-heat of Vapour of Water on Bessemer Steel.—From 50 to 200 grms. of Bessemer steel have been placed in the part of the tube from B to C. In every case the borings were first heated to dull redness in a muffle, to get rid of any trace of grease or ammoniacal salts. The retort has next been attached, and the water in it caused to boil vigorously by a lamp placed underneath. As soon as the whole apparatus became hot, and uncondensed steam was issuing freely from the open end of the tube at A, cold water was allowed to circulate through G, and the condensed water collected in glass cylinders. Notwithstanding all the precautions taken to prevent the accidental introduction of traces of ammonia, the first portions of the distillate have usually shown distinct traces of colour when examined by Nessler's method.* This, of course, is an exceedingly delicate test, but I have always continued the operation till the condensed water shows no yellow colour in addition of the reagent, and is consequently free from even the faintest trace of ammonia, or any substance

* I do not think this colour has always been due to the presence of ammonia as an impurity. From the experiment with artificial nitride of iron, it is not improbable that the ammonia is actually produced by the incipient action of the steam at 100° C.

capable of simulating it. On reaching this point the operation has been in progress a considerable time, so that every trace of atmospheric air must have been expelled from the retort and posterior part of the tube by the continuous and rapid current of steam transmitted. The portion of the tube containing the steel borings has next been heated to redness by a Hofmann's combustion-furnace placed outside it. On reaching an incipient red-heat a formation of ammonia at once occurs, and the condensed water gives a deep brown colour with Nessler's test. This effect soon reaches a maximum, and ammonia continues to be found in the distillate as long as the oxidation of the iron continues, whereas, if it were due to occluded nitrogen, or to an unexpelled trace of atmospheric air, it would be anticipated that the formation would be of a very temporary character. As a fact, I have never succeeded in oxidising the steel borings throughout (though I have continued the current of steam for as long a time as six hours), and as a consequence have never got to the end of the production of ammonia.

In addition to the reaction with Nessler's reagent, the condensed steam has been proved to contain ammonia by the following absolutely conclusive reactions:—

1. On re-distilling the condensed water the first portions have a distinct smell of ammonia, and marked alkaline reaction to litmus-paper. 2. On acidifying the re-distilled liquid with hydrochloric acid, and evaporating it to dryness in platinum with platinic chloride, a sensible amount of yellow precipitate is obtained, having the characteristic crystalline form of ammonium chloroplatinate.

I produce a tube containing 0.1185 grm. of the yellow salt. It was produced from Bessemer-steel borings, and contains 0.0072 grm. of nitrogen. This amount is as great as that contained in 7.2 cubic centimetres, or 0.44 cubic inches of air.

I need scarcely add that the accidental retention of so large a volume of air in the apparatus is perfectly inconceivable; and even if present, only a minute fraction of its nitrogen would be likely to undergo conversion in ammonia.

I regard the experiment last described as absolutely conclusive as to the presence of a small proportion of nitrogen in the steel operated on. It will be observed that steam was the only reagent employed in the operation of producing the ammonia.

Freedom from Nitrogen of Iron reduced by Hydrogen.—

The apparatus already described was in one case arranged in the usual way, but the portion of the tube from B to C was filled with recently ignited, finely granular, red hæmatite iron ore. A current of steam was passed through the apparatus, and the hæmatite heated to redness. No trace of ammonia was found in the condensed water. The tube F was then attached to an apparatus in which hydrogen was evolved by the action of Bessemer-steel on hydrochloric acid, the gas being perfectly free from any trace of ammonia or atmospheric air. As soon as the hydrogen was found to be escaping freely from K, the water in the retort was allowed to cool down. The current of hydrogen was passed continuously over the red-hot hæmatite until the latter was supposed to have suffered complete reduction to the metallic state; the water in the retort was again boiled, and, when it commenced to distil, the current of hydrogen was arrested, and the reduced iron re-oxidised by the steam. The condensed water from K contained no trace of ammonia when examined by Nessler's test. The negative result is important, for while proving the freedom from nitrogen of the iron reduced from the oxide by hydrogen, it shows that the ammonia obtained from the Bessemer steel was not a constant accompaniment of the operation, and hence was really due to the presence of nitrogen in the steel, and not to some accidental source.

Nitride of Iron.—With a view of ascertaining the behaviour of undoubtedly nitrogenised iron when treated in the apparatus, I prepared a quantity of nitride of iron by heating to redness a quantity of very thin soft iron wire in dry ammonia gas. The product was of a dark grey

colour and extremely brittle. It contained about 2½ per cent of nitrogen, and evolved abundance of ammonia when heated with slaked lime.

Some of the substance was placed in water acidulated with hydrochloric acid, well washed, heated to dull redness in the air, and then placed in the apparatus. On passing a current of steam over it at 100° C., the condensed water contained a sensible quantity of ammonia which did not apparently diminish on continuing the experiment for some hours. On heating the nitride of iron to redness (the current of steam being continued), a large quantity of ammonia was formed, and the production continued regularly for four hours, when the experiment was arrested. The formation of ammonia at 100° C. is doubtless due to the incipient decomposition of nitride of iron when heated at that temperature in a rapid current of steam.*

Formation of Ammonia by the Solution of Steel in Acid.

—The solution of steel in hydrochloric acid affords the most convenient and satisfactory means of converting the contained nitrogen into ammonia. The method was originally employed by Boussingault, but since the date of his researches analytical chemistry has been enriched by the discovery of the extraordinarily delicate reagent for ammonia known as "Nessler's test," and hence the examination of steel and iron for nitrogen, and the determination of the amount present, can now be satisfactorily carried out on as small a quantity as one gramme of the metal—a circumstance which has greatly facilitated the execution of the somewhat extensive series of experiments about to be described.

One grm. of the sample of iron or steel to be examined is weighed out and heated to incipient redness in a porcelain crucible. While still hot it is tipped from the crucible into the flask A (Fig. 2), which contains a small quantity

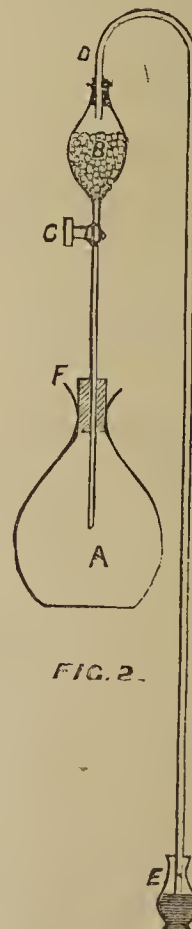


FIG. 2.

of ammonia-free distilled water. The cork F is next inserted, and with it the tapped bulb and tube B C. B contains a number of recently ignited glass beads. The water in the flask is then boiled for some minutes, till steam

* Stahlschmidt found that nitride of iron was gradually decomposed by boiling with water.

issues copiously from the open top of B and the air is judged to have been completely expelled. The tap c is then closed, and the lamp removed from under A. As the cooling proceeds the flask becomes vacuous through condensation of the steam, and any trace of air contained in the pores of the cork or elsewhere is got rid of by subjecting the water in the flask to a second boiling, followed by closure of the tap.

20 c.c. of hydrochloric acid of 1.11 sp. gr. are next poured into the bulb B, and the bent tube and india rubber cork D inserted. The tube is at least 3 feet in vertical height, and the lower end is closed by mercury. The acid in B is next boiled until all the air is expelled through the mercury, when the tap c is opened and the acid runs into the flask. By operating in this manner the metal may be dissolved without admitting the smallest quantity of air or the loss of the least trace of the ammonia formed. I may say, however, that experiment has conclusively proved that the presence of air during the solution of the metal does not cause any formation of ammonia, so that, as far as the accuracy of the results are concerned, the foregoing precautions prove to have been unnecessary. The employment of the glass beads is, of course, to prevent the escape of any traces of ammonia together with the hydrogen. To facilitate the solution of the metal, heat is applied to the flask.

When the solution of the metal is complete, the source of heat is withdrawn and the tap closed. The long tube is removed, water poured in the bulb, and allowed to enter

the flask by opening the tap. The contents of the flask are then transferred to a larger flask or retort adapted to a Liebig's condenser, an excess of freshly-burned finely-powdered quicklime added, and the liquid distilled. In the distillate, the ammonia is determined by Nessler's reagent, in the same manner as in a water analysis.

Throughout the experiments made by this method, the most scrupulous care has been taken to avoid the introduction of ammonia as an impurity in either of the reagents. The distilled water was proved not to evolve any trace of ammonia when re-distilled either with or without lime. The lime itself was freshly made by re-igniting slaked lime in a muffle. The hydrochloric acid was specially prepared by passing the gas evolved on heating common salt with sulphuric acid into ammonia-free distilled water. It was also proved to be free from nitrous compounds and free chlorine. As a direct proof of the freedom of the reagents from contamination by ammonia, blank experiments were made on them at intervals throughout the series of experiments. A correction for the minute proportion of ammonia thus found has been made in all the results recorded below. But the best and most conclusive proof of the freedom of the reagents from ammonia and the harmlessness of the presence of air is to be found in the fact that zinc, whether in the absence or the presence of air, gave *absolutely no ammonia* on solution in hydrochloric acid.

The following numerical results have been obtained by the solution method:—

NITROGEN FOUND IN SAMPLES OF COMMERCIAL METALS.

N.B.—The determinations of nitrogen in the last column were obtained with free access of air. In all other cases the atmospheric air was rigidly excluded.

Nature of Sample:	Remarks on Sample.	Combined Carbon per cent.	Nitrogen per cent.	
			All Air excluded.	Air present.
SOFT IRONS—				
Thin iron wire	Matters not iron=0.05 per cent ..	None	0.0123	0.0098
Swedish iron	Matters not iron=0.13 per cent ..	None	0.0111; 0.0094	—
Wrought iron armour-plate ..	J. Brown and Co.	Traces	0.0131	0.0148
STEELS—				
Siemens-Martin steel	0.22	0.0107	—
Blister steel from Swedish iron..	W. Jessop and Sons	0.35	0.0148; 0.0131	0.0143
Thomas-Gilchrist steel	Brown, Bayley, and Dixon	0.39	0.0115	0.0115
Bessemer steel	Chas. Cammell and Co.	0.50	0.0164	0.0156
Siemens-Martin steel	Chas. Cammell and Co.	0.60	0.0098	0.0098
Pianoforte wire	0.62	0.0094	—
Double-shear steel	W. Jessop and Sons	1.05	0.0139	—
Crucible steel	W. Jessop and Sons	1.09	0.0082; 0.0080	0.0063
Blister steel from Swedish iron..	William Hall	1.25	0.0156; 0.0156; 0.0152	0.0152
Steel from Dannemora iron ..	B. Huntsman	1.30	0.0172	—
PIG IRONS—				
Chrome pig iron	Contains much graphite and 0.45 per cent of chromium.. .. .	Traces	0.0098; 0.0094	0.0078
“Titanic plate” iron	J. Brown and Co.	1.90	0.0049	0.0043
White iron	0.15 per cent phosphorus; 0.31 per cent manganese	3.10	0.0074	—
Spiegeleisen	20 per cent of manganese	3.80	0.0041	0.0041
VARIOUS METALS—				
Aluminium foil	—	None	—
Distilled zinc.. .. .	Free from lead and iron	—	None	None
Sheet nickel	Pure; Fleitmann and Witte	—	—	None
Magnesium ribbon	—	0.0020	—
Sodium	—	—	0.0011

From an examination of this table it will be seen that the experiments made in presence of air give results agreeing with those in which every precaution was taken to prevent the presence of air. I cannot say that I am personally surprised at this result, as I could see no reasons to suppose that nascent hydrogen would combine with atmospheric nitrogen below 100° C., whatever might be the case at more elevated temperatures.

Another interesting point is the fact that the samples of commercial aluminium, zinc, and nickel examined contained no trace of nitrogen, while in the magnesium and sodium* the proportions found were far less than those met with in iron and steel. No very apparent connection

* From information communicated by Dr. Carnelly I believe some samples of commercial sodium contain a notable proportion of nitrogen.

exists between the proportion of nitrogen found in iron and steel and the percentages of combined carbon, and the experiments show that the presence of nitrogen is by no means peculiar to *steel*, as distinguished from cast- or wrought-iron.

So far, I have made no attempt to establish any relation between the proportion of nitrogen present in steel and the physical and mechanical characters of the metal, and the fact that the amounts of nitrogen hitherto found have been invariably less than 1 part in 5000 suggests the probability that the exact proportion of that element is a matter of but little importance. Nevertheless, the solution method now affords a rapid and accurate means of examining steel and iron for the amount of nitrogen contained in them, and it is probable that in certain cases the determination may prove of considerable value. Owing to the difficulty which has hitherto existed of detecting nitrogen, metallurgists have been in the habit of ignoring its probable presence in iron and steel, but there are a number of curious facts which go to indicate that nitrogen occasionally exists in steel to a very sensible extent, and the properties of nitride of iron render it probable that any abnormal proportion of nitrogen would have a very decided influence on the physical and mechanical characters of the metal. As an instance of the presence of an unusual amount of nitrogen in some form I may mention the fact observed by P. Regnard, that the freshly fractured surface of certain steel melted in the Ponsard furnace evolved an unmistakable odour of ammonia. Notwithstanding some attempts, I have not hitherto succeeded in obtaining specimens of such steels as might be suspected to contain an abnormal proportion of nitrogen, but should strongly recommend metallurgical chemists to make a determination of the nitrogen in all cases in which the mechanical characters of steel are at variance with the indications of the chemical analysis as ordinarily made.

The following conclusions are deducible from the experimental results described in the foregoing paper.

1. Commercial irons and steels contain nitrogen in such a form as to undergo conversion into ammonia by the action of steam or hydrochloric acid.
2. The solution method indicates a proportion of nitrogen in commercial iron and steel, varying from 0.0041 per cent in spiegeleisen to 0.0172 per cent in steel from Dannemora iron.
3. The solution process of estimating nitrogen gives closely concordant results on repetition.
4. The results obtained by the solution process are not affected by dissolving the metal with free access of air.
5. No nitrogen was present in the specimens of commercial aluminium, zinc, and nickel examined by the solution method, and but very small proportions were found in magnesium and sodium. Hence iron is exceptional in the proportion of nitrogen contained in it.
6. The iron reduced from the oxide by hydrogen differs from all varieties of the commercial metal by containing no trace of nitrogen.
7. No connection is distinctly traceable between the proportion of nitrogen in commercial iron and steel and that of carbon or other foreign elements.
8. There is no positive evidence as to its condition, but the probability is somewhat in favour of its existence as nitride of iron.

In conclusion I beg to express my indebtedness to Mr. Leonard Archbutt and Mr. Charles Harrison for their assistance in the experimental part of the investigation described in this paper.

Technology of the Mint.—Hermann Danert.—The copper used as an alloy must be exceedingly pure. The author gives information concerning the arrangement of furnaces and the choice of crucibles.—*Berg. und Hutten Zeitung*.

ON THE SEPARATION OF SILICA IN THE ANALYSIS OF LIMESTONES, IRON ORES, AND OTHER MINERALS.*

By H. ROCHOLL, F.I.C., Clarence Iron Works, Middlesbough.

THE separation of silica is usually the first step in analysing minerals containing it. If possible, the mineral is decomposed by hydrochloric acid, evaporated to dryness, and then treated with hydrochloric acid and water. Silica in the insoluble condition, and the bases as soluble chlorides, are thus obtained, and these are separated by filtration.

Many silicates, however, do not yield to this treatment, prominent amongst them being clay. This silicate of alumina is hardly ever absent as a more or less intimate admixture in such samples of limestones, iron, and manganese ores, and other minerals, as occur for manufacturing processes. Consequently, if silica be present in the form of clay, or any other not decomposable silicate, the insoluble residue referred to above is not pure silica, but contains alumina or other bodies. Professor Percy, in the analyses of British iron ores which were made in his laboratory, and are given in his "Metallurgy of Iron," examined separately the soluble and the insoluble part of the samples. On referring to these tables, it will be found that the insoluble residue is, with one exception, never pure silica, but contains alumina, and mostly also other bases. If the quantity of insoluble substance be small, say below 2 per cent, and iron and certain rarer bodies are known to be absent, we may safely, unless absolute accuracy is required, treat it with hydrofluoric and sulphuric acids, ignite, and assume the difference to be silica, the residue being alumina. If the appearance of the insoluble matter be that of pure quartz, we may take it to be all silica.

For more accurate analysis, however, and when the insoluble residue is considerable in quantity, either the whole of the sample under treatment, or the insoluble part, must be fused with sodium carbonate, or some other basic flux, in order to obtain a more basic compound, which is readily decomposed by hydrochloric acid. This way of proceeding has two drawbacks, which, whenever possible, are eschewed by the analyst, viz., considerable expenditure of time and the introduction of a considerable quantity of foreign matter into the substance under examination.

Now, in the method which I am desirous to bring before this Institute, I propose to utilise as flux the bases present in the mineral itself. I prepare, by mere ignition, a basic silicate decomposable by hydrochloric acid.

As regards *limestones*, we have successfully treated in the Clarence laboratory specimens containing up to 21.2 per cent silica and 6.5 alumina, care being taken to prove the purity of the silica obtained. I utilise in this case the same portion of the finely powdered sample which has served for the determination of water and carbonic acid by ignition in a platinum crucible. It is transferred into a dry basin, and carefully treated with a little water; the lime left in the crucible is then washed into the basin with water and hydrochloric acid, and more acid added until solution has taken place. The evaporation to dryness is then quickly effected, as only small quantities of liquid have to be employed. The silica obtained is perfectly pure.

For the analysis of *ores* a somewhat more circuitous procedure is necessary, as the sample after ignition has to be re-weighed and re-powdered. I proceed as follows:—I weigh out into a tared platinum crucible a quantity of the finely powdered sample, something more than what is required for actual analysis; then ignite, at first gently, afterwards for about twenty minutes, to nearly a white heat, either in a good muffle or within a clay jacket over

* A Paper read before the Iron and Steel Institute.

a strong blowpipe flame. An incipient fusions should take place. I re-weigh and carefully detach as much as possible from the crucible; this will be effected easily if the substance before ignition had been pressed into one corner of the crucible, and not spread over the bottom. The mass will be found exceedingly hard, and is best treated by being at first roughly pounded in a steel mortar and then finished in an agate or wedgewood mortar.

In weighing out a portion for analysis, an allowance may be made in proportion to the loss sustained in the ignition, so that subsequently calculations may be made on the weight usually taken for analysis. The power is then digested in a porcelain dish with strong hydrochloric acid. If the treatment has been successful, a clear jelly or solution will readily form, and the analysis can be proceeded with in the ordinary way.

The ignited silica may still contain certain impurities in small quantities. Titanic acid has to be looked for in certain iron ores, and barium sulphate in manganiferous ores; indeed, I have met with a red hematite which contained 19 per cent of the latter body. It is advisable, in the presence of baryta, to precipitate it completely as sulphate before filtering the silica. The ignited residue, on treatment with hydrofluoric and sulphuric acids, will leave the whole of it, the difference being silica. The residue may be examined for titanac acid by the known methods.

The applicability of this method is limited by the percentage of silica. I have been successful with ores containing up to 25 per cent of silica, but not with an ore containing 31 per cent. The latter was a magnetic ore, containing 41 per cent of iron; the gangue consisted of quartz, greenstone, and garnet. In order to try if the want of success was merely owing to the excessive amount of silica, and not to the nature of the compounds, as which in this case it occurred, I added to a fresh portion 50 per cent of ferric oxide, so bringing the silica down to about 21 per cent. After ignition, I obtained a mass which was readily decomposed, even in rough lumps.

Calcined Cleveland mine, a very heterogeneous mixture of numerous compounds, which does not even yield the whole of its iron to hydrochloric acid, is readily decomposed after semi-fusion of the finely powdered sample.

The platinum crucible, after use, is best cleaned by fusing some sodium carbonate in it. Although hydrochloric acid slowly dissolves whatever adheres to the sides, its use is better avoided on account of the possible presence of manganese.

My experience with this method has been confined to the class of minerals specified; but it may undoubtedly be made useful for the analysis of many others, impure carbonates, and such as are chiefly of a basic character.

The idea of letting the base contained in the mineral act as a flux for its impurities seems so readily to suggest itself that other chemists may have been employing it, particularly for limestones, where its advantages are greatest. However, I have not seen it mentioned in recent editions of standard books on analysis, or in any technical periodicals; and having found it of practical value, I have ventured to lay it before the Institute, hoping it may help to banish from chemical analysis that un-chemical body insoluble residue.

THE METALLURGICAL CHAIR AT THE ROYAL SCHOOL OF MINES.

THE resignation of Dr. Percy, who has so long and so creditably held the Professorship of Metallurgy at this institution, has excited more attention upon the Continent than at home, and has been pronounced by the *Revue des Mines* a loss not to England alone, but to the civilised world. To find a worthy successor for a man of such eminent merit is admittedly no easy task. Metallurgy,

like every other department of applied science,—or perhaps it would be better to say of scientific art—has very much altered its character within the last quarter of a century. It has rejected the old routine processes handed down from the middle ages, or perhaps even from Phœnician days. Instead of merely presenting the pupil with a number of empirical procedures, it lays before him principles based upon exact scientific data, and hence capable of being adapted to those varying circumstances which ever and anon put the rule-of-thumb school to utter rout. The modern metallurgist is required to work with advantage ores which his predecessors neglected, and even to obtain valuable results from the old waste-heaps of former days. He has to eliminate those foreign ingredients which even in minute traces often seriously detract from the utility of a metal and reduce its market value. He is called upon to reduce the working cost of his operations by obtaining from fuel the fullest amount of duty it is able to yield. In view of future legislation on the "Noxious Fumes Question" he is expected to limit the escape of gases and volatile matters which may be injurious to animal and vegetable life. In short, his tasks are from year to year growing in delicacy and complexity. To undertake a professorship like that vacated by the resignation of Dr. Percy is therefore a most serious responsibility. To follow in the footsteps of such a predecessor, and to continue his work in a manner advantageous to science and honourable to the nation, a man of exceptional attainments is required. He should be profoundly versed both in physics and chemistry. He must possess abundant mental resources, and, above all, he must be capable of looking at every problem—every difficulty—from a point of view at once thoroughly practical and truly scientific.

But whilst thus fully estimating the importance of the position to be filled, we think that we may congratulate our readers and the nation at large on the selection that has been made. Mr. W. Chandler Roberts, the professor-elect, has fully won his spurs in chemical and physical research. We need merely mention his investigations on the use of the spectroscope in the quantitative analysis of metals. His successful connection with Her Majesty's Mint has given him a wide experience in many of the most delicate metallurgical questions, and affords the surest guarantee for his satisfactory discharge of the duties of his new position.

We feel perfectly satisfied that his department of the School of Mines will be conducted as all true friends of Science and of our national industry could wish.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.
Annual Meeting, Saturday, May 1, 1880.

Mr. GEORGE BUSK, F.R.S., Treasurer and Vice-President,
in the Chair.

THE Annual Report of the Committee of Visitors for the year 1879, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The real and funded property now amounts to nearly £85,000, entirely derived from the contributions and donations of the members. Forty-nine new members paid their admission fees in 1879. Sixty-seven lectures and Twenty Friday evening discourses were delivered in 1879. The books and pamphlets presented in 1879 amounted to about 278 volumes, making, with 509 volumes (including periodicals bound) purchased by the Managers, a total of 787 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, D.C.L., LL.D.

Treasurer—George Busk, F.R.C.S., F.R.S.

Secretary—Warren De la Rue, M.A., D.C.L., F.R.S.

Managers—The Earl Bathurst; George Berkley, M.I.C.E.; William Bowman, F.R.S., F.R.C.S.; Thomas Boycott, M.D., F.L.S.; Frederick Joseph Bramwell, F.R.S.; Joseph Brown, Q.C.; The Earl of Derby, M.A., LL.D., F.R.S.; Capt. Douglas Galton, C.B., D.C.L., F.R.S.; Hon. Sir Wm. R. Grove, M.A., D.C.L., F.R.S.; Cæsar Henry Hawkins, F.R.S., F.R.C.S.; William Watkiss Lloyd; Henry Pollock; John Rae, M.D., LL.D.; Robert P. Roupell, M.A., Q.C.; James Spedding.

Visitors—George B. Buckton, F.R.S., F.L.S.; Stephen Busk; The Lord Sackville Cecil; George Howard Darwin, M.A., F.R.S.; William Henry Domville; James N. Douglass; Right Hon. The Lord Claud Hamilton; Alfred G. Henriques; Robert Mann, M.D., F.R.C.S.; John Fletcher Moulton; William Henry Preece, M.I.C.E.; Lachlan Mackintosh Rate; James Romanes; Hon. John Gage Prendergast Vereker; Edward Woods, M.I.C.E.

PHILOSOPHICAL SOCIETY OF GLASGOW.

CHEMICAL SECTION.

Ordinary Meeting, April 19, 1880.

Mr. J. MACTEAR, F.C.S., F.I.C., President, in the Chair.

THE PRESIDENT read a paper translated from the French of M. EUGÈNE PLANCHUD, "*On the Formation of Sulphuretted Mineral Waters.*" The observations and experiments of the author showed the presence of sulphur in mineral waters to be due to the reducing action of living vegetable matter on the sulphates contained in these waters. On examining under the microscope the long delicate threads found clinging to the stones in the neighbourhood of sulphur springs, and which are generally supposed to be threads of sulphur, the author discovered them to be composed of hollow cylindrical tubes matted together. Most of these tubes were filled with spores, which, when liberated, moved about with a rapid motion, finally came to rest, and developed hair-like processes similar to those from which they had been discharged. M. Planchud conjectured these hair weeds to be the cause, and not the consequence of the sulphur in the water. To prove this he made the following experiment:—He filled three flasks with a solution of sulphate of lime. Into one of these he put dead organic matter; into the other two, hair-weeds obtained from a sulphur spring. One of the two flasks containing the hair-weeds he boiled to destroy the life of the weed and its spores. All three flasks were then sealed hermetically, and allowed to stand under similar conditions. On opening them it was found that only the flask containing the living hair-weeds gave off sulphuretted hydrogen. The other flasks remained unchanged during several months. At the end of six months, however, the flask into which the dead organic matter had been put was found to smell faintly of sulphuretted hydrogen, and on examination hair-weeds were found in it.

The PRESIDENT next read a paper "*On the Action of Concentrated Sulphuric Acid on Various Qualities of Lead.*" The question of the action of sulphuric acid on lead is one that every now and then forces itself strongly upon the attention of the chemical manufacturer, and usually causes him not a little trouble and loss ere the matter is set aside for the time.

Within recent years various changes have been made in the methods of refining the various qualities of lead, and some at least of the manufacturers of sheet and pipe lead are using brands of pig-lead, which formerly they did not employ. It is owing to these changes no doubt that there has been within the last few years so much more trouble with the leaden pans used in concentrating vitriol.

The subject of the action of concentrated sulphuric acid on lead has been investigated by various chemists, notably by Calvert and Johnson. They exposed plates of lead of 1 square metre to the action of sulphuric acid for periods of ten days, and then estimated the amount of sulphate of lead which had formed. The following table gives the results of their experiments:—

Lead operated on Contained.	A. B. C.		
	Ordinary.	Virgin.	Chemically pure.
Pb	98.8175	99.2060	100
Sn	0.3955	0.0120	—
Fe	0.3604	0.3246	—
Cu	0.4026	0.4374	—

1 square metre treated with 16 litres of pure sulphuric acid for ten days gave the following quantities of sulphate of lead:—

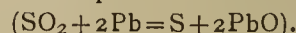
Experiment.	Sp. Gr. Acid used.	A. B. C.		
1.	1.842	67.70	134.20	201.70
2.	1.705	8.35	16.50	19.70
3.	1.600	5.55	10.34	16.20
4.	1.526	2.17	4.34	6.84

Two experiments made with ordinary commercial acid gave—

5.	1.746	49.67	50.84	55.00
6.	1.746	51.91	54.75	57.41

The result is that lead is all the more acted on the purer it is, and that an energetic action only takes place above the sp. gr. of 140° T.

Mallard (*Bull. Soc. Chim.*, 1874, ii., p. 114) experimented with lead containing:—Lead, 99.62; antimony, 0.14; iron, 0.03; not estimated, 0.21; and with ordinary commercial sulphuric acid. At a strength of 148° T., and the corresponding boiling-point, 205° C., it acts on the lead, yielding SO₂ and PbSO₄. Above 148° T., and up to 166° T., at the same time sulphur is formed



Hasenclever employed very pure soft lead from Mechnich with 99.9941 Pb, 0.0006 Ag, 0.0008 Cu, 0.0040 Sb, 0.0005 Fe, which he heated in a flask with acid of 120° T. Already at 40° C. small gas bubbles rose the lead; at 80° C. a perceptible evolution of hydrogen and H₂S took place, and this increased at higher temperatures. The same lead melted up with some pure antimony and again exposed to pure vitriol of 120° T., caused at 85° C. only a scarcely visible; at 100° C., a just perceptible; and not below 140° C., a strong evolution of gas. This agrees with Calvert and Johnson's experiments.

Lastly, Bauer has examined this matter. He employed several alloys of certain composition and vitriol at 170° T. When the acid already contained lead sulphate a higher temperature was required for the action. The results were as follows:—

1. Pure Lead.—0.2 grm. heated with 50 c.c. acid of 170° T. First sensible evolution of gas at 175° C.; stronger at 190° C. At 230° to 240° C. suddenly all the lead is changed into sulphate, which dissolves in the acid, SO₂, H, and H₂S being observed.
2. Lead and Bismuth.—(a) 90 Pb, 10 Bi; action beginning at 150°, goes on slowly and quietly up to 190°, when all the metal is decomposed. (b) 96 Pb, 4 Bi; decomposition quicker than in a; terminated at 130° to 140° C. (c) 99.27 Pb, 0.71 Bi; decomposition rapid and sudden at 160° C.
3. Lead and Antimony.—(a) 10 per cent Sb; slow and even decomposition, beginning at 190° C., ending at 230° to 240° C. (b) 5 per cent Sb; slow decomposition, beginning at 180° to 190° C., ending at 220° to 225° C. (c) 1 per cent Sb; slow decomposition just perceptible at 250° C., ending at 280° C.

4. Lead and Arsenic.—10 per cent As. Process similar to the alloy with 10 per cent Sb; ended at 240° C.
5. Lead with 1 per cent copper behaves similarly to lead with 1 per cent Sb; stronger action beginning at 250° C.; all the metal dissolved at 280° C.
6. Lead and Platinum.—(a) 10 per cent Pt; slow incomplete decomposition, ending at 280° C. (b) 2 per cent Pt; sudden decomposition complete.
7. Lead with 10 per cent Tin.—Process similar to pure lead; sudden decomposition at 200° C.

From this it would seem as if small quantities of antimony and copper made the lead more resisting and as if bismuth acted the other way.

This is about all the information there is on the subject, and it is not so widely known as it might be.

Every now and then chemical manufacturers, especially those who concentrate vitriol for sale, find that their pans give way in a much more rapid manner than is at all agreeable, and in most cases no explanation is forthcoming: in all probability the new pan lasts all right, and so the opportunity is lost of examining into the cause of the damage.

Some six years ago I had a great deal of trouble with some leaden pans; they gave way one after another, sometimes only lasting for a day, and on examining the lead in the laboratory, nothing could be found to account for the rapid action of the acid; the lead seemed to be almost pure, and at that time pure lead was considered to be the great desideratum.

On heating a piece of this specimen of lead, the sheet being nearly $\frac{1}{2}$ " thick, in strong sulphuric acid, little action took place until the temperature approached the boiling-point of the acid, when it began violently, the lead dissolving almost as rapidly as zinc does under ordinary circumstances.

A new brand of pig-lead being used for making the next set of sheets no further trouble was experienced for a long period. About the same time, however, a number of other firms in Glasgow had experience of the same sort.

The most striking case was where a lead pipe was used for supplying the pans with acid; in three weeks the action had been so severe on the lead that it resembled more a crystalline mass with a few fibres of lead holding the sulphate of lead together.

Again, it is the practice in packing vitriol for shipment to some foreign markets, to use leaden cases of about 12" square, which are enclosed in a stout wooden case clamped with iron. It happens that if lead which is easily acted on has been used the lead gets distended with the gases produced by the action of the acid, and bursts the wooden cases to pieces, the lead case becoming sometimes as round as a ball. Some very nasty explosions have occurred through a hole being pierced in the lead cases which a light has been near, and a short time ago a plumber at Greenock was very seriously injured in this way.

More recently we have again had considerable trouble with lead pans, and after examination of the lead which gave way we found as before that it was the most pure lead which gave way. In conjunction with the manufacturer we procured a large number of different brands of lead, and these were carefully analysed by my assistant, Mr. Hope. Shortly speaking, it was found that the soft pure leads gave way soonest, while the leads containing antimony or copper stood the action of the acid best. Of two samples, one containing 0.15 per cent copper apparently stood as well as one containing 0.45 per cent antimony.

It would be an interesting study, had one time to devote to it, to ascertain what bearing the relative electrical condition of the metals has to do with this subject. Less is required of copper, a good conductor slightly negative to lead, than of antimony, a bad conductor considerably more negative to lead.

The simplest safeguard against risk of pans, &c., giving way would seem to be the careful testing of the lead prior to its being made into sheets, &c. For this purpose it

will not be necessary to make an analysis, but simply to cut thin clean shavings of the lead to be tested and place them in pure cold vitriol in a test-tube; the amount of the action is clearly visible, and a comparison can easily be made of the relative value of the samples.

ANALYSIS OF LEAD.

Kind.	Lead.	Tin.	Copper.	Iron.	Zinc.	Total.
Pans which were not good—						
	99.87	0.030	0.050	nil	nil	99.95
Pans, good	99.77	nil	0.040	nil	0.16	99.97
Boxes, bad	99.82	0.120	0.030	nil	0.03	100.00
Sheet for pans—						
	99.805	0.025	0.070	nil	0.16	100.06
Sheet in stock—						
	99.884	nil	0.052	0.008	0.056	100.00
Pipe (in use for long time)—						
	99.79	0.030	0.030	nil	0.120	99.97
Sheet ..	99.89	0.021	0.048	nil	0.077	100.036
Stolberg..	99.963	trace	0.012	0.007	0.018	100.000
E. M. S..	99.924	nil	0.027	0.012	0.037	100.000
Saxony ..	99.810	nil	0.100	nil	0.090	100.000
Lead Hills	99.75	nil	0.150	nil	0.100	100.000
Mechernich	99.924	nil	0.005	0.007	0.064	100.000
La Cruz..	99.870	nil	0.066	nil	0.074	100.01
Llanelly..	99.880	nil	0.040	0.014	0.088	100.02

NOTES.—Lead Hills dissolved least in cold sulphuric acid, and the Mechernich dissolved most. The others dissolved quick in inverse proportion to the copper.

The Lead Hills contains 5.4 ozs. silver per ton = 0.015 p.c.
The Mechernich „ 0.25 „ „ „ = 0.0007 „

CORRESPONDENCE.

DEPHOSPHORISATION OF IRON.

To the Editor of the Chemical News.

SIR,—Referring to my letter which you kindly inserted in the CHEMICAL NEWS, vol. xl., p. 228, I take the liberty of again troubling you with a few further remarks and calculations. When a charge of cast iron is blown in the converter it is well known that the metal passes from a bright red to a dazzling white colour, which to some observers appears tinged with violet. This corresponds with the appearance termed "blue heat" by Sainte-Claire Deville, and is believed by him to represent a temperature near the melting-point of platinum. This increment of temperature cannot be very accurately expressed in figures, but it may be stated with sufficient relative exactitude for our purpose. Taking Daniells' measurement of the melting-point of cast-iron, 1530° C., and assuming that the temperature of the blown metal is from 1900 to 2100° C., which I believe most metallurgists allow, we have an increase of temperature of some 400 to 500° C., due to the oxidation of certain constituents of the crude metal.

By calculations based upon the most reliable data I could obtain, I made the increase of temperature to amount to 588° C., viz., from 1650° to 2238° C. This difference is evidently excessive, the specific heats having been calculated too low, the residual products being at a very high temperature; but inasmuch as this difference of 588° C. is made up of items calculated out upon similar physical data, and although the sum of these component items may not be absolutely correct, they nevertheless bear to each other an approximately true comparative value.

Now when a charge of cast iron is blown in a lime-lined converter the metalloids and manganese present are more or less burned, as also a small proportion of the iron. Referring to my calculations in your last week's number, it will be seen that—

The total number of effective heat units finally developed	= 332.1
Heat units contributed by molten cast iron	= 187.7
Heat units contributed by the blast	= 3.5
	<hr/> 191.2
Heat units contributed by combustion, &c.	140.9
$\frac{140.9}{0.1484\dagger} = 949^\circ \text{C.}$	

From the foregoing it appears that the elements oxidised contribute to the increment of temperature during the blow in the following proportions:—

2.54 p.c. carbon	0.84 p. c.
0.08 „ sulphur	0.42 „
0.98 „ phosphorus	29.09 „
1.03 „ silicon	47.31 „
0.98 „ manganese	8.22 „
1.68 „ iron	14.12 „
	<hr/> 100.00 „

The temperature attained is made up as follows:—

1.0 Crude iron $\times 0.1138$ sp. heat of Fe (Regnault) $\times 1650^\circ \text{C.}$	{ Temperature at which it is assumed the iron is introduced into the Bessemer. }	$\frac{187.7}{0.1484\dagger} = 1265^\circ \text{C.}$
0.0098 P burning with $0.01265 \text{ O} = 0.02245 \text{ P}_2\text{O}_5$ $\therefore 0.0098 \text{ P} \times 5708$ calorific value of P (mean of Andrews and Abria)		= 55.9
$\frac{0.01265 \text{ O} \times 76.3}{23.7} = 0.04072 \text{ N} \times 0.244$ sp. heat of N $\times 1500^\circ \text{C.}$		= 14.9
0.98 per cent phosphorus $0.0098 =$ nett heat units	$\frac{41.0}{0.1484\dagger} =$	276°C.
0.0254 C burning with $0.03387 \text{ O} = 0.05927 \text{ CO}$ $\therefore 0.0254 \text{ C} \times 2473$ calorific value of CO (Favre and Silberman)		= 62.8
$\frac{0.05927 \text{ CO} \times 0.245 \text{ sp. heat of CO (Regnault)} \dots}{0.03387 \text{ O} \times 76.3}$		= 0.0145
$\frac{\dots}{23.7} = 0.10904 \text{ N} \times 0.244$ sp. heat of N		= 0.0266
$0.0411 \times 1500 \text{ C}$	$= 61.6$	
2.54 per cent carbon $\therefore 0.0254 \text{ C} =$ nett heat units	$\frac{1.2}{0.1484\dagger} =$	8°C.
0.0008 S burning with $0.0008 \text{ O} = 0.0016 \text{ SO}_2$ $\therefore 0.0008 \text{ S} \times 2220$ calorific value of S (Favre and Silberman)		= 1.8
$\frac{0.0016 \text{ SO}_2 \times 0.154 \text{ sp. heat of SO}_2 \text{ (Regnault)} \dots}{0.0008 \times 76.3}$		= 0.0002
$\frac{\dots}{23.7} = 0.00257 \text{ N} \times 0.244$ sp. heat of N		= 0.0006
$0.0008 \times 1500^\circ \text{C.}$	$= 1.2$	
0.08 per cent sulphur $\therefore 0.0008 \text{ S} =$ nett heat units	$\frac{0.6}{0.1484\dagger} =$	4°C.
0.0098 Mn burning with $0.00386 \text{ O} = 0.01366 \text{ Mn}_3\text{O}_4$ $\therefore 0.0098 \text{ Mn} \times 1639$ cal. val. of Mn (assumed to be the same as that of Fe)		= 16.1
$\frac{0.00386 \text{ O} \times 76.3}{23.7} = 0.0124 \text{ N} \times 0.244$ sp. heat of N $\times 1500^\circ \text{C.}$		= 4.5
0.98 per cent manganese $\therefore 0.0098 \text{ Mn} =$ nett heat units	$\frac{11.6}{0.1484\dagger} =$	78°C.
0.0103 Si burning with $0.01178 \text{ O} = 0.02208 \text{ SiO}_2$ $\therefore 0.0103 \text{ Si} \times 7830$ calorific value of Si (Troost and Hautefeuille)		= 80.6
$\frac{0.01178 \text{ O} \times 76.3}{23.7} = 0.03796 \text{ N} \times 0.244$ sp. heat of N $\times 1500^\circ \text{C.}$		= 13.9
1.03 per cent silicon $\therefore 0.0103 \text{ Si} =$ nett heat units	$\frac{66.7}{0.1484\dagger} =$	449°C.
0.0168 Fe burning with $0.0064 \text{ O} = 0.0232 \text{ Fe}_3\text{O}_4$ $\therefore 0.0168 \text{ Fe} \times 1639$ calorific value of Fe (mean of Andrews and Dulong)		= 27.5
$\frac{0.0064 \text{ O} \times 76.3}{23.7} = 0.0206 \text{ N} \times 0.244$ sp. heat of N $\times 1500^\circ \text{C.}$		= 7.5
1.68 per cent iron $\therefore 0.0168 \text{ Fe} =$ nett heat units	$\frac{20}{0.1484} =$	134°C.
$\frac{0.06936 \text{ O} \times 76.3}{23.7} = 0.22329 \text{ N} = 0.29265 \text{ air} \times 0.2375$ sp. heat of air (Regnault) $\times 50^\circ \text{C.}$		
(Temperature at which the air is blown in)	$= 3.5 \frac{3.5}{0.1484\dagger} =$	24°
		2238°C.

† The sum of the specific heats calculated on the respective quantities of the non-gaseous residual products.

In the following calculations I have deducted the amount of heat required to raise to 2238° C. the residual non-gaseous products of the oxidation, as well as the lime necessary to neutralise the silica, and also the magnesia and the remainder of the lime, as if they were necessary to supersaturate the phosphoric acid.

0.0098 P burning with 0.01265 O =	0.02245 P ₂ O ₅ + 0.02481 CaO = 0.04726 Ca ₃ P ₂ O ₈ × 0.205 sp. heat of Ca ₃ P ₂ O ₈ = 0.0097	
59.35 p.c. CaO × 0.23 slag =	0.1365 CaO - 0.02481 CaO = 0.04122 CaO × 0.202 sp. heat of CaO = 0.0142	
5.01 p.c. MgO × 0.23 slag	= 0.0115 MgO × 0.266 sp. heat of MgO = 0.0031	
0.87 p.c. CaSO ₄ × 0.23 slag	= 0.002 CaSO ₄ × 0.2 sp. heat of CaSO ₄ = 0.0004	
	0.13326 i.e., 13.3 p.c per ton of metal	0.0274
0.0098 Mn burning with 0.00386 O	= 0.01366 Mn ₃ O ₄ × 0.17 sp. ht. Mn ₃ O ₄ = 0.0023	
0.0103 Si „ „ 0.01178 O = 0.02208 SiO ₂ + 0.04122 CaO	= 0.0633 Ca ₂ SiO ₄ × 0.214 „ Ca ₂ SiO ₄ = 0.0135	
0.0168 Fe „ „ 0.0064 O	= 0.0232 Fe ₃ O ₄ × 0.17 „ Fe ₃ O ₄ = 0.0039	
	0.10016 i.e., 10 p.c. per ton of metal	0.0197
1.0 Crude iron - 0.11 loss by oxidation, &c. 0.89 Fe × 0.1138 sp. heat of Fe =		0.1013
		10.1484

Initial heat of the molten metal	$\frac{187.7}{0.1484 - 0.0274 - 0.0197} = \frac{187.7}{0.1484 - 0.0471} = \frac{187.7}{0.1013} = 1852^{\circ} \text{C.}$	
2.54 per cent carbon burned to CO	$\frac{1.2}{0.1484 - 0.0274 + 0.0197} = \frac{1.2}{0.1484 - 0.0471} = \frac{1.2}{0.1013} = 11^{\circ}$	
0.08 per cent sulphur burned to SO ₂	$\frac{0.6}{0.1484 - 0.0274 - 0.0197} = \frac{0.6}{0.1484 - 0.0471} = \frac{0.6}{0.1013} = 6^{\circ}$	
0.98 per cent phosphorus	$\frac{41.0 - (0.0274 \times 2238^{\circ} \text{C.})}{0.1484 - 0.0274 + 0.0197} = \frac{41.0 - 61.3}{0.1484 - 0.0471} = \frac{20.3}{0.1013} = -200$	
1.03 per cent silicon	$\frac{66.7 - (0.0135 \times 2238^{\circ} \text{C.})}{0.1484 - 0.0274 + 0.0197} = \frac{66.7 - 30.2}{0.1484 - 0.0471} = \frac{36.5}{0.1013} = 360^{\circ}$	
0.98 per cent manganese	$\frac{11.6 - (0.0023 \times 2238^{\circ} \text{C.})}{0.1484 - 0.0274 + 0.0197} = \frac{11.6 - 5.1}{0.1484 - 0.0471} = \frac{6.5}{0.1013} = 64^{\circ}$	
1.68 per cent iron	$\frac{20.0 - (0.0039 \times 2238^{\circ} \text{C.})}{0.1484 - 0.0274 + 0.0197} = \frac{20.0 - 8.7}{0.1484 - 0.0471} = \frac{11.3}{0.1013} = 112^{\circ}$	
Heat contributed by the blast	$\frac{3.5}{0.1484 - 0.0274 + 0.0197} = \frac{3.5}{0.1484 - 0.0471} = \frac{3.5}{0.1013} = 34^{\circ}$	
		2438° C. - 200° C. 200° C. 2238° C.

After deducting the heat required to raise their non-gaseous residual products to 2238° C., the elements oxidised would contribute to the increment of temperature during the blow as follows:—

Per cent.	Heat generated.	Heat expended.
2.54 carbon burned to carbon oxide	1.99	
0.08 sulphur burned to sulphurous acid	1.09	
0.98 phosphorus with the basic addition requires ..	36.23	
Heat producers { 1.03 silicon	65.22	
0.98 manganese	11.59	
1.68 iron	20.11	
Heat employed in raising the temperature of the blown product to 2238° C.—		
$\frac{2238^{\circ} - 1352^{\circ} + 34^{\circ} = 352^{\circ} \times 100}{2438^{\circ} - 1852^{\circ} + 34^{\circ} = 552^{\circ}} =$	63.77	
	100.00	100.00

Of the three elements, silicon, manganese, and iron, the first undoubtedly produces the greatest nett amount of heat, and, on account of its importance as a slag constituent, will of necessity be present in the charge blown. It will, however, be convenient and preferable from several points of view, to use comparatively non-siliceous iron in dephosphorisations conducted in the Bessemer converter. Certain metallurgists now assert that phosphoretic non-siliceous iron should be made specially for the operation, which presents the peculiar anomaly of putting phosphorus into the metal in order to take it out. In a letter which appeared in *Iron*, July 19, 1879, I argued in favour of employing a comparatively non-silicious poor manganiferous iron of lower percentage than what is ordinarily called spiegeleisen. This metal would be made by mixing manganiferous ores in the furnace charge and smelting with basic fluxes. Large quantities of poor manganiferous ores can be obtained at the same price per unit of manganese as per unit of iron; and an abundant supply of manganese can also be readily obtained from the great heaps of manganese cinder produced in the smelting of spiegel prior to 1877, when a basic cinder was first introduced. Of this slag many thousand tons have been made since the commencement of mangan-

ese smelting in this country, containing from 15 to 30 per cent of manganese protoxide. These slags make an excellent flux, and would be sought after like those ancient iron scoræ which were long ago dug up and re-smelted in our island.

When Bessemerising crude metal containing 2 or 3 per cent of manganese the afterblow would be rather longer than usual, and most of the manganese present in the metal would be burnt out. A little iron would also burn, and in this manner bases having affinity for phosphoric acid would be formed in molecular contiguity with it. To increase the temperature yet further the converter should have double tuyeres, lower and upper. Through the upper tuyeres a stream of air would be injected to burn the carbonic oxide as it emerged from the surface of the decarburising metal. A very high temperature would be thus obtained, and the metal would stand pouring and casting without that tendency to set which produces the objectionable skulls of steel sometimes left in the ladle; and the refractory cinder would remain molten and pour clean from the vessel, thus avoiding the mechanical wear and tear of the lining at present incurred in its removal. Messrs. Cooper and Pink have pointed out at the recent meeting of the Iron and Steel Institute that the explosions which ensue when the spiegel is added to overblown metal do not take place when a poor manganiferous iron is subjected to the afterblow. The manganese, it is well known, induces the removal of the sulphur from the metal both in the blast-furnace and likewise in the basic-lined converter. As iron and manganese require far less air to oxidise them than the other elements present in the crude metal, the Bessemer operation will be curtailed, and less spiegel will be necessary to convert the product into steel. The converter would of course have the usual basic lining, and I do not assert that the addition of basic oxides during the blow could be altogether dispensed with.—I am, &c.,

JOHN HOLLWAY.

London, May 18, 1880.

AMMONIA FROM THE NITROGEN OF THE ATMOSPHERE.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 229, there is a note signed "S. E." stating that Rickman and Thompson's patent has been anticipated by Clark and Smith's patent, No. 4650, 1878. With the two processes before one, it is difficult to tell what E. S. means by this statement: there is not the slightest resemblance between them. The end aimed at in both, of course, is the same; that is all. If he means that their process is better than ours, let them be judged on their merits, which they inevitably will be. But if E. S. means that our process is a repetition or infringement of theirs, then I can account for such a mistake only by assuming that E. S. does not know what our process is. Let us see first what they propose to be done; certainly, to me, they propose some startling things.

First, the apparatus:—A quasi retort of fire-clay, cooled round with a copper or iron pipe wherein to make superheated steam; a fire-clay tube filled with platinum-foil or platinised pumice stone, which also surrounds the retort. This is No. 1, or the ammonia furnace. The other two inventions and their appliances described in the specification not relating to ammonia I omit.

I will give their mode of procedure in their own words (p. 7, n. 35):—"One of the essential purposes of the No. 1 furnace is to heat the nitrogen of the atmosphere to as high a temperature as possible, because it is only when at an intensely high temperature that atmospheric nitrogen becomes nascent or atomic, and so is rendered capable of entering into combination."

About this sort of *nascent state* nothing need be said here; but that atmospheric nitrogen is only capable of entering into combination when at an intensely high heat

does certainly not agree with the facts—all the worse for the facts, no doubt.

On page 8 (n. 15) are represented five reactions. The fifth formula shows the reaction of steam on barium cyanide to form ammonia.

Here, then, we have their process of making ammonia:—First, a cyanide is said to be formed, and this is decomposed by superheated steam, and the hydrogen and nitrogen unite to form ammonia, and all this is said to take place at an intensely high heat and at a white heat. But a cyanide is decomposed at such a heat, therefore cannot be formed at that heat, and free ammonia is irrecoverably decomposed at a red heat; therefore, there is no possibility of any being formed at an intensely high heat or a white heat.

But assuming that there is an error as regards the heat, and that ammonia is formed as set down, except the heat. It will be seen how widely different the processes are. We take our nitrogen as it is in the atmosphere, and decompose our vapour of water, not superheated steam, by means of incandescent coal-dust, not made into bricks by hydraulic pressure, but simply coal dust or slack. The hydrogen being nascent at the instant of the decomposition of the vapour of water, unites with the free nitrogen of the air and forms ammonia in the presence of chloride of sodium, which is decomposed by the ammonia. The ammonia appropriating the chlorine and taking up another equivalent of hydrogen passes off as sal-ammoniac, and is condensed as a powder or in solution. Here, then, you have the two processes. Wherein is the sameness? Moreover, we work at a comparatively low heat; Clark and Smith require an intensely high heat. We use a simple brick chamber as furnace; Clark and Smith describe a complicated and expensive structure of clay retort and tubes, copper or iron pipes, and platinum-foil or platinum-black and pumice. How long this structure would hold together at the intense heat described, if ever set to work, it would not be much of a prophecy to predict. But this, of course, is no affair of mine. I only have to show that the note of E. S. is a mistake, which I think I have done.—I am, &c.,

J. B. THOMPSON.

May 15, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 15, April 12, 1880.

Explanation of the Experiment of MM. Lontin and De Fonvielle.—M. Jamin.—The Ruhmkorff machine gives two sorts of currents, the one direct and the other inverse, which succeed each other alternatively and indefinitely at very short intervals. The direct current magnetises the disc transversely. It is to be noted that in spite of the movement which the disc may have, this magnetisation has always the same direction in space, but that it is variable in the disc. It does not adhere to the matter of the disc and cannot draw it along. But as soon as the direct current ceases there remains in the disc a permanent magnetisation, which inheres in the matter of the disc and endows it with polarity.

Certain Compounds of the Halogens.—M. Berthelot.—The mean formation heat of iodic chloride = +41.35 cal. For iodic terchloride the mean formation heat is +16.3 cal.; iodic bromide, +11.9 cal.; bromous chloride, obtained by the action of gaseous chlorine and liquid bromine, gives +0.60, but if the bromine is gaseous, +4.6 cal. In the formation of iodised potassium

iodide, KI_3 , from solid ingredients, the heat evolved is substantially *nil*; if the iodine is gaseous the amount = + 10.8. The corresponding bromine compound, KBr_3 (?), gives with dissolved ingredients + 3.53, but with solid bromide + 3.3, and with gaseous bromine + 11.5. The author calls attention to the analogy of the polyiodides and polybromides with the alkaline polysulphides and peroxides. There is a complete parallelism between the series of the sulphides or oxides of any metal and the series of chlorine, bromine, or iodine derivatives of the metal, if all these derivatives are referred to the equivalent weight of the metal. These relations are rendered manifest by the equivalent notation, whilst the atomic notation tends to mask them by obscuring the natural signification of the law of multiple proportions.

Change of Form in Glass Tubes exposed to High Pressures.—E. H. Amagat.—The author's experiments do not favour the idea that these tubes exposed to great and prolonged pressure from within undergo any change of form or size.

Certain new Experiments on Magnetic Attractions.—M. Ader.—Among the bodies examined, such as wood, paper, &c., elder-pith is most sensibly attracted by the magnet. With a Jamin magnet capable of supporting 100 kilos. the author has been able to attract a ball of elder-pith at a distance of 0.002 metre.

The Congelation-point of Alcoholic Liquids.—F. M. Raoult.—Fermented liquids freeze at a lower point than mixtures of alcohol and water of the same strength. The difference increases with the proportion of alcohol, and has about 0.10° C. for each percentage of alcohol.

Two New Sodium Silico-titanates.—P. Hautefeuille.—One of these compounds is formed of silica, 31.83 per cent, titanate acid, 52.08, and soda, 16.09. The composition of the other is:—Silica, 43.86; titanate acid, 40.00, and soda, 16.14. The chemical and crystallographical properties of both are described.

The Assay of Pyrites by the Gravi-volumetric Method.—A. Houzeau.—Inserted at length.

Formation of Tetra-methyl-ammonium Nitrate.—E. Duvillier and A. Buisine.—Juncadella proposes to prepare methylamine by causing an alcoholic solution of ammonia to react upon methyl nitrate at 100° in a close vessel. The authors use in preference a solution of ammonia in wood-spirit, avoiding thus the possible formation of ethylamine.

Natural and Mydriatic Alkaloids of Belladonna, Datura, Jusquiama, and Duboisia.—A. Ladenburg.—Belladonna contains two alkaloids, atropine, which is capable of being resolved into tropine and tropic acid, and hyoscyamine. *Datura stramonium* contains hyoscyamine, which is identical with daturine, and apparently also atropine. Jusquiama contains hyoscyamine, and a base which the author has not yet succeeded in isolating.

The Existence of Ammonia in Plants.—H. Pellet.—The author has detected ammonia in vegetable matter, such as the roots and leaves of the beet, and in wheat, in which all the magnesia present existed in the state of ammonia-magnesian phosphate. He recommends that these observations should be extended, especially noting if the proportion is modified by the addition of ammonia or of nitrogen under various forms, to the manures.

NOTES AND QUERIES.

Cyanogen.—Will any correspondent kindly inform as to a method of estimating the above quantitatively in ammoniacal liquids?—W.

White Muriate of Ammonia.—Would Mr. H. Rayner kindly give the most economical process for the manufacture of white muriate of ammonia of a nearly pure quality and fit for best markets?—J. T.

Gluten.—Through the medium of your journal I shall be glad to be informed of a solvent for gluten so as to become food for the yeast plant.—D. M. P.

MEETINGS FOR THE WEEK.

- TUESDAY, 25th.—Civil Engineers, 8.
— Royal Medical and Chirurgical, 8.30.
— Royal Institution, 3. Mr. Fiske, "American Political Ideas."
— Anthropological Institute, 8. John Milne, F.G.S., "The Stone Age in Japan." C. Pfoundes, "The Japanese People."
WEDNESDAY, 26th.—Society of Arts, 8.
— Geological, 8.
THURSDAY, 27th.—Royal Institution, 3. Mr. T. W. R. Davids, "Buddhist Sacred Books."
— Royal, 4.30.
— Philosophical Club, 6.30.
FRIDAY, 28th.—Royal Institution, 8. Mr. F. Hueffer, "Musical Criticism," 9.
— Quekett, 8.
— Clinical, 8.30.
SATURDAY, 29th.—Royal Institution. Prof. Morley, "The Dramatists before Shakespeare," 3.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1070.

PROPOSED SOCIETY OF INDUSTRIAL CHEMISTS.

WE learn that meetings have been held at Liverpool, Manchester, and Widnes to consider the formation of a society or institute for promoting the application of chemical science to the arts and manufactures. It will, we think, be admitted that some organisation which should afford those interested in industrial chemistry an opportunity of exchanging ideas and discussing professional questions would be of great value to the interests affected and to the trade of the country at large. Such organisations, slightly varying in their respective scopes and characters, exist abroad, and have proved exceedingly useful. But in England, manufacturing chemists hitherto have to a very great extent held aloof from each other, and have actually shunned the interchange of opinions and experience. Hence, both societies and journals of a technical character have met with very scant encouragement. But it is now very widely felt that this mutual isolation is one of the very causes which are placing our industrial eminence in jeopardy, and which, in the chemical arts as well as in other departments, are enabling foreign nations to steal a march upon us. The formation of some such society as the one proposed we should therefore regard as a hopeful sign, and should be prepared to give it every support in our power.

At the same time both its scope, its objects, and its very name should, we submit, be carefully considered and well defined. The desired association will not, of course, wish to come in collision with the Chemical Society or the Institute of Chemists. To do so would be a mere waste of energy, time, and resources. It must take up ground hitherto uncovered, the whole of such ground, and, if possible, nothing but such ground. This object will, we think, be best secured by acting on the opinions expressed by Dr. Schunck, Prof. Roscoe, Mr. Mond, and Dr. R. Angus Smith. The Society should embrace the whole field of manufacturing chemistry, and its objects would be most satisfactorily expressed by its adopting the name "Society of Technical Chemists." To the views of Mr. Eustace Carey, shared apparently by Dr. Hewitt, certain grave exceptions may be taken. The former of these gentlemen proposed that the Society should be called the "Society of Chemical Engineers." He further defined a "chemical engineer" as a "person who had mechanical knowledge, and who applied that knowledge to the utilisation of chemical action." But there are many ways in which the chemical arts may be promoted which do not in any way turn upon the application of mechanical knowledge. An invention may, and very generally does, turn upon the utilisation of some reaction hitherto unknown, or at least overlooked, and the person discovering such reaction may either have no mechanical knowledge at all, or if he has may not derive from it any assistance in the investigation which he has in hand. We do not deny that a Society of Chemical Engineers might exist, and might as such do work of great importance. But it would not cover the ground which is required, and would necessarily exclude many men engaged not in pure chemistry but in the chemical arts, and who are promoting manufactures by their chemical knowledge. On the other hand, the name proposed by Mr. Carey would, as Dr. Mond hinted, admit men who were not chemists at all. The great part of the work to be done, and of the problems to be solved in the future—as far as the chemical arts are concerned—cannot, in our opinion, be brought within the limits of the term

"engineering" as understood in England. In France and Germany the word covers more or less distinctly the application of any science to industrial purposes.

We feel, therefore, very well satisfied with the resolution which was unanimously adopted:—"That a committee be appointed to consider the question of the formation of a society for the promotion of the application of chemical science to manufactures."

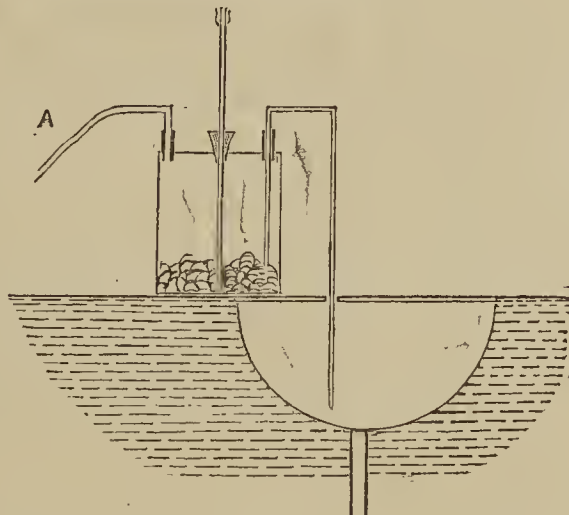
We shall await the report of this committee with much interest, and shall feel extremely gratified if they are able to draw out a definite plan for the organisation of such a society. Any assistance which we can render in so laudable an undertaking we shall be happy to furnish.

SULPHURETTED HYDROGEN APPARATUS.

By A. PERCY SMITH, F.C.S., F.I.C.

FROM time to time letters have appeared in the *CHEMICAL NEWS* describing various forms of apparatus for producing sulphuretted hydrogen, designed with the purpose of obtaining an automatic supply of the gas, or else as a more convenient method than the old original way. Having tried all these forms, and a good many others of my own designing, I have long since come to the conclusion that they are all failures, and that the only apparatus that is to be depended upon is the old hottle and thistle funnel.

Of all the forms of automatic apparatus perhaps Kipp's is the best, but I have found it almost impossible to prevent leakage of gas at the cork where the tap is inserted. This, of course, results in finding no sulphide left when you wish to use the gas. Added to this, the cold acid dissolves so slowly and so soon gets saturated with ferrous sulphate that I soon gave up the use of it in despair, and turned it into a hydrogen generator.



Apparatus consisting of two bottles connected with an india-rubber tube are not bad in idea, but in practice are failures, owing to the hardening of the tube and consequent leakage, also clogging, from deposition of ferrous sulphate. In fact, whatever form is used constant washing out is imperative, and when the apparatus is at all complicated in structure this becomes a nuisance; so that all appliances for saving the acid are really of no use. One of the forms that have been suggested consists of a lump of galena suspended in a bottle of acid, and forming one pole of a galvanic couple. On joining two wires outside the cork H_2S is evolved, but so slowly as to be practically useless, otherwise it is a good idea.

Fusion of sulphur and paraffin in a flask usually results in an explosion, caused by the stoppage of the evolution tube with sublimed sulphur, and the mixture spreads over the surface of the flask so as to totally exclude a sight of its contents; the gas, moreover, has a peculiarly offensive smell. We are therefore driven back to the old form, and

in order to render the washing out a simple matter, about two years ago I made an addition in the form of a syphon, which obviates the necessity of removing any of the tubes.

A three-necked Wolff's bottle has a funnel-tube in the centre hole, a syphon in one of the side holes, and the delivery-tube in the other. The gas is evolved by adding a freshly prepared mixture of H_2SO_4 and water (which is at about the temperature of boiling water). The evolution of gas under these circumstances is immediate and rapid, and when a sufficient gas is obtained, I pinch the rubber tube A, and pour more water down the funnel till the acid liquid begins to syphon off, and flows away through an aperture in the lid into the basin below (in the fume chamber). When the acid has gone, I follow up with more water, till the sulphide is properly washed. (It is as well to have the syphon capable of sliding in its cork, as if the interior end becomes covered with powder it may refuse to work until raised).

I have had this apparatus in constant work for two years, and during that time it has never been taken to pieces for cleaning. Fresh sulphide is dropped through the centre hole as required.

Rugby, May 10, 1880.

ON A BLACK SUBSTANCE PRODUCED FROM SULPHUR.*

By H. CHAPMAN JONES.

WHEN sulphur is burned there is invariably a black residue left, which disappears if heated to a red heat. Every sample of sulphur, of whatever quality, that I have been able to obtain during the last few years has shown this reaction most distinctly.

During my work on sodium polysulphides (see *Journ. Chem. Soc.*) this substance has been occasionally obtained in very appreciable quantities; but so mixed with sulphur and other matters that it was in a totally unfit state for anything like a complete examination. In order to know how to deal with this body I have obtained the following facts concerning it:—

Concentrated HCl and HNO_3 , concentrated or dilute H_2SO_4 , may be evaporated away from a vessel containing it, apparently without affecting it in the least. Boiling solution of KCy has no action on it, nor fuming sulphuric acid after standing even for days. It does not appear to suffer any change on digesting it for three weeks in CS_2 . If heated to just below a red heat it disappears, apparently burning away, but no characteristic odour has ever been detected. Fusing nitre dissolves it at once, at a temperature considerably below that necessary to get rid of it by mere heat.

To get an idea of the quantity produced by merely burning sulphur, I prepared some sulphur by acidifying with hydrochloric acid and boiling, a mixture of clear solutions of sodium thiosulphate and ammonium sulphide. The precipitated sulphur was boiled with dilute hydrochloric acid, then repeatedly with water, and dried at 100° . It was again treated with acid, and boiled a considerable number of times with water, and dried.

Portions of this sulphur were then burned in porcelain vessels, and the residue heated to a temperature that would ensure the absence of unchanged sulphur. The black substance was distinctly seen to separate from the melted mass as combustion proceeded.

Weight used.	Total Residue.	Loss on Ignition.
1 grm.	0.0005	0.0002
1 „	0.0004	0.0002
1.5 „	0.0008	0.0004
2 „	0.0012	

This last residue, or rather film, was scraped together, introduced into a small glass tube and heated. At a red heat a sublimate apparently of ordinary sulphur was formed, the residue being scarcely appreciable.

Having thus obtained sufficient information respecting this substance to know how to deal with it, I do not propose to investigate it further, as Dr. Theodor Cross (*Berl. Acad. Ber.*, 1879, p. 788) is examining certain products from sulphur, and among them most probably this one.

ANALYSES OF SOME AMERICAN TANTALATES.*

By W. J. COMSTOCK.

THE following paper contains analyses of three American tantalates which have not been previously investigated, and which offer some points of especial interest.

No. I. was collected by the late Prof. F. H. Bradley, in Yancey county, N. C.; its precise locality is unknown. The specimen analysed was from a massive piece, a few ounces in weight. Specific gravity = 6.88.

	I.	II.	Mean.	Ratios:
Ta_2O_5 ..	60.50	59.35	59.92	0.1349
Nb_2O_5 ..	23.02	24.24	23.63	0.0882
FeO ..	12.90	12.82	12.86	0.1786
MnO ..	3.09	3.03	3.06	0.0431
MgO ..	0.35	0.32	0.34	0.0085
Total ..	99.86	99.76	99.81	

$\text{R}_2\text{O}_5 : \text{RO} = 1 : 1.03$ and $\text{Nb}_2\text{O}_5 : \text{Ta}_2\text{O}_5 = 1 : 1.53$,
or nearly 2 : 3.

No. II. was from Northfield, Mass. The portion analysed was a fragment of a large crystal, which had the habit and angles of ordinary columbite. It was placed in my hands by Prof. Brush. Specific gravity = 6.84.

	I.	II.	Mean.	Ratios.
Ta_2O_5 ..	57.23	56.57	56.90	0.1281
Nb_2O_5 ..	26.62	27.01	26.81	0.1000
FeO ..	10.11	9.98	10.05	0.1397
MnO ..	5.92	5.85	5.88	0.0828
Total ..	99.88	99.41	99.64	

$\text{R}_2\text{O}_5 : \text{RO} = 1.025 : 1$ and $\text{Nb}_2\text{O}_5 : \text{Ta}_2\text{O}_5 = 4 : 5.1$.

No. III. was from Branchville, Conn. Its occurrence was described by Messrs. Brush and Dana,† and the specimen analysed was given to me by them. Only a small quantity of the mineral was found at the locality, and enough for one analysis was all that could be obtained pure. Its powder was brownish grey, and in thin fragments it was slightly translucent. Specific gravity = 6.59.

		Ratios.
Ta_2O_5 ..	52.29	0.1175
Nb_2O_5 ..	30.16	0.1126
MnO ..	15.58	0.2194
FeO ..	0.43	0.0059
CaO ..	0.37	0.0066
Total ..	98.83	

$\text{R}_2\text{O}_5 : \text{RO} = 1 : 1.007$ and $\text{Nb}_2\text{O}_5 : \text{Ta}_2\text{O}_5 = 1 : 1.04$.

Tantallic and niobic acids were separated by Marignac's method,‡ in the application of which I have received all necessary aid from Prof. O. D. Allen. In other respects the methods recommended by H. Rose were followed.

* Contributions from the Laboratory of the Sheffield Scientific School, No. lvii.

† *American Journal of Science*, July, 1878, p. 34. The specific gravity, by a typographical error, as I am informed, is there given 5.6 instead of 6.5.

‡ *Archives des Sci. Phys. et Nat.*, January, 1866.

* Read before the Chemical Society, April 1, 1880. Communicated by the Author.

The ordinary methods of testing for and separating tin, tungsten, and titanium were applied in each case, with negative results.

The relation between the specific gravities of columbites and tantalites and the percentage of tantallic acid, shown by Marignac, holds good in these examples also, as will be seen by a comparison with the numbers given in Marignac's table.*

	Sp. gr.	Ta ₂ O ₅ .
1. Columbite, Greenland	5.36	3.3 p.c.
2. „ Acworth, N. H.	5.65	15.8 „
3. „ La Valate, near Limoges	5.70	13.8 „
4. „ Bodenmais (<i>Dianite</i>)	5.74	13.4 „
5. „ Haddam, Conn.,	5.85	10(?) „
6. „ Bodenmais	5.92	27.1 „
7. „ Haddam..	6.05	30.4 „
8. „ Bodenmais	6.06	35.4 „
9. „ Haddam..	6.13	31.5 „
10. Tantalite	7.03	65.6 „

To which are here added—

Yancey Co., N.C.	6.88	59.92 „
Northfield, Mass.	6.84	56.90 „
Branchville, Conn.	6.59	52.29 „

These all agree with the formula (Fe,Mn)(Ta,Nb)₂O₆.

Since tantalum and niobium appear capable of replacing each other in all proportions in columbites and tantalites, Rammelsberg† has suggested that when the number of tantalum atoms exceeds that of the niobium atoms, the mineral should be called tantalite, and when the number of niobium atoms exceeds that of the tantalum, the mineral should be called columbite (Rammelsberg uses *niobite*). According to this method of classification, the Yancey county and Northfield minerals would be called tantalite, although the latter in form is not to be distinguished from columbite. The manganese niobo-tantalate from Branchville, however, has the ratio Nb : Ta = 1 : 1 (very nearly), a coincidence which we might reasonably have supposed possible. The almost complete displacement of iron by manganese is also an interesting peculiarity of the Branchville mineral, and is doubtless the cause of its slight translucency and the light colour of its powder.

It is perhaps of interest to add here that a mineral of this group from Utö, Sweden, containing 85.5 per cent of tantallic and niobic acids and 9.5 per cent of manganese protoxide (3.6 FeO), has been called *mangantalite* by Nordenskiöld.‡

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 20, 1880.

Prof. H. E. Roscoe, President, in the Chair.

THE following certificates were read for the first time:—P. S. Brown, E. Moritz, and F. E. Matthews. It was announced that the Longstaff Medal had been awarded to Dr. T. E. Thorpe, of Leeds. The presentation will probably take place in October.

THE PRESIDENT then called on Dr. FRANKLAND to read a paper "On the Action of Air upon Peaty Water," by Miss LUCY HALCROW and E. FRANKLAND. Dr. Tidy, in his paper on "River-Water" (*Chem. Soc. Journ.*, 1880, 295), contends that the peaty matter in running water is rapidly oxidised. He instances the water of the Shannon, which

he alleges loses thus more than 38 per cent of its organic matter during a flow of one mile, and the water supply of a town in the north of England, which similarly loses 50 per cent of its organic carbon during a flow of less than 4 miles through an open brickwork conduit. If peaty matter in solution in water possesses this extraordinary affinity for oxygen at ordinary temperatures it cannot be necessary to appeal for proofs of it to large bodies of water, which are always more or less liable to alterations from other causes. The authors have therefore studied upon an experimental scale the action of exceptionally strong peaty water upon atmospheric air, so as to ascertain how far air loses oxygen by contact with such water, and have submitted to eudiometrical analysis limited volumes of atmospheric air, which had been exposed for various periods to the action of large volumes of peaty water from unpolluted mountain sources. A sample of peaty water freely exposed in a stratum of 5 inches deep to the action of sunlight and air without shaking in a glass bottle for 70 days, lost only 6 per cent of its organic elements, at the same time depositing a slight brownish sediment. Four experiments are given as to composition of the air which had for some time been in contact without shaking with peaty water. The following will serve as an example:—Original water contained 0.0399 part of organic carbon, and 0.074 N per 100,000. After a year it contained—0.0354 C and 0.069 N. The air which had been in contact with the water contained—CO₂, 0.10; O, 20.76; N, 79.14. The results prove that the extent to which the peaty matter of the upland water was oxidised in periods of more than a year was very small indeed, as the carbon alone would have been sufficient to have taken up the whole of the oxygen from 100 c.c. of air. Experiments are next quoted in which the peaty water was violently agitated with air in accurately stoppered bottles of about 500 c.c. capacity, half filled with the sample under examination. The water before use contained in 100 volumes, 2.01 vols. of air, having the composition 1.99 per cent CO₂, 31.96 per cent O, 66.05 per cent N, and was therefore well aerated. Four experiments are then given, in which the water was well shaken with the enclosed air; in one case on the connecting rod of a steam-engine making 100 strokes per minute for ten and a half hours, at temperatures from 18° to 30°. In no sample was the colour materially altered, and the composition of the enclosed air remained almost unaffected. Thus the air in the bottle which had been attached to the steam-engine contained—0.12 per cent CO₂, 20.75 per cent O, 79.13 per cent N. Analyses were also made of air which had been shaken up with the Kent Company's water, the Colne Valley Company's water, and pure distilled water respectively. An analysis of the atmospheric air used gave—CO₂, 0.12 per cent; O, 20.94 per cent; N, 78.94 per cent. The volume of air contained in each bottle was 234.5 c.c., and from this the organic carbon alone in the water (0.00728 grm.) would remove during complete oxidation 0.0194 grm. or 13.5 c.c. of oxygen, leaving the residual air with only 15.1 per cent of oxygen. If we compare the percentage amounts of oxygen found in the different samples of air which had been in contact with peaty water with those found in the air which had been similarly shaken up in the Kent and Colne Valley Companies' waters and in the organically pure distilled water, and lastly with the percentage of oxygen in the air of the room in which the experiments were made, it is evident that minute quantities of oxygen have been absorbed by nearly all the peaty waters, but even in an extreme case in which the water travelled more than 20 miles on the connecting rod of a steam-engine, the amount of oxygen absorbed by 250 c.c. of extremely peaty water was only 0.345 c.c., which is scarcely 1.39th of that required for the complete oxidation of the organic carbon alone, or in other words, only 2½ per cent of the organic matter was oxidised, assuming that all the oxygen taken up was employed in the oxidation of organic matter; and deducting from this percentage the amount of oxygen absorbed by the Kent and Colne Valley

* Given in his paper first referred to, in which he, explains the variations from a regular progression, which are seen in the table.

† *Mineral Chemie*, 2nd edition, 1875, p. 356.

‡ *Zeitsch. Kryst.*, i., p. 386, 1877.

Companies waters, we have only 1·6 per cent of the total organic matter in the peaty water oxidised in the steam-engine experiment. The authors therefore conclude that if peaty matter dissolved in river water is spontaneously oxidised (of which there is as yet no sufficient proof), the process takes place with extreme slowness, and cannot be accomplished to any considerable extent in the flow of a river. During the experiments a considerable precipitation of brown peaty matter was observed when the strong bog drainage was mixed with a small bulk of distilled water. The cause of this precipitation the authors intend to investigate.

The PRESIDENT in proposing a vote of thanks to Dr. Frankland and Miss Lucy Halcrow (who was present at the meeting), said it would perhaps be advisable not to take the discussion on this paper until Dr. Frankland had read a further communication.

Dr. FRANKLAND, in acknowledging the vote of thanks, said that the work in the paper was almost entirely carried out by Miss Halcrow.

The PRESIDENT then said that the Discussion on Prof. Tidy's paper would now take place. He suggested that it would be well to limit the discussion to the point of "The Oxidation of Organic Matter in Rivers." He then briefly mentioned the chief statements in Prof. Tidy's paper, on which he based his assertion that organic matter is rapidly oxidised in running water, and called on Dr. Frankland to open the discussion.

Dr. FRANKLAND said that he must apologise to Prof. Tidy for confining himself on that evening to the question of oxidation. There were many portions of the paper in which he cordially agreed with Prof. Tidy, and his researches on the water of the River Nile and on Filtration were of the highest value. He should, however, consider only the question of oxidation. Can running water be at all times safely used for dietetic purposes a few hours or days after it has been mixed with sewage? This is a question of very vital importance to many millions of people in this country. Twelve years ago there was a general impression amongst chemists and others that polluted water quickly regained its original purity by spontaneous oxidation. This opinion had no foundation in quantitative observations; indeed there was not a single experimental fact to support it, for the previous condition of water analysis did not permit of the quantitative investigation of organic matter dissolved in water. Two classes of persons strongly interested in its acceptance were chiefly instrumental in the origination and diffusion of this opinion. These were the polluters of river-water and water companies drawing their supplies below the sewer outfalls of towns. Both these influential classes have always contended that even the most abominable of organic rubbish is destroyed by oxidation, in fact utterly burnt up during a flow of a few miles. This was a comfortable doctrine, and therefore a popular one. Nevertheless, it did not pass entirely unchallenged; indeed, it is difficult to imagine how any chemist accustomed to the habits of organic compounds could accept such an opinion without proofs. In 1865, Prof. Brodie entered his protest against the popular doctrine. The first Rivers Pollution Commissioners, R. Rawlinson, J. T. Harrison, and Prof. Way, concluded "that as a water supply the Thames, polluted with the sewage of the inhabitants of the river basin, is open in kind, if not in degree, to the same objections as well-water infiltrated by liquid from an adjoining cesspool. Well-water so tainted may appear to sight, taste, and smell to be harmless, and has been known to be drunk for a length of time without apparent mischief, but beyond all doubt that same water is liable under particular conditions to become poisonous." The prevailing opinion was, however, in favour of the rapid oxidation of organic matter in running water, when the Second Rivers Pollution Commissioners of 1868 undertook the quantitative investigation of the subject. During six years they determined the rate of oxidation, first, during the flow of intensely and moderately polluted rivers, secondly in artificial

mixtures of sewage and water by prolonged agitation with air, and thirdly by observing the diminution of dissolved oxygen in a mixture of sewage and water excluded from the air. For the determination of the rate of oxidation in strongly polluted water, the rivers Mersey, Irwell, and Darwen were selected as streams offering favourable conditions for quantitative experiments, because, after being intensely polluted, each of them flows for several miles without encountering any further material pollution, and their waters are well mixed by weirs, &c. The speaker then detailed the analyses, which unmistakeably disclose the fact that a flow of between 11 and 13 miles at a temperature of 18° C. produces but little effect upon the organic matter dissolved in the water, and that at or below 13° C. oxidation is practically arrested, although in the case of the Irwell the water was most effectively aerated by falling over six weirs. The results may be summarised thus:—

				Percentage Reduction of Organic Elements.	
Irwell, after a flow of 11 miles at 65° C.	..	12·7	..	1·7	0·0
" " " " " "	..	17·8	..	26·5	0·0
Mersey " " 13 miles at 4·5	..	8·7	..	20·5	0·0
Darwen " " " "	..	8·7	..	0·0	0·0

* No correction for unpolluted affluents.

The rate of oxidation in a river less polluted, in which animal and vegetable life still flourished:—For this purpose the Thames between Reading and Shiplake Paper Mill was chosen. Every care was taken to ensure fair and strictly comparable samples. Some difficulty was experienced in effecting this, owing to the great distance necessary to ensure a complete mixture between the waters of a main stream and those of a tributary. Thus the Kennet, after joining the Thames, flows nearly two miles before its individual character ceases to be apparent, and it is only after the passage of the weir and rapids at Sonning Bridge that a thorough mixture takes place. The samples were taken on a bright sunny day at the end of May, and therefore under circumstances very favourable to oxidation, but the reduction in the proportion of organic matter was exceedingly small, only 5·7 per cent at 16·7° C.; so that allowing for the retardation of the process by night, &c., it would be incorrect to assume that the river would clear itself of organic matter by a flow of 70 miles. These results were checked by experiments with artificial mixtures of sewage and water. The mixture was violently agitated every day, and was daily syphoned off and allowed to fall slowly about 3 feet, at a temperature of about 20° C. In 96 hours the organic carbon was thus reduced 6·4 per cent, and the organic nitrogen by 28·4 per cent: after 192 hours the reduction was 25·1 per cent carbon and 33·3 per cent nitrogen. This shaking, &c., was supposed to be equivalent to a flow of 96 and 192 miles respectively at a rate of one mile an hour. Similar experiments with a mixture of urine and water gave similar results. By studying the rate at which atmospheric oxygen disappears when dissolved in polluted water it was again proved that oxidation takes place very slowly, the results being that in the first period of 24 hours 6·8 per cent of the sewage was destroyed; in the second period of 24 hours, 8·9 per cent; in the third period of 48 hours 14·3 per cent; in the fourth period of 24 hours, 5·4 per cent; in the fifth, 5·8 per cent; in the sixth, 2·1; making altogether 43·3 per cent. That even this oxygen was not all used up in oxidising the carbon was proved by the fact that the quantity of carbonic acid present remained constant. The Rivers' Commission therefore concluded from the experiments made upon rivers and artificial mixtures of sewage and pure water that the oxidation of the organic matter in sewage proceeds with extreme slowness, and that no river in the United Kingdom is long enough to effect the destruction of sewage by oxidation. For six years these results have never been called in question. Dr. Tidy, however, whilst conceding the accuracy of the experiments, objects to the

conclusions, and from his own experiments concludes that peaty matter is rapidly, and sewage matter still more rapidly, oxidised in running water, and states that in his opinion the oxidation of the organic matter in sewage matter proceeds with extreme rapidity, and that after a run of a few miles, if the dilution be sufficient, the removal of the whole of the organic impurity will be effected. This conclusion is founded upon the results of analyses of samples of water taken from the Severn, the Shannon, and from certain conduits in the North of England, and also upon experiments made with sewage matter flowing through glass-lined troughs. A careful examination, however, of the conditions under which the samples were taken and the experiments made proves that they are in most cases consistent with the results obtained by the author of the present paper, while none affords any trustworthy basis for the conclusions arrived at by Dr. Tidy. As regards the Severn, the analyses, if they prove anything, prove too much. Thus, comparing sample No. 2 with sample No. 3, it appears that a flow of 30 yards has reduced the organic elements by 32 per cent; the river should therefore be quite pure after flowing 100 yards. The next mile only produced a diminution of 12 per cent more. This discrepancy is doubtless due to a want of care in taking the samples, and is caused by the varying admixture of the pure water from the affluent, the Teme, with the polluted water of the Severn. This is, indeed, proved by the chlorine determinations, which show for the first 30 yards flow a diminution of 41 per cent, whilst during the next mile only another 11.4 per cent, and proceeding, therefore, *pari passu* with the diminution of organic matter. Prof. Tidy suggests that the rapid decrease of the chlorine in 30 yards is caused by its absorption by vegetation, but produces no proof of such a powerful, and at the same time irregular, absorption. The River Wear is next considered. It is stated that between Bishop Auckland and Durham, a flow of 13 miles, the whole of the sewage of the former town is oxidised, and indeed the river is purer than it was above Bishop Auckland; even the nitrates themselves appear to be oxidised. The Rivers' Commissioners testify that the Wear at Durham is a "good water." The fact has been apparently completely overlooked by Prof. Tidy that during this 13 miles flow a considerable tributary of highly ferruginous water enters the Wear, the potency of iron in removing various organic matters from water being well known, and referred to by the Rivers' Commissioners. It is therefore probably to this ferruginous tributary and to the influx of a larger affluent, the Brownie, that this diminution of organic matter is due, at all events no one can allege that the cleansing is due solely to oxidation. As regards the alleged statement of the Rivers' Commissioners, they really said that the water was good *considering its source*, having previously described the impurity of the latter. As to the evidence drawn from the composition of Thames water at Lechlade and Hampton, the only evidence on the subject is furnished by two samples taken at Lechlade on April 18, 1868, and at Hampton on May 4, 1868, the former containing 0.133 and 0.033 of organic carbon and nitrogen, the latter 0.260 and 0.024. At Lechlade the river was running 100,000,000 gallons daily; at Hampton, 554,000,000; a flood had also occurred on April 21-24. But granting that the Thames at Hampton contains no more organic matter than at Lechlade, it by no means follows that the sewage of a million people had been got rid of, for (1) the volume has been augmented more than fivefold, to some extent by pure spring water; (2) a considerable proportion of the sewage drains into cesspools, &c., and only enters the Thames after a flood; (3) when the Wey mixes with the Thames about 6 miles above Hampton, the mixtures becomes turbid, and a precipitation of organic matter takes place; (4) for several miles above Hampton the Thames runs through a vast deposit of flint, gravel, and sand, which sops up its water like a sponge and restoring it again at a lower level, and thus vast volumes are exhaustively filtered through gravel and

sand, and the organic matter is consequently oxidised in a porous medium—a most potent agency in nature for the removal of organic matter. As regards the Shannon, Prof. Tidy's observations were limited to a portion of 37 miles, 23 in Loch Derg, and 14 in the river. It was found that in flowing through Loch Derg the organic elements diminished from 1.02 to 0.84 per cent, *i.e.*, about 18 per cent. The next sample, taken a mile lower down, shows a diminution of more than 38 per cent. The next sample, 4 miles lower down, shows an increase of 75 per cent. Four miles lower, at the foot of the Castle Connell Falls, a further sample was taken: it showed a diminution of 28 per cent. The last sample was taken above the junction of the River Mulkear, and here the organic elements had again increased 53 per cent. Thus, from the analyses, a run of a mile having a surface of less than one-tenth of a square mile reduces by oxidation the organic elements 38 per cent, whilst the passage through Loch Derg of 23 miles, exposing a surface of over 50 square miles plus 2 miles of river, diminishes them by only 18 per cent. After leaving the oxidising section the river flows 4 miles, and arrives at O'Brien's Bridge, and has gained 75 per cent of peaty matter. To account for this increase by an influx even of black peaty water requires an augmentation of the river one-third of its total volume. This increase is quite incredible, and the author concludes that somehow or other the samples taken did not fairly represent the main body of the river, and that the conclusions drawn by Prof. Tidy are therefore quite unwarranted. The analyses of the peaty water flowing through an open and a closed conduit, quoted by Prof. Tidy, are probably in a similar way rendered fallacious owing to the probable introduction *en route* of spring-water into the open conduit. The mysterious influence which so favours the oxidation of polluted water running in rivers with numerous unpolluted affluents appears to be always absent when the water is put under conditions admitting of the application of accurate experimental tests. Prof. Tidy therefore constructed a series of V-shaped troughs lined with glass, altogether 200 feet long. By means of this apparatus he finally demonstrated the oxidation of sewage in water. To these experiments the author objects—(1) That the water was exposed to an enormously greater solid surface than in any river. (2) It is not stated that the sewage was filtered, and loss might occur by deposition on the troughs. (3) And chiefly, after a short time the urea in sewage would be transformed into ammoniac carbonate. While there is no evidence whatever of the destruction by oxidation of the dead organic matter by a flow of a dozen miles, still less is there any ground for assuming that the organised and living matter of sewage is destroyed under like circumstances. Bacteria develop, multiply, and flourish for weeks in attenuated liquids. Here the author read a letter from Prof. Tyndall, in which, after apologising for his compulsory absence, he designates Prof. Tidy's views on germs as very plausible, but utterly chimerical, and continues—"Well, what about the germs of bacteria, do they commit suicide by endosmotic action? So far is this from being the case, that Dr. Tidy cannot take a single drop of water from his tap that does not contain living germs capable, when placed in a proper nutritive fluid, of developing themselves into countless multitudes of bacteria. Pray ask Dr. Tidy what reason has he to suppose that other germs behave differently from those of *Bacteria termo*." The author then criticises the assumption that the epithelial scales from scarlet fever patients, &c., are daily oxidised and disappear, and challenges Prof. Tidy to prove by a delicate balance any marked diminution of weight in any such mass of organic *débris* from oxidation. The author then upholds and justifies his statement as to the immunity which would be conferred on the Metropolis from epidemics of cholera if it had a supply of pure water, by reference to statistics from Bethlehem Hospital, Millbank Prison, &c. In conclusion the author agrees with Professor Tidy when he says, of water supplied to a community, "Art may be fairly asked to improve a good water, but it is simply

madness to ask her to deal with bad water." And again, "That impure water has been one of the most active causes of disease is a fact which to my mind has been proved beyond doubt. The researches of Dr. Ballard on typhoid, &c., supply evidence that should make us very jealous indeed of allowing a water to be used for drinking over which hangs the merest shadow of a suspicion. No one can be too strictly a water purist." The question, therefore, Can running water be at all times safely used for dietetic purposes a few hours or days after it has been mixed with sewage? is answered by the author in the negative.

The PRESIDENT said that all must compliment Dr. Frankland on the complete, clear, and withal, good natured criticism, to which they had listened with so much interest. He would ask Prof. Huxley to say something on the subject of bacteria.

Prof. HUXLEY did not wish to take part in the chemical controversy, but it had struck him on reading over Dr. Tidy's paper that there was a good deal of what he might venture to call "biological turbidity" in it. To this turbidity he would, as far as was in his power, act as a filter. He would state briefly only what were demonstrable facts. Diseases caused by what people, not wisely, call germs, e.g., splenic fever, pig-typhoid, &c., are caused invariably by bodies of the nature of bacteria; they could be cultivated through twenty to thirty generations, and then when given to the ox or the pig would invariably give rise to the characteristic disease. We have no reason even to imagine that any body capable of causing disease by such means could be anything but a body having the nature of a bacterium. Now bacteria are just as much plants as mushrooms or cabbages or the *Wellingtonia gigantea*, so that we know under what conditions bacteria can live and what they will do. Bacteria can be sown in Pasteur's solution just as mustard and cress can be sown in the soil; in it they thrive, and the liquid becomes milky, and he would ask the President whether there was any known method by which if one drop of this Pasteur's solution were placed in a gallon of water its constituents could be estimated. (The PRESIDENT having answered that he thought it was doubtful, the speaker continued)—Every cubic inch of such water would contain 50,000 to 100,000 bacteria, and one drop of it would be capable of exciting a putrefactive fermentation in any substance capable of undergoing that fermentation. For purposes of public health, the human body may be considered as such a substance, and we may conceive of a water containing such organisms, which may be as pure as can be as regards chemical analysis, and yet be as regards the human body as deadly as prussic acid. I am aware that chemists may consider this as a terrible conclusion, but it is true, and if the public are guided by percentages alone, they may often be led astray. The real value of a determination of the quantity of organic impurity in a water is, that by it a very shrewd notion can be obtained as to what has had access to that water. If it be proved that sewage has been mixed with it, there is a very great chance that the excreta of some diseased person may be there also. On the other hand, water may be chemically gross and yet do no harm to any one. The whole source of damage being, in the belief of the speaker, in the disease germs. As to the bursting of the envelopes by endosmosis, it was a question whether they had any: bacteria would be large if 1-20,000th of an inch in diameter; moreover, ordinary water was full of them, and in it they could be shaken for an indefinite period without harm. As long as bacteria had nutrition, there was no reason to suppose that oxidation or endosmosis would affect them. If, however, they were deprived of nourishment and exposed to sunlight the case might be very different.

The SECRETARY then read a few remarks which had been sent by Dr. Mills. Dr. Mills has calculated the ratio of—

$$\frac{\text{Oxygen consumed}}{\text{Sum of organic C+N}}$$

and finds that it is not constant but varies in different streams. He does not think it possible to determine the peat in a water by its tint depth, owing to the difference of colour. River water commonly contains a slimy or pectinous material which tends to separate out on any substance which acts as a nucleus. This has, in the author's opinion, a most potent influence on the purification of river water. The oxygen theory of the natural purification of waters seems utterly untenable. The criticisms of the author coincide in several respects with those already advanced above by Dr. Frankland. In conclusion the author expresses his admiration of the patience with which Dr. Tidy has collected his facts, and of the meritorious accuracy of his analytical results.

Prof. BISCHOF called attention to the omission of the cholera years in the statistics given by Prof. Tidy.

Prof. TIDY, in his reply, relied mainly on the powerful testimony given in his behalf by the statistics of the last ten years. Notwithstanding the possible contamination of a large bulk of river water by a minute drop of a fluid containing germs, yet there were as many cases of fever in towns supplied solely by well water as in those supplied by river water; this holds good for towns all over England as well as in different districts of the same town. He took exception to the laboratory experiments of Dr. Frankland on oxidation; they were doubtless most interesting and satisfactory experiments as regards shaking fluids up in bottles, but they did not represent the flow of a river, there was no vegetation, no animal life. As regards the diminution of sodium chloride in the Severn, he contended that plants did cause a decrease in the quantity of sodium chloride in running water. As to the Shannon, he knew every inch of it, and perfect streams of black drainage entered into Lock Derg and elsewhere quite sufficient to account for the discrepancies noted by Dr. Frankland. He collected the samples of water himself, and did his utmost to collect them fairly. He had no interest whatever in commending any water. In conclusion, Prof. Tidy said, that although his paper might be considered in some respects an attack on Dr. Frankland, he wished to thank him for the freedom and the kind way in which he had met him at every turn, and expressed a hope that Dr. Frankland would join him in fighting the prevailing heresies on this question which tended so to upset the public mind.

Dr. FRANKLAND having declined, owing to the late hour, the right of reply, the meeting, which had been crowded all the evening, separated shortly after eleven, and the Society adjourned to June 3.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 9, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"Analyses of the Ash of Wood of Two Varieties of the '*Eucalyptus*' Tree," by WATSON SMITH, F.C.S., F.I.C., Demonstrator and Assistant Lecturer in the Owens College.

During Easter, 1877, Mr. James Young, LL.D., F.R.S., suggested to me that I should make analyses of the ash of the two varieties of eucalyptus, viz., *E. rostrata* and *E. globulus*, commonly known as red and blue gum trees. The pieces of wood I was supplied with by Dr. Young. They appeared to be stout branches, "crop-wood," and had the bark still on. The wood of *E. rostrata* ("red gum") was evidently closer and denser in texture than that of the "blue gum," or *E. globulus*. Its bark was also thinner and smoother. I took the samples of the wood to a joiner in Zürich, and requested him to plane them down to thin shavings, with the exception of a

transverse section of each variety, to be reserved for specific gravity determinations. I requested him also to notice any peculiarities he might notice in the wood and report them, with his opinion of their quality. On calling subsequently, I noticed on entering the shop the odour of the gum or essential oil peculiar to the tree. The whole shop was fragrant with it. On planing the wood to shavings this odour is quickly diffused around. The joiner stated it as his opinion that the wood of the "red gum" variety especially was extremely hard. It had quite turned the edge of his plane. The timber of such a tree, he said, would be of great value. The wood of the "blue gum" tree was a little behind in point of hardness and solidity. Determinations were now made of the *specific gravity* of these woods in what may be termed the *air-dried* condition. The sections of both varieties had been placed in a dry atmosphere for one month. The results were:—

Eucalyptus rostrata,
"Red gum."

Eucalyptus globulus.
"Blue gum."

Specific gravity.. 0.8112 Specific gravity.. 0.772

These sections were now planed (bark included) to thin shavings, which were now weighed, burnt in a platinum dish to white ash, and this was weighed. The results were:—

E. rostrata.

E. globulus.

Ash 2.25 p.c. Ash 2.01 p.c.

Analyses were now made of the ash obtained by burning the shavings of the larger samples. This ash was white, inclining to straw colour:—

Eucalyptus rostrata,
"Red gum."

Eucalyptus globulus,
"Blue gum."

K ₂ O	9.50	K ₂ O	25.00
Na ₂ O	3.40	Na ₂ O	6.47
MgO	6.30	CaO	35.08
CaO	43.80	Ferric and aluminic	
Ferric and aluminic		phosphates ..	1.07
phosphates ..	0.78	MnO	trace
MnO	trace	SiO ₂	0.34
SiO ₂	0.29	Al ₂ O ₃	1.55
SO ₃	1.57	SO ₃	0.85
Cl.. .. .	0.60	Cl.. .. .	1.04
Sand and carbon	1.77	Sand and carbon	

The leaves of the eucalyptus contain very considerable quantities of tannin; in fact, I think it is certain at some time or other they may prove a valuable source of tannin if the tree be largely cultivated. I have been told that in some parts where the tree abounds a kind of decoction is made of the leaves, and drunk as tea is.

NOTICES OF BOOKS.

Alphabetical Manual of Blowpipe Analysis, showing all known Methods, Old and New. By W. A. Ross, Lieut-Colonel, late R.A., Member of the German Chemical Society (Author of "Pyrology or Fire-Chemistry"). London: Trübner and Co.

THE blowpipe, however justly appreciated in Germany and in America, finds little favour in England. We can scarcely name a chemist of any grade who is either engaged in researches requiring its application or who is seeking to extend its capabilities and verify its indications. A few brief and somewhat perfunctory notices of its use in qualitative mineral analysis do indeed figure in the innumerable chemical manuals and handbooks which are year by year showered upon us, but the present volume and its predecessor, "Pyrology," are, we believe, the only original and independent contributions to blowpipe che-

mistry which England has produced for at least half a century. The instrument, or to speak more correctly, the whole region of chemistry of which it serves as the symbol, is scouted alike by college graduates and amateurs, our scientific chemists and our examinees.

Colonel Ross, therefore, has a strong claim upon our attention and respect for keeping the blowpipe and its utility before the notice of the scientific public. Nor can it be denied that in this his peculiar department he has done sound and valuable work which is more appreciated elsewhere than at home. If he is mistaken in certain points surely experimental investigation would be a more legitimate way of meeting, and if needful refuting, his statements than what has been happily called "the conspiracy of silence."

The only perfectly satisfactory method of criticising the little book before us, *i.e.*, to go through all the reactions here described, in as far as at least as they differ from what we find laid down in Plattner and earlier authorities, unfortunately requires more time than we have at our disposal. As far as we can fairly judge without such a practical survey, the methods here given are in general likely to prove of great value.

There are some passages, however, with which we are puzzled, and which we think, pending further evidence, might better have been omitted. Thus on p. 41 we read:—

"CHEMICAL WATER.—A combustible but not vaporisable compound, composed apparently of hydrogen and oxygen; present in every natural and in almost every artificially prepared oxide. It ignites at apparently red heat, and burns with an orange pyrochrome." Water which is combustible and not volatile at high temperatures is a paradoxical body, and we naturally wish to know whether it has been isolated and analysed?

On p. 51 we find a process for making artificial diamonds which certainly do not consist of carbon, and whose title to the name is therefore very questionable. The process is:—"Heat a fragment of caustic lime of a size proportioned to that of the stone required in the oxidising flame at the bottom of a bead of pure boric acid. A colourless and highly refractive ball is formed inside the bead, from which it is extracted by boiling water, the bead being very soluble, but the ball insoluble. The ball should be re-purified in a fresh boric acid bead, and more caustic lime added until it is the required size, and can be then again extracted by boiling water. . . . It is a curious coincidence with regard to these lime borate balls that they are, though perfectly spherical while within the bead, often extracted from that with a curved edge, formed by the meeting of two contiguous facets, like the natural form of many diamonds." We do not think that the author means to assert the identity of calcium borate with crystalline carbon, but we fear that this passage will be misunderstood or misrepresented.

Some question may also arise concerning the following "rough way of detecting alumina," namely, by holding a pea-fragment supported on a platinum hook in the flame of a spirit-lamp, or of a good blue candle reducing flame, when alumina and silica blacken, while lime and the alkaline earths remain white." Is this reaction peculiar to alumina and silica, or does it extend to all the earths? Is not the blackening due to a deposit of carbon, and if so may it not be produced on other bodies, according to the manner they are heated?

No experimentalist seems to have taken in hand to decide a question which Colonel Ross raised some years ago in the CHEMICAL NEWS, vol. xxxvi., p. 214. In Profs. Roscoe and Schorlemmer's work on Chemistry it is stated that:—"Most metallic oxides dissolve in boron trioxide at a red heat, many of them imparting to the mass characteristic colours; hence this substance is much used in blowpipe analysis." On the contrary, Colonel Ross asserts that most metallic oxides do *not* dissolve in boric acid before the blowpipe, do *not* impart colours to it, and finally that it is not much used in blowpipe analysis except upon his system. Referring to Plattner's work (Cookeley's

edition, p. 45) we certainly find nothing to confirm the assertion that it is much used. According to this authority:—"It serves in qualitative examination to detect phosphoric acid in minerals and a small quantity of copper in presence of much lead."

We see no reason to retract or modify the opinion which we expressed when noticing the author's "Pyrology," that to go through the methods here laid down and carefully note the results would be a most useful and suggestive course of study.

The Chemistry of Agriculture. By R. W. EMERSON MACIVOR. Melbourne: Stillwell and Co.

THIS work is a treatise on agricultural chemistry, with especial reference to the local circumstances of Victoria, and contains a very large amount of useful information clothed in very plain language. After some introductory explanations the author treats successively on the constituents of plants, on the growth of crops, on the formation and the chemical and physical properties of soils, on draining and tillage operations, on the exhaustion of land, on farm-yard manure and composts, on bones and superphosphates, on guano, saline manures, neglected fertilisers, the use of lime in agriculture, and on the rotation of crops.

We notice that bone-dust as sold in Australia differs considerably from English samples: it contains more organic matter, but less nitrogen, less phosphate, and sand reaching up to 14 per cent. It appears that the Australian farmers are too much given to the use of undissolved phosphates. We regret that, as far as can be judged by this work, no extensive deposits of mineral phosphate of lime have been found in Australia. The phosphatic guanos of Lacede Island and Flat Island are used to a considerable extent both by farmers and manure-makers. Bat-guanos of fair value have been found in a number of caves. Blood and other animal refuse, capable of yielding manures no way inferior to Peruvian guano, is wasted to a very serious extent.

One of the most interesting portions of the book is the admirable Appendix by Baron von Müller, on the maintenance, creation, and enrichment of forests. The learned author shows most conclusively the importance of forests both in a sanitary and a climatic point of view. For covering the Australian deserts he particularly recommends the fig-tree, alike from its rapid growth, its adaptation to the climate, and its fruit.

The chief defect of the work is, in our opinion, the lack of an index.

CORRESPONDENCE.

SULPHUR IN PYRITES.

To the Editor of the Chemical News.

SIR,—The existence of sulphur in coal in combinations other than FeS_2 has been recognised long before the date mentioned by F.I.C., in his note on Dr. Wallace's paper. (CHEMICAL NEWS, vol. xli., p. 230.) Dr. Percy, in the first volume of his "Metallurgy," published in 1861, says of the sulphur in a certain coal that it "was present in the same state of combination in the coal as it exists in albumen, fibrine, &c." (Vide p. 102.) In the same work are numerous instances of coals containing a higher percentage of sulphur than of ash.—I am, &c.,

HENRY LOUIS.

BLOWPIPE ANALYSIS.

To the Editor of the Chemical News.

SIR,—In April last I wrote you a letter, in which I alluded, among other things, to what Colonel Ross calls "ellychnine

pyrochrome." I had not at that time been able to produce it, but being in London the other day, Colonel Ross was kind enough to show me the way. I can now get it readily, and it seems to me better, because more unmistakeable, than the flame colouration produced in the ordinary manner. Here I ought properly to add my own receipt for producing E.P., but I do not propose to do so, as I own I cannot improve on Colonel Ross's instructions already given, and, as I am sure your readers will know, ten minutes instructions in a laboratory is worth many pages of directions how to do it,—I am, &c.,

TRITON.

THE ESTIMATION OF CARBON IN STEEL.

To the Editor of the Chemical News.

SIR,—In reply to Mr. G. E. Davis (CHEMICAL NEWS, vol. xli., p. 229), if steel contained iron and carbon only, no doubt the weight of Fe_2O_3 produced would be a valuable check on the completeness of the combustion, but considerable percentages of other substances may be present. Several times statements have been made in the CHEMICAL NEWS and elsewhere impugning the accuracy of the colour test for the estimation of carbon in steel: my object in writing the note referred to by Mr. Davis was to show that when carefully worked very accurate results may be obtained.—I am, &c.,

JAMES W. WESTMORELAND.

Bowling Iron and Steel Works,
May 24, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 16, April 19, 1880

Reciprocal Displacements of the Halogens.—M. Berthelot.—The author concludes that the inverse substitution of bromine for chlorine and of iodine for bromine is, *a priori*, possible in cases where the heat liberated by the formation of the secondary compounds exceeds the heat absorbed by direct substitution, and takes place in proportions regulated by the degree of dissociation of the secondary compounds. This substitution becomes likewise possible if the temperature is raised to the degree at which metallic chlorides, bromides, and iodides are dissociated, because then the halogen acts in reality upon a portion of the free metal. This substitution does not take place between potassium chloride and bromine, nor between potassium bromide and iodine, if heated to about 400° , at least if the accessory influences of air, moisture, and the materials of the glass are avoided.

The Stability of Oxygenated Water.—M. Berthelot.—The decomposition during the first month takes place in proportion to time, but afterwards the reaction slackens and is not complete at the end of two years. Finally, however, all the oxygenated water disappears. Dilution decidedly retards decomposition. The change is greatly modified by the presence of foreign matter. A specimen containing a trace of hydrochloric acid and of barium chloride was decomposed five times less rapidly than a specimen of the same strength free from these additions. The smallest trace of a soluble alkali, or of precipitated base, determines rapid decomposition.

The Earths of Samarskite.—C. Marignac.—The author divides the earths into four portions: (1.) Earths soluble in less than 100 parts potassium sulphate and having equivalents below 119. (2.) Earths soluble in

100 to 200 vols. potassium sulphate, and whose equivalent lies between 119 and 120. (3.) Earths very slightly soluble in potassium sulphate, and with equivalents between 119 and 115. (4.) Earth almost insoluble, didymium oxide, retaining energetically a quantity of the former earths. The first group contains merely two earths already known, yttria and terbia. The second group contains terbia, traces of decipia and of didymia, and a new earth, which the author designates provisionally as Ya. The third group contains a little terbia, a notable quantity of Ya, didymium oxide, and an earth Y β , whose spectroscopic characters agree with those of M. Delafontaine's decipia, and better still with the samarium oxide of M. Lecoq de Boisbaubran.

The Electro-magnetic Gyroscope.—W. Fonvielle.—The author does not accept the explanation given by M. Jamin in the session of the Academy, April 12, since he has found it impossible to obtain rotation with induction coils in which the wire is very long.

Measure of Thermo-electric Electro-motor Forces on Contact of a Metal and a Liquid.—E. Bouty.—The results are given in the form of tables not capable of useful abridgment.

An Automatic Mercurial Pump.—G. Couttolenc.—Will be inserted at length.

On the Tropeines, Artificial Mydriatic Alkaloids.—A. Ladenburg.—The author has prepared and examined the tropeines of the following acids:—The salicylic, oxybenzoic, paroxybenzoic, amygdalic, benzoic, and phthalic. Oxy-toluy-tropine he names homatropine, on account of its homology with atropine.

On Gelose.—H. Morin.—Gelose is a mucilaginous substance imported from China under the name Ta- δ . Its properties are described at length, but its composition is not given. Although very different in its origin it is in many respects analogous with the gums. Thus it is transformed into mucic and oxalic acids by the action of nitric acid.

Ammonium Carbonate.—E. J. Maumené.—Two samples of this salt, differing very little in composition, yielded saturated solutions varying considerably in specific gravity, that of the one being 1.140, whilst the other only reached 1.060.

The Existence of Ammonia in Plants and in Muscular Flesh.—H. Pellet.—In 100 grms. of ox flesh the author found 0.15 grm. ammonia.

Sophistication of Commercial Silicate of Soda.—F. Jean.—The sample in question gave on analysis:—

Soda combined with silica.. ..	8.54
Carbonate of soda	6.36
Soda soap	2.00
Silica	21.40
Ferric oxide, alumina, traces of lime	0.74
Alkaline chlorides and sulphates ..	0.66
Water	60.05
Matter not determined, and loss ..	0.25

This sample of silicate of soda contained therefore 2 per cent anhydrous soap, but as such a solution forms a jelly on cooling the object of its introduction was evidently to thicken the silicate, giving it the appearance of a very concentrated product, and to prevent its strength being taken with the hydrometer.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 2, 1880.

Synthesis of the Homologues of Hydrocarbostyryl and Chinolin.—A. Baeyer and O. R. Jackson.—The formation of carbostyryl and hydrocarbostyryl takes place exactly in the same manner as that of cumarin and hydrocumarin. If normal phenyl-valerianic acid is treated with nitric acid and afterwards reduced, no substance resembling hydrocarbostyryl is obtained. The authors describe the

preparation and properties of phenyl-ethyl-propionic acid, of ethyl-hydrocarbostyryl, of ethyl-chlor-chinolin, ethyl-chinolin, and normal phenyl-valerianic acid.

On Diphenyl-di-imido-naphthol.—B. Goës.—By the action of aniline and the toluydines upon di-imido-naphthol there are formed a number of very beautiful bodies, particularly characterised by their stability. The reaction takes place with elimination of hydrogen, the two imid-hydrogens being substituted by phenyl groups. The authors particularly describe the action of aniline upon di-imido-naphthol.

Constitution of Nitroso-dimethyl-meta-toluydin.—C. Riedel.—The author concludes that in this compound the nitroso-group occupies the para-position with respect to the amido-group.

Action of Phosphonium Iodide upon Carbon Disulphide.—H. Jahn.—The gaseous products of the reaction are hydrogen sulphide and phosphide and light hydrogen carbide.

Certain Derivatives of Ortho- and Para-toluydin.—G. Staats.—Among these products are ortho-tolyl-sulphurea and the corresponding para compound; ortho-tolyl-ethyl-sulphurea and the para compound; ortho-tolyl-phenyl-sulphurea, ortho-tolyl-glycocol, and aceto-ortho-homoparoxy-benzaldehyde.

Formation-heat of Cuprous Chloride.—J. Thomsen.—The author considers that his results, 59,625 cal., is much nearer the truth than that obtained by M. Berthelot, 54,200 cal.

Niobite from the Iser Mountains.—J. V. Janovsky.—The author gives analyses of his specimens in comparison with those of iserite, nigrine, and iserine as given by Rammeisberg.

New Method of Determining the Specific Gravity of Liquids.—H. Sommerkorn.—Already noticed.

Composition of the Anethol Derivatives.—F. Landolph.—The author describes the following of these compounds:—Anethol-dihydride, anethol-tetrahydride or anethol-camphor, anethol-hexahydride or anethol-borneol, and the condensation-products obtained by the action of alcoholic potassa upon anethol, and upon mono-chlor-anethol.

Determination of Aqueous Vapour in the Atmosphere.—F. Rüdorff.—The author's process cannot be intelligibly described without reference to the figure of his apparatus.

The Formation-heat of Cyanogen.—J. Thomsen.—Another difference with M. Berthelot. The author had observed as early as 1854 the absorption of heat on the formation of cyanogen, which, according to M. Berthelot, had been ignored till pointed out by him in 1864.

Pyridin-tricarbonic Acid from the Cinchona Alkaloids.—S. Hoogewerff and W. A. van Dorp.—Remarks on a paper by Skraup (*Berichte*, xii., p. 2331).

Quantitative Determination of the Oxygen Dissolved in Water.—J. König.—Inserted at length.

Reactions taking place in Presence of Aluminium Chloride and Bromide.—G. Gustavson.—The author points out that he had already answered the question which Kekulé and Schrötter propose to deal with in their memoir on the conversion of propyl-bromide into isopropyl-bromide (*Berichte*, xii., p. 2280).

Preparation of Bioxy-fumaric Acid in a State of Purity.—S. Tanatar.—The nickel salt is the best medium for separating this acid from the oxalic and the fumaric acids.

Etherification of the Non-saturated Mono-basic Acids.—N. Menshutkin.—The author considers these acids under three groups, the primary, secondary, and tertiary. The etherification of all is strictly analogous.

The Structure of Sorbic and Hydrosorbic Acid.—N. Menshutkin.—Not suitable for abstraction.

Maleic and Malic Acids from Bibrom-propionic Acid.—S. Tanatar.—Bibrom-propionic acid was heated with an excess of potassa-lye, and an aqueous solution of potassic cyanide was then added, the boiling being continued for six to eight hours in a cohobator.

Belladonnine.—K. Kraut.—This base is probably isomeric with atropine.

A New Chemical Photometer for Determining the Intensity of the Ultra-violet Rays of Daylight by means of Mercury Oxalate, and Contributions to the Photo-chemistry of Mercuric Chloride.—J. M. Eder.—The author recommends a mixture of mercuric chloride with neutral ammonium oxalate. The photometer is filled with 2 vols. of a solution of 40 grms. ammonium oxalate in 1 litre water mixed with 1 vol. of a solution of 50 grms. sublimate in 1 litre of water. Before use it is exposed to light till the deposit of mercurous chloride begins to occasion a turbidity, and is then filtered. The mixture can be kept without change in the dark. Light which has passed through red, yellow, or yellowish green glass is inactive, and the decomposition is mainly produced by the ultra-violet rays.

Chemical Composition of Pyroxyline and Formula of Cellulose.—J. M. Eder.—A very extensive memoir, incapable of abstraction.

Synthesis of Methyl-ketol, an Isomer of Skatol.—A. Baeyer and O. R. Jackson.—This substance, C_9H_9N , is distinct in its properties both from skatol and indol. The authors are endeavouring to ascertain whether it is the methyl derivative of an unknown base, isomeric with indol.

Further Contributions to a Knowledge of the Putrefaction-products of Albumen.—E. and H. Salkowski.—An examination of the occurrence of aromatic oxy-acids among the putrescent-products of albumen, and of a substance which forms skatol.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.

No. 73, January, 1880.

Report Presented by MM. de Luynes and H. Peligot on behalf of the Committee of Economic Arts on a New Laboratory Balance Constructed by M. Deleuil.—This balance carries 3 kilos. in each pan, and is sensitive to 5 m.g. Its construction cannot be intelligibly described without the accompanying illustration.

No. 74, February, 1880.

This number contains no chemical matter.

Chemisches Central-blatt.

No. 12, March 18, 1880.

The Electric Light Applied to the Examination of the Natural Cavities of the Human Body.—This discovery has lately been claimed by Herr Nitze in opposition to M. Gustave Trouvé.

The New Seismograph of M. Galli.—This apparatus cannot be intelligibly described without the accompanying illustration.

The Tommasi Universal Electrical Company.—An exposition of the advantages of the system of electric lighting devised by M. Tommasi.

Justus Liebig's Annalen der Chemie,
Band 200, Heft 3.

Ammonium Nitrite and the Secondary Products Obtained during the Ozonisation of the Air by Moist Phosphorus.—A. R. Leeds.—The chief secondary product of the ozonisation of moist air is phosphoric, not phosphorous acid. The dense white clouds produced, and commonly taken for ammonium nitrite, are chiefly phos-

phoric anhydride. At the end of the experiment no ammonium nitrite is present, even if it is formed previously.

The Hydroxyl-valerianic Acids and Angelic Acids.—Dr. W. v. Müller.—By the oxidation of valerianic acid the author obtains an angelic acid, possessing a much higher melting-point than that of Buchner.

On Cinchona Barks.—O. Hesse.—The author considers that his Cusco bark is probably identical with the aricine-ferous bark of Pelletier and Coriol.

On Gardenin.—J. Stenhouse and C. E. Groves.—The authors describe the extraction of gardenin, the terpen of *Gardenia lucida*, gardenic acid, acetyl-gardenic acid, and hydrogardenic acid.

A New Volumetric Method for the Determination of Manganese and Cobalt.—C. Rössler.—This memoir has been already noticed under *Berichte der Deutschen Chem. Gesell.*, xii., 125.

The Camphen of Borneol and of Camphor.—J. Kachler and F. V. Spitzer.—The two camphens are identical. The derivatives C_{10} , H_{16} , HCl are identical with each other, and with borneol chloride. Camphor is an addition-product of camphen and oxygen.

Annalen der Physik und Chemie.

Heft 3, No. 11, 1879.

Phenomena of Phosphorescence.—B. Stürtz.—The author, along with M. Müller, has extended the experiments of Crookes and Maskelyne upon the phenomena of phosphorescence in a very high vacuum, to a series of substances:—Phosphate of magnesia, wolframite, cerussite, adular, double spar, apatite, Franklinite, lazulite, dolomite, red spinell, cobalt glance stannite, baryta, chrome iron, lepidolite, zinnwaldite, ankerite, greenockite, pectolite, cinnabar, borax, leucite, sanidine, and a meteorite from Java, all of which became phosphorescent. Brucite, magnesite, orthoclase, kaolin, axinite, silicate of zinc, zinc spar, Fergusonite, apophyllite, and celestine appeared phosphorescent only if previously heated to redness.

MEETINGS FOR THE WEEK.

MONDAY, 31st.—Royal Geographical (Anniversary), 2.

TUESDAY, June 1st.—Zoological 8.30.

Royal Institution, 3. Mr. Fiske, "American Political Ideas."

Mineralogical, 8. Prof. M. Forster-Heddle, "On a New Face on Crystals of Stilbite from Scotland and Western Australia." A. E. Arnold (communicated by J. H. Collins), "On a Portable Chemical Cabinet for Quantitative Work." J. H. Collins, "On Kaolinite and Kaolin."

WEDNESDAY, 2nd.—Obstetrical, 8.

Society of Public Analysts, 8. F. P. Perkins, "On the Determination of Organic Carbon in a Water Residue." G. W. Wigner, F.C.S., "Note on the Analysis of some Samples of Chian Turpentine," and "On the Relating of the Scale of Baumé's Hydrometer for Liquids Heavier than Water to the Specific Gravity." C. Heisch, F.C.S., "On the Acts Regulating the Sale of Poisons and Poisonous Colours in Sweden."

THURSDAY, 3rd.—Royal Institution, 3. Mr. T. W. R. Davids, "Buddhist Sacred Books."

Royal Society Club, 6.30.

Chemical, 8. Mr. W. H. Perkin, "On Some Products of the Oxidation of Paratoluidine." A. Dupré, "The Detection of Foreign Colouring Matters in Wine." E. Frankland and D. A. Louis, "On the Action of Zinc Ethyl on Azobenzene." E. Frankland and J. Castell Evans, "On the Action of Zinc Ethyl upon the Azonitriles." W. N. Hartley, "On the Relation of Actinic Absorption to the Chemical Structure of Carbon Compounds." Cuthbert Neison, "On the Luminosity of Calcium Sulphides."

FRIDAY, 4th.—Royal Institution, 8. Mr. H. H. Statham, "An Analysis of Ornament," 9.

Geologists' Association, 8.

SATURDAY, 5th.—Royal Institution, Prof. Morley, "The Dramatists before Shakespeare," 3.

THE CHEMICAL NEWS.

VOL. XLI. No. 1071.

ON A COMBINATION BLOWPIPE FOR GLASS-WORKING.

(PATENT.)

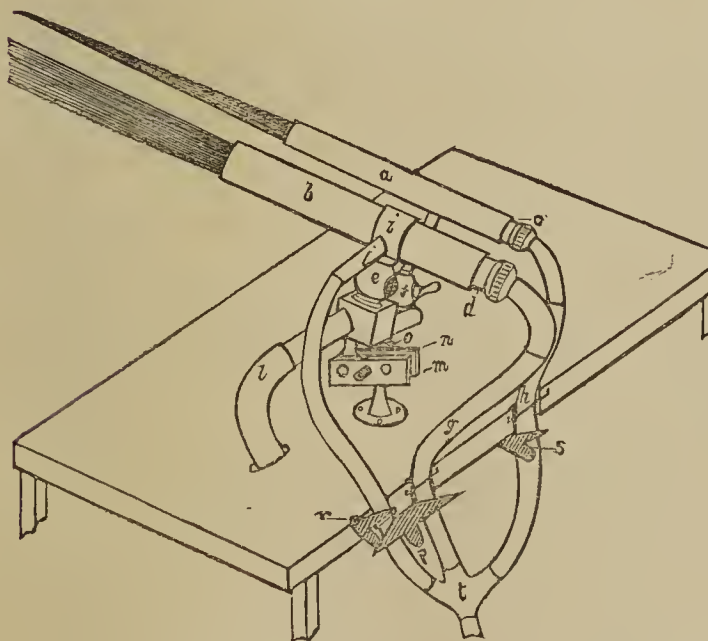
By CHARLES H. GIMINGHAM, F.C.S.

At the present advanced stage of chemical and physical inquiry, when so much depends upon the construction of original apparatus, into the composition of which no material enters so largely as glass, it is impossible to over-estimate the value, to workers in almost all branches of science, of even a slight proficiency in the art of manipulating that substance in the blowpipe flame.

In the course of my experience in experimental science I have frequently met with difficulties in the construction of originally designed glass instruments, which have necessitated improvements in the ordinary glass-blowing apparatus, in order to properly overcome them. Such experiences have led me to devise, and from time to time improve, a blowpipe for myself, with which I may safely say anything in the way of glass work within the limits of possibility may be done with far greater ease than with any ordinary blowpipe.

The description and woodcut given below, taken from the instrument that has assisted me so much in carrying out the experiments in Mr. Crookes's later researches, with which I have had the honour of being so closely associated, will, I trust, be of service to those who wish to be able to construct for themselves small pieces of originally-designed glass work, which are so difficult to obtain from the instrument maker, and which the ordinary workman seems incapable of comprehending.

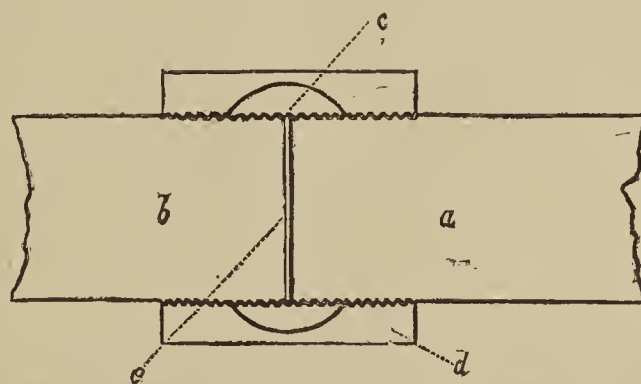
FIG. 1.



a and *b* are two brass tubes, $\frac{1}{2}$ " and $\frac{3}{4}$ " in diameter respectively, *a* being used for the production of the small pointed blowpipe flame, and *b* for the large flame or blast. Gas is supplied to these two tubes separately by the taps *e* and *f*, both of which have long lever handles projecting out to the right of the instrument. I may here mention that these lever handles to the taps are very necessary when rapidity is required in turning the gas on or off, as a knock with the finger up or down is then generally sufficient for the purpose. The inner sliding pieces of

brass tube *c* and *d* carry the air jets, the one for the blast being about 5 m.m., and that for the small 1 to $1\frac{1}{2}$ m.m. diameter. These sliding tubes are necessary in order to adjust the distance of the jets from the end of the tubes of which they form the centre, the form of the flame obtained being greatly dependent upon this adjustment. The jets are supplied with air from foot bellows through the india-rubber tubes *g* and *h*. In the interior of the support (*i*) for the large tube *b*, is turned a groove, into which the small brass pipe, *j*, conducts air from the bellows through the india-rubber tube, *k*. The rate at which the air is forced into the tube *b* from the groove, is regulated once for all by screwing *b* into its support (*i*) until it nearly meets the other part of the tube, which is screwed in from the opposite side, thus forming a narrow slit, through which the air is admitted equally all round (see Fig. 2) Mixing the air with the gas in this

FIG. 2.



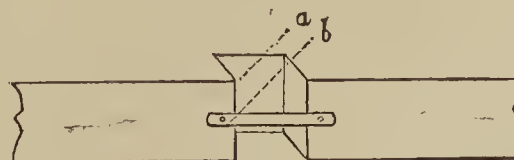
SECTION.

a and *b* are the two parts of the tube *b*, Fig. 1. *d*, the support Fig. 1. *c*, the groove into which the air is conducted by the little pipe *j*, Fig. 1. *e*, the circular slit formed by *a* and *b*, nearly meeting in the centre of the groove.

manner causes its complete combustion at the mouth of the tube, the flame issuing similarly to that from a very large Bunsen burner, but much broader. The india-rubber pipe, *l*, which comes up through a hole in the bench, supplies the gas from the main.

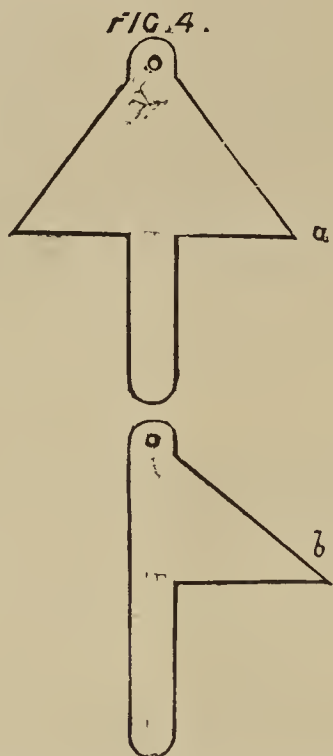
The whole instrument is supported from the bench on some kind of universal joint, which enables the operator to direct his flame in any direction he pleases. The best arrangement for this purpose consists of two brass balls about $\frac{3}{8}$ " diameter, one attached to the instrument, the other screwed into the bench. These two balls are clamped together by two pieces of brass or iron, *m* and *n*, through which passes a thumb-screw, *o*. This forms a joint which will allow the blowpipe to be placed in almost any position, and is capable of far more motion than the ordinary ball and socket, at the same time being very simple and easy of construction.

FIG. 3.



The air from the bellows is divided by a three-way piece, *t*, from which india-rubber pipes, *k*, *g*, and *h*, proceed to the different parts of the blowpipe. These three india-rubber pipes which pass the bench in little nicks (Fig. 3, *a*) cut in its edge, are kept in their places by pieces of brass screwed over them. The passage of the air is stopped by simply kinking the india-rubber tubes, which is quite sufficient for the purpose, and to a great extent

does away with the destruction of the tubes by excessive pinching. This operation of kinking, and the consequent changes in the direction of the wind from the bellows, is brought about most successfully and rapidly by means of brass levers (cut out of metal about $\frac{1}{8}$ " thick) of the shape shown in Fig. 4, *a* and *b*.



Referring back to Fig. 1, it will be seen how these pieces of brass are used. The double one, *a* (Fig. 4), is screwed to the under side of the bench (see *p*, Fig. 1), midway between the india-rubber tubes, *k* and *g*. It will be seen that when the projecting arm of this lever is turned towards the right the tube *g* will be doubled under the bench, and consequently kinked sufficiently to stop the passage of the air from the bellows. On the other hand, if it be turned towards the left the air will be stopped in the tube *k* in the same manner, at the same time opening *g*. In this way we are able to change the flame from the large blue or Bunsen flame to the blowpipe blast, or *vice versa*, by simply pushing this lever to and fro. Two single levers are used of the shape shown in Fig. 4, *b*, and represented in Fig. 1 at *r* and *s*. *r* is screwed on to the upper side of the bench and used to kink *k*, when the little pointed flame is only at work, it being then necessary to shut both the blast and Bunsen off. The use of the lever *s* is obvious from the foregoing explanations, viz., to shut off the air when the small flame is not required.

The use of the instrument will be easily gathered from the foregoing description, and therefore little more need be said regarding it. In order to avoid depositing soot over the room, and the injurious effect of the large flickering flame upon the eyesight, the operator must always use the Bunsen flame for heating up the glass, which necessitates the almost continuous use of the bellows; for this reason a water blower or other source from which a continuous blast might be obtained would be a great luxury, but is by no means necessary. I have always used the ordinary double action bellows, which answer every purpose. In the adjustment of the flames the j-ts should be exactly in the centre of the tubes containing them, and about 10 m.m. below the end of the outer tube in the case of the large flame, and about 15 m.m. in that of the small, but these distances must be slightly varied by sliding the telescope tubes, *c* and *d*, in and out till the desired flames are obtained. The large flame should be equal all round, have a fairly sharp outline, give a steady roar, and heat in the middle as well as at its edge. The small flame should be moderately long, sharp pointed, and require a good deal

of pressure on the bellows to make it hiss. In future numbers of this journal I hope to go more fully into the details of the blowpipe flame and its use as applied to chemical and physical manipulation.

ORGANIC MATTER IN WATER.

By C. T. KINGZETT, F.I.C., F.C.S.

IN a paper* recently communicated to the Chemical Society, I endeavoured to trace the gradual destruction of the organic matter present in water, as induced by animal ferments of the genus *Vibrio*, and to show that as this matter becomes less in quantity it becomes (under generally accepted teachings) more pernicious in quality. It will be convenient to explain the meaning of this statement a little more fully. In his paper, "On the Processes for Determining the Organic Purity of Potable Waters,"† Dr. Tidy, throughout, evidently regards putrescent matters as the most dangerous ones that can be present in potable waters, and he also cites an expression from a publication of Dr. Frankland's bearing the same interpretation, and, further, one from Dr. Angus Smith, to the same effect. Under these circumstances it seemed to me that a water might be found containing such a small amount of organic matter in a given volume that it would be passed by Tidy's process as of "great organic purity," and yet might, under given conditions, pass into a state of putridity, and therefore become pernicious, and I confess this is a difficulty that still remains before me. To instance this:—It is possible to dissolve an amount of gelatine in water, which, upon subsequent examination by Tidy's permanganate method, may be passed as fit for potable purposes, and yet, if we place this water out of intimate contact with oxygen, it will pass into such a putrefactive condition that it would unhesitatingly be condemned by all analysts. And yet the water in this state would use up less permanganate of potassium than in the stage before putrefaction, because this latter process of fermentation necessarily decomposes and destroys the matter in which it is induced.

In connection with this subject it is perhaps desirable to refer to a recent publication,‡ by Messrs. F. Tiemann and C. Preusse. These authors seem to have repeated some of my experiments, and they found (with me) that "the products of putrefaction have a slightly stronger reducing action than the original compound." But had they kept their putrefied solutions a little longer they would have found that the reducing action diminishes very regularly day by day, as I observed during periods of more than a hundred and fifty days.

In explanation of this it must be remembered that putrefaction in its essence is an act of hydration only, no organic matter being lost, but only prepared to undergo rapid oxidation. Later on, this oxidation sets in, and gradually completes itself, at least upon the surface of the solution in the body of which the organic matter has been first hydrated.

In order to record a few further experimental observations, I may be permitted to reproduce here the report of Experiment 4 from my paper above alluded to.

"Expt. 4 (November 3, 1879).—4 or 5 lbs. of raw lean beef was minced and macerated in pure water at about 40° C. The extract contained 3 per cent (?) solid matter, dry at 100° C.

"A. 200 c.c. of this extract was placed in a stoppered bottle of about 250 c.c. capacity.

"B. 200 c.c. of the same extract was placed in a stoppered bottle of this exact capacity.

"C. 50 c.c. was exposed to 47.5 c.c. of air over mercury."

* "Contributions to the History of Putrefaction," Part I., *Journal Chem. Soc.*, January, 1880.

† *Journal Chem. Soc.*, January, 1879.

‡ *Journal Chem. Soc.*, April, 1880 (from *Berichte*, xii., 1906 1924).

Initial Oxygen-capacity of 5 c.c.	After 96 hours.	192 hours.	278 hours.	374 hours.	926 hours.	155 days.
A. . . . 0.0073959	0.006942	0.0064614	0.00631455	0.006942	0.00643552	0.003604
B. . . . 0.0073959	—	—	0.006942	0.007743*	0.00685776	0.00289
C. . . . 0.0073959	Volume of gas in air-tube = 44.5 c.c.			0.0071289	0.00643552	0.003604

* After this examination a quantity of this extract was placed in a bottle of its precise measure, and at the 926 hours' stage the oxygen-capacity was found to be 0.0075712.

It will be seen that I have added to the table as it originally stood, the observations made after 155 days.

Of course it will be understood that in the case of B, after the first examination, the solution was no longer out of contact with air, the volume of which necessarily increased with each examination.

The results just expressed may be put in the following form:—

Grms. of Oxygen used up by a Gallon of the Extract from 36,000 c.c. of Acidified Standard Permanganate (1 c.c. = 0.001334 grm. Oxygen).

	At the Start.	After 39 days.	After 155 days.
A. . . .	6.656	5.79	3.243
B. . . .	6.656	6.17	2.601
C. . . .	6.656	5.79	3.243

Thinking it would be interesting to ascertain, now, the effect of passing a current of air through these solutions, the putrid extract in A was subjected to a good current of air for twenty-four hours, with the effect of reducing its oxygen-capacity from 3.243 to 2.234 grms. per gallon.

Similarly, a current of air was blown through B for seventy-two hours, but in this case the oxygen-capacity was not diminished, apparently. I state this with all reserve, because I omitted to ascertain what loss of water (if any) had occurred by evaporation. In each case the putrid character of the solutions, so far as the senses are concerned, was almost entirely removed.

It would be of extreme interest to make a large series of experiments upon the aerial oxidation of animal and vegetable matters in a state of solution, or solution and suspension, both in the putrid and fresh state, and for such a purpose a current of air passed into the fluid gives some obvious advantages over mere exposure to air.

Now, if it be granted that putrescent matters are pernicious, surely waters containing them should be condemned; and yet, if waters be divided into the four classes, as done by Dr. Tidy (*Journ. Chem. Soc.*, January, 1879, p. 91), it would be possible to introduce something like 50 fluid grains of the putrid extract—referred to above (B) as being examined after 155 days—into a gallon of chemically pure water without removing such from the class of "waters of great organic purity." Similarly 170 fluid grains could be introduced so as to obtain a water of "medium purity," and 255 fluid grains to get a water of "doubtful purity." I cannot help regarding these facts as being of great significance.

Such putrid extracts as here referred to swarm with low forms of life; besides which, they are known to contain *sepsin*, which is a blood-poison, as proved by Burdon-Sanderson.

Without committing oneself to the germ-theory of disease, it really appears that chemical analysis must of necessity fail to discover in waters the germs of contagious diseases, assuming that such germs have a real existence. And, for the matter of that, it is not easy to see how even biological science is going to make any such discovery. Although—as Prof. Huxley reminded us at the Chemical Society the other evening—we may not be sufficiently acquainted as chemists with the teachings of biology, I do not think we can be accused of disregarding altogether the almost undiscoverable and infinitesimal contagious particles, whether they be morphological germs or degraded protoplasts (bioplasts), or neither.

In determining, then, whether a given supply of water is fit for potable purposes, we can only act upon the very broadest basis, and from a general sanitary standpoint, being assisted with our chemical investigations; but neither chemists nor biologists can yet presume to state

whether a given water does or does not contain the infectant of any communicable disease. At the same time I believe, with profound respect for the biologists, the chemists are entitled to give a better judgment than the representatives of any other branch of Science.

Dr. Tidy very pertinently remarked that in enumerating the diseases which appear to be, or which have been proved to be, dependent upon so-called germs (or, perhaps better, "parasites"), Prof. Huxley happened to mention *the only three there are*, viz., relapsing fever, splenic fever, and pig-typhoid; and even the warmest supporters of the germ-theory cannot ignore the existence of the graft-theory, nor the fact that there is a very strong—indeed, at present, an impregnable—position occupied by those who are reluctant to apply to all contagious diseases the explanation that has been, we will say, established for three or four only, and which differ not immaterially in nature from (say) cholera, small-pox, and scarlet fever. For my part I cannot permit myself to forget, among other important researches, those of Dr. Creighton on the infectiveness of cancerous tumours, and they do not by any means adapt themselves to the necessities of the germ-theory of disease.

One other point, and I have done. In his paper on River Water,* Dr. Tidy refers to the fact, apparently well established, "that the progress of such a disease as cholera is not *down* a river at all (as we should expect it to be), but in the opposite direction" (page 322); and he regards the promise made by the Rivers' Pollution Commissioners, of "absolute immunity from epidemics of cholera," conditionally that the Metropolis were supplied with the deep well water of the Kent Company, as at once startling and bold.

I will conclude by quoting, in reference to this subject of cholera, a few words† of Dr. Cunningham, the Imperial Sanitary Commissioner of India, from the Fourteenth Report. He says that "Cholera almost invariably travels, not down, but up the great drainage channels of the country." And again, "Even if it had been shown beyond all manner of doubt that cholera is due to a specific poison, and that this poison resides in the evacua on, it would require very strong proof before the accidental pollution of the water supply over enormous areas could be accepted as the explanation of an epidemic year; for even in a non-epidemic year there is no want of cholera cases amply sufficient to contaminate the water. But as the very existence of the cholera germ has itself still to be established, as well as the opinion that it resides in the discharges, not only is the chain of argument imperfect, but every important link in it seems to be wanting."

THE ALKALOIDS.

On the occasion of the visit to London of Dr. Burggraave, Emeritus Professor of Anatomy and Surgery of Ghent University, and Member of the Royal Academy of Medicine of Belgium, several eminent members of the medical profession met, on the 27th ult., at the residence of Dr. Phipson, F.C.S., to listen to a discourse by the veteran Belgian Professor, in which he disclosed the history of his researches upon the alkaloids, and their application to the treatment of diseases by what is termed the dosimetric method.

Professor Burggraave, in spite of his great age, addressed the meeting with wonderful fluency and without the

* *Journ. Chem. Soc.*, May, 1880.

† *Practitioner* for August, 1879.

slightest fatigue for upwards of two hours, illustrating his remarks by numerous interesting cases, taken for the most part from his own practice. His object was to show how such powerful agents as strychnine, hyoscyamine, aconitine, veratrine, &c., can be applied in medicine as safely and efficiently as quinine in cases of intermittent fever. He demonstrated their action in fevers of all kinds and in other forms of disease, and insisted energetically upon the absolute necessity of a complete reform in our present system of therapeutics.

ON THE
RECOGNITION OF ALIZARIN, ISO-PURPURIN,
AND FLAVO-PURPURIN IN MIXTURES,
AND ON THE
QUANTITATIVE DETERMINATION OF ALIZARIN.

By E. SCHUNCK and H. ROEMER.

ALIZARIN, iso-purpurin, and flavo-purpurin display so characteristic reactions that their recognition, especially in a state of purity, presents no difficulty. It is otherwise when they are present in a mixture, the separation of which into its constituents, each free from admixture with the others, is difficult, or even impossible, when the quantity at disposal is small. These bodies, and also their compounds, vary little from each other in their behaviour with solvents, and the characteristic reactions above-mentioned are chiefly changes of colour, which become untrustworthy, or at least less distinct, as soon as the respective bodies are not met with in a state of purity. The authors have searched for a practicable process for their separation or recognition when small quantities only are concerned. They have succeeded by fractionated sublimation. Alizarin begins to sublime at 110° , flavo-purpurin at 160° , and iso-purpurin at 170° . The first-mentioned body may be easily removed from a mixture of the three if the temperature is kept below 160° . Greater difficulty attends the sublimation of flavo-purpurin from the residual mixture with iso-purpurin. This, however, is scarcely necessary, as the two compounds may readily be distinguished under the lens in a sublimate obtained above 170° . Iso-purpurin sublimes in massive well-developed crystals, whilst flavo-purpurin forms fine reddish yellow needles. These compounds can, moreover, be separated in the moist way, since flavo-purpurin dissolves readily in benzol, in which iso-purpurin is almost insoluble. The purpurins, when separated in this manner, show distinctly the properties described by the authors in *Berichte*, ix., 680. The operation is performed by simply heating the mixture in an air-bath between two glass disks kept a few millimetres apart by a leaden ring.

As the mixture is not carbonised even at 200° , the authors have applied the process to the quantitative determination of alizarin in such mixtures as the above. Such an application is the more valuable, as commercial alizarins almost invariably contain an admixture of the purpurins, and as hitherto no ready method of determining the proportion of actual alizarin has been published.

As an experiment the authors exposed a mixture of pure flavo-purpurin and alizarin to a prolonged heat of 140° . From time to time the sublimate was removed from the upper glass, and heat was again applied till it ceased to be re-formed. This may easily be ascertained by examination with the microscope or by moistening the upper glass with a solution of potassa. The weight of the residue then became constant. The mixture was composed of 90.91 flavo-purpurin and 9.09 per cent alizarin. The analysis showed 8.91 per cent of the latter. On examining commercial alizarins in this manner it is recommended first to remove any anthraquinon, oxy-anthraquinon, anthra- and iso-anthraflavic acid present.—*Berichte der Deutschen Chemischen Gesellschaft*.

A METHOD FOR THE PROXIMATE ANALYSIS
OF PLANTS.*

By HENRY B. PARSONS.

AT the request of my friend and former instructor, Prof. Albert B. Prescott, of the University of Michigan, I have prepared the following scheme for the analysis of plants. This method will appear, in substance, in his new *Proximate Organic Analysis*, now nearly completed.

The plan submitted is the outgrowth of a quite varied experience in the proximate analysis of plants; no claim to originality is made, the sole aim being to arrange in one simple scheme those methods best suited to ensure accuracy.

It must be premised that no one method is applicable in all cases, and that the operator will so modify and adapt the proposed processes as to best attain the truths he seeks. If the present scheme shall serve merely as an example, to be improved upon as discoveries multiply, it will at least have served to stimulate to the more thorough study, this side the Atlantic, of a much neglected, yet very important, branch of analysis. The American student, when first entering upon the study of plant analysis, is perplexed and disheartened, owing to the lack of any elementary treatise in which he may find directions for the quantitative estimation of the various plant constituents. The works of Rochleder and Wittstein, while giving most valuable assistance in the investigation of special constituents and their separation from large quantities of the crude herb, still fail to give clear and practicable directions for the quantitative estimation of each constituents. Von Mueller's latest enlarged edition of Wittstein's *Plant Analysis* gives a scheme, most excellent in many respects, yet cumbered with tiresome methods of extraction and manipulation, which serve to unnecessarily lengthen the time required for making the analyses without increasing the accuracy of results obtained.

Too many American analyses of plants have been summarised thus: "The plant contains gum, resin, tannin, a volatile oil, and a peculiar bitter principle, to which may be ascribed its medicinal activity." The foreign journals bring occasionally most excellent examples of accurate examinations of vegetable substances; as instances may be cited the examination of ginger, by J. C. Thresh,[†] and of ergot,[‡] aloes,[§] and other articles, by Prof. Dragendorff. To these sources the student must look for his best models, until a more thorough and systematic training is given American students in proximate organic analysis.

In following the plan now presented, the use of the apparatus for re-percolation is strongly urged for the extractions with benzole, alcohol, and other volatile solvents. A very simple and inexpensive apparatus has been described by various American and foreign chemists.||

"In any convenient water-tight vessel is a worm of block tin pipe, having an internal diameter of 9 m.m., and a length of about 2.5 metres. The lower (external) part of this worm is fitted, by an ether-soaked, velvet cork, to a glass percolator having a diameter of 4 c.m., a length of 20 c.m. to the constriction, and 5 c.m. below. Within this percolator is a smaller tube, flanged at the top and bottom, and suspended by fine platinum or copper wires. This tube has a diameter of 2.5 to 2.8 c.m., and a length of 14 c.m.; the bottom is covered by filter paper and fine washed linen,[¶] tied on by linen thread. The weighed

* From the *American Chemical Journal*, vol. i., No. 6.

† *Pharm. Journ. Trans.* (3), 10, 81, August, 1879.

‡ *Pharm. Journ. Trans.* (3), 6, 1001, June 17, 1876.

§ *Werthbestimmung*, 1874, p. 110.

|| B. Tollens, *Zietsch. f. Anal. Chem.* (17), 320, 1878; *New Remedies*, (7), 335, November, 1878; W. O. Attwater, *Proc. Am. Chem. Soc.* (2), 2, p. 85; S. W. Johnson, *Am. Journ. Sci. Arts*, (13), 196; H. B. Parsons, *New Remedies*, (8), 293, October, 1879.

¶ In place of the linen and filter paper may be substituted fine brass or platinum wire gauze. Asbestos suspended in water may then be poured in to form a fine felt. The tube can then be dried and weighed, and the amounts extracted may be found by the loss of weight of the tube and substance. A little experimentation will show the operator how to prepare and use the tube. It is but an adaptation of the Gooch's filter here recommended.

sample of the finely powdered herb is placed within this tube for extraction. A light glass flask, weighing about thirty grms., is fitted by an ether-soaked cork to the outer percolator." Having introduced the solvent into this glass flask, the connections are made secure, and heat is applied, by a water-bath, to the flask. If the liquid is too slowly volatilised, the addition of a little common salt to the water in the bath serves to remove the trouble.

Next in importance is the use of a good tared filter. The form originally presented by F. A. Gooch* leaves little to be desired. It may be made by perforating with fine holes the bottom of an ordinary platinum crucible, and fitting it accurately to a perforation made in a large rubber cork; this cork connects it with a receiving vessel, which, in turn, is connected with a Bunsen's pump. Fine asbestos suspended in water is poured into the crucible, the air exhausted from the receiving vessel, add thus a firm thin layer of asbestos is deposited on the bottom of the crucible. After ignition and weighing, the crucible is ready for the reception of any precipitate which it is desired to separate and weigh.

The use of these two pieces of apparatus will eliminate two grave sources of error, viz., incomplete extraction of soluble matters, and inaccuracies introduced by the use of tared paper filters.

The other necessary apparatus is simple, and includes one or more platinum crucibles and evaporating dishes, accurate burettes and graduated cylinders, a good balance sensitive to at least 0.5 milligram, and the ordinary glass and porcelain ware found in all laboratories.

It is assumed that whoever attempts the analysis of a plant is informed as to the normal constituents to be sought, that he has had considerable experience in inorganic analysis, and in the identification of the principal classes of proximate constituents which he now undertakes to estimate quantitatively. Accordingly tests for identification will not be here presented; they should, however, never be omitted. The necessity of recording in detail all physical and chemical peculiarities, with every weight that is taken, is self-evident.

A METHOD FOR THE PROXIMATE ANALYSIS OF PLANTS.

I. Preparation of Sample.

The air-dry specimen should be carefully examined, and all extraneous substances removed. The entire sample should then be ground, or beaten in an iron mortar, until it will all pass through a sieve having from forty to sixty meshes to the linear inch. After thoroughly mixing this sample, take of it about a hundred grms., which should be further pulverised until it will all pass through a sieve having from eighty to one hundred meshes to the linear inch. From this smaller portion remove all iron, derived from mill or mortar, by use of a magnet. Then place in a clean dry bottle, which should be labelled and securely corked. This small sample is for the analysis; the larger portion should be reserved for the separation of those proximate principles which seem, from the analysis, to be worthy of more extended investigation.

II. Estimation of Moisture.

Dry rapidly, at 100 to 120° C., two or more grms. of the sample; the loss of weight equals moisture, and occasionally a little volatile oil. In some cases it is best to dry at a lower temperature, and at other times the drying should be conducted in a stream of hydrogen or carbonic anhydride.

III. Estimation of Ash.

In a weighed crucible gently ignite two or more grms. of the sample until nearly or quite free from carbonaceous matter; the heat should not be permitted to rise above faint redness, or loss of alkaline chlorides may occur. Weigh this residue as *crude ash*, and in it determine:

a. Amount soluble in Water.—This portion may contain chlorides, sulphates, phosphates, and carbonates of potas-

sium and sodium, also slight amounts of chlorides and sulphates of calcium and magnesium.

b. Insoluble in Water; Soluble in dilute Hydrochloric Acid.—The residue from *a* should be treated with a slight excess of hydrochloric acid, and evaporated in a porcelain dish over a water-bath until all free acid has been expelled; it should then be again moistened with hydrochloric acid, water added, and be filtered from any remaining insoluble substances. This treatment removes carbonates (with decomposition) and phosphates of calcium and magnesium, sulphate of calcium, and oxides of iron and manganese.

c. Insoluble in Water; Insoluble in dilute Hydrochloric Acid; Soluble in concentrated Sodid Hydrate.—Boil the residue from *b* with a solution containing about twenty per cent of sodic hydrate. This treatment removes combined silica of the ash. The residue still insoluble is sand and clay which adhered to the specimen; this residue should be separated, washed thoroughly, and weighed.

Always determine the amounts removed by the above treatment by weighing the dried, undissolved residues. The ash, as thus estimated, usually includes a little unconsumed carbon, together with more or less carbonic anhydride (CO₂), most or all of which was not originally present in the plant, but was produced by the combustion of the organic matter. For most purposes it is unnecessary to estimate and exclude from the ash this carbonic anhydride; where great accuracy is desired, a complete quantitative analysis should be made, the amount of each base and acid being determined, and in the statement of results only those should be included which existed originally in the plant. For this purpose it is necessary to burn from twenty to one hundred grms. of the sample; for further directions consult text-books on agricultural and inorganic analysis.

IV. Estimation of Total Nitrogen.

In half a gram or more of the sample determine total nitrogen by combustion with excess of soda-lime, as directed by Prof. S. W. Johnson and E. H. Jenkins.* If later in the analysis no other nitrogenous substances are discovered, calculate the total amount of nitrogen to albuminoids by multiplying by 6.25. When other nitrogenous compounds are present, their content of nitrogen should be determined directly or by difference; after proper deductions have been made, the remaining nitrogen should be calculated to albuminoids.

V. Estimation of Benzole Extract.

In a suitable apparatus for re-percolation completely exhaust 5 grms. of the sample with pure coal-tar benzole (specific gravity 0.85–0.88, boils at 80–85° C., leaves no residue when evaporated). The extraction requires from four to six hours' continued action of the solvent. Carefully evaporate this liquid to dryness in a weighed dish, and record its weight as *total benzole extract*. This extract may contain volatile oils and other aromatic compounds, resins, camphors, volatile or non-volatile organic acids, wax, solid fats, fixed oils, chlorophyll, other colours, volatile or fixed alkaloids, glucosides, almost no ash.

To the weighed extract add water, again evaporate on the water-bath, and complete the drying in an air-bath at 110° C. In absence of other vapourisable substances the loss of weight approximates the amount of *volatile oil*. If the presence of a volatile alkaloid is suspected (from a characteristic odour or an alkaline reaction), add a drop of hydrochloric acid to prevent its volatilisation. Camphors are partially dissipated by this treatment; hence, when they are present, this evaporation should be dispensed with.

Treat, now, the residue with a moderate amount of warm water, allow to stand until cool, then filter through fine paper by aid of a Bunsen's pump. In half of the aqueous filtrate determine *total organic matter* and *ash*; test the remaining half for *alkaloids*, *glucosides*, and *organic acids* by salts of lead, silver, barium, and calcium.

* *Proc. Amer. Acad. Sci.* (13), 342, 1878; *New Remedies* (7), 290, October, 1878; *Amer. Chem. Journ.*, 1, 317.

* "Report Conn. Agric. Exp. Station, 1878; *CHEMICAL NEWS* vol. xl., p. 28; *American Chemical Journal*, 1, p. 77.

Care must be taken not to mistake a slight amount of suspended matter, frequently resinous, for other substances actually soluble in water.

The still undissolved residue should be again removed from filters and dishes by solution in benzole, the benzole solution being again evaporated to dryness. Treat this residue with warm, very dilute hydrochloric acid, allow to cool, and filter through paper. The filtrate should be tested for *alkaloids* and *glucosides*. The amount extracted by acid, if any, may be determined by weighing the still undissolved residue. Treat this residue with several considerable portions of eighty per cent alcohol (specific gravity 0.8483 at 15.6° C.), allowing at least an hour for each treatment. Filter through paper and determine by evaporation the matter dissolved; this usually consists of *chlorophyll* with one or more *resins*, which may sometimes be separated by use of petroleum naphtha, chloroform, or similar solvents. Purified animal charcoal removes *chlorophyll* and *some resins* from alcoholic solution, while certain other resins are not removed. If *camphors* were present in the plant the greater portion will be found in the alcoholic liquid.

The substances undissolved by eighty per cent alcohol may be *fixed oil*, *solid fat*, *wax*, and very rarely a *resin*: their separation may be attempted by refrigeration and pressure, or by use of ether, chloroform, &c.

Recapitulation.

1. Loss by evaporation, with precautions: *volatile oil*.
2. Soluble in water: *alkaloids*, *glucosides*, *organic acids*.
3. { Insoluble in water. } : *Alkaloids*, possibly *glucosides*
 { Soluble in dilute acids. }
4. { Insoluble in water. } : a. { Removed by animal charcoal: *chlorophyll*, *some resins*.
 { Insoluble in dilute acids. } :
 { Soluble in 80 p.c. alcohol. } b. { Not removed by animal charcoal: *some resins*.
5. { Insoluble in water. } : *wax*, *fats*, *fixed oils*.
 { Insoluble in dilute acids. }
 { Insoluble in 80 p.c. alcohol. }

It is frequently advantageous to extract the plant with petroleum naphtha (specific gravity 0.66—0.70, boils at about 50° C., wholly volatile) before treatment with benzole; by reference to the accompanying table of comparative solubilities it will be seen that this treatment may serve to separate fixed and volatile oils and some resins and colours, from certain solid fats, wax, other resins and colours.

Where benzole of sufficient purity cannot be had, pure chloroform is the best substitute. The use of ether is objectionable in this place, and its solvent properties are less distinctly marked than are those of naphtha, chloroform, and benzole; in other words, more plant constituents are sparingly soluble in ether than in the above mentioned solvents. Consequently many substances which should properly be extracted by 80 per cent alcohol, would be sparingly dissolved if ether were used, while benzole, chloroform, and naphtha would have no perceptible solvent action upon them; tannic acids may be cited as instances illustrating this point.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

A MEETING of this Society was held on May 22, at Cambridge. On arrival there the party had luncheon at St. John's College, which had been kindly placed at the disposal of the Council by the College authorities. Prof. W. G. ADAMS occupied the Chair; and Mr. WARREN DE LA RUE proposed a vote of thanks to the Master and Senior Fellows of the College for providing the hall.

After some remarks from Prof. ADAMS, the party proceeded to the Cavendish Laboratory, where Lord RAYLEIGH, Vice-President of the Society, presided.

The routine business of the meeting being waived, Lord RAYLEIGH described a plan for limiting the slit of a

TABLE OF COMPARATIVE SOLUBILITIES.

Substances.	Water.	Ab. Alcohol.	80 per cent Alcohol.	Ab. Ether.	Chloroform.	Benzole.	Pet. Naph.	10 per cent Ammonia.	Ammoniac Cupric Oxide.	H ₂ SO ₄ H ₂ O Sp. gr. 1.78.	Fehling's Solution.	Lead Subacetate.
Volatile oils ..	Sp	Sol	Sol	Sol	Sol	Sol	Sol	Sol	—	—	—	—
Fixed oils ..	Ins	Ins?	Ins	"	"	"	"	"	—	—	—	—
Wax ..	"	Sp	Sp	Sp	Sp	"	Sp?	Ins	—	—	—	—
Solid fats ..	"	"	"	"?	"?	"?	"?	"?	—	—	—	—
Chlorophyll ..	"	Sol	Sol	Sol	Sol	"	"	"	—	—	—	—
Soft resins ..	"	"	"	"	"	"	Sol	"?	—	—	—	—
Hard resins ..	"	"?	"	"?	"?	"?	"?	Sol?	—	—	—	—
Glucose ..	Sol	Ins	Sp	Ins	Ins	Ins	Ins	"	—	—	Reduced	Not prec
Sucrose ..	"	"	"	"	"	"	"	"	—	—	Not reduced†	"
Tannin ..	"	Sol	Sol	Sp	"	"	"	"	—	—	Reduced	Prec
Glucosides ..	"?	"?	"?	Sol?	Sol?	Sol?	Sol?	"?	—	—	"†	Not prec
Alkaloids ..	"?	"?	"?	"?	"?	"?	"?	"?	—	—	—	"
Albuminoids ..	"?	"?	"?	Ins	Ins	Ins	Ins	Ins?	—	—	—	Prec
Gums ..	"	Ins	Ins	"	"	"	"	Sol?	—	—	Not reduced	"
Pectin ..	"	"	"	"	"	"	"	"?	—	—	—	"
Pectic acid ..	Ins	"	"	"	"	"	"	"	—	—	—	"?
Organic acids ..	Sol?	Sol?	Sol	"?	"?	"?	"?	"?	—	—	—	"?
Salts of org. acids ..	"?	"?	"?	"?	"?	"?	"?	"?	—	—	—	"?
Starch ..	Ins	Ins	Ins	"	"	"	"	Ins	—	—	—	—
Cellulose ..	"	"	"	"	"	"	"	"	Sol	Sol	—	—
"Para cellulose"	"	"	"	"	"	"	"	"	Ins*	"	—	—
"Meta cellulose"	"	"	"	"	"	"	"	"	"	"	—	—
"Vasculose"	"	"	"	"	"	"	"	"	"	Ins	—	—
"Extractives" ..	Sol	"?	Sol?	"	"	"	"	Sol?	—	—	Reduced?	Not Prec
Colours ..	"?	"?	"?	"	"	"	"	"?	—	—	—	Prec

* "Para cellulose" soluble in ammonia-cupric oxide after boiling with dilute HCl.

† Glucosides reduce Fehling's solution after boiling with dilute acids; same with sucrose.

A? shows that some marked variations or exceptions occur. Sp=sparingly soluble. Sol=soluble. Ins=insoluble.

telescope so as to alter the angular interval with which it can deal. The interval is measured by means of a grating formed by winding a fine wire round two parallel screws of very fine thread.

Mr. SHAW exhibited a modification of Weinholdt's apparatus for distilling mercury, by which a kilogramme of mercury can be distilled per hour.

Mr. SEDLEY TAYLOR exhibited a device for showing the motion of the particles of water in the transmission of a surface wave. Sixteen discs were arranged in single file, each having a white spot on its face, and on turning a handle the discs rotated so that the spots, which represented particles of water, moved so as to present a wave motion to the eye. Mr. Taylor also showed a manometric flame apparatus for exhibiting to the eye the difference of phase between two musical notes. This consisted in two bent tubes, into which the notes were sounded, and capable of being lengthened or shortened by hand like the pipes of a trombone. Opposite the ends of each of these tubes a sensitive flame was placed, and a rotating mirror showed the disturbance produced in the flames by the two different notes. A third flame exhibited the joint effect of the two notes. When the tubes were silent the images of the flames on the revolving mirror were seen as plane bands; but when notes were sounded into the tubes they became serrated, and the serrations were like or unlike, according as the phases of the notes were like or unlike.

Mr. POYNTING exhibited a plan for altering the plane of polarisation of the two halves of a pencil of rays from the polariser, so that half the field may be made to appear dark when the other is bright, or both of equal brightness, at will.

Mr. Glazebrook described a method of measuring the rotation of the plane of polarisation of light by means of two spectra giving dark lines made to coincide.

Lord RAYLEIGH described a plan for demonstrating that yellow colour can be formed by combining red and blue together. He mixes a red solution of chromate of potash with a blue solution of litmus, and on filling it into a glass cell of a certain thickness, the light transmitted through it is seen to be yellow. Plates of glass coated with gelatin impregnated with litmus, and gelatin impregnated with chromate of potash, and placed side by side, also transmit yellow light. Lord Rayleigh finds, however, that the eyes of different persons vary considerably in their power of appreciating the tinge of the transmitted yellow; one deeming it greenish, another reddish, while a third considers it pure yellow. This peculiarity is not to be confounded with "colour blindness," since all three persons would distinguish the red and green components accurately. Lord Rayleigh also exhibited a colour-box, based on the Newtonian principle, first carried out by the late Prof. Clerk-Maxwell, but of a small size.

Sir W. THOMSON then proposed a vote of thanks to Lord Rayleigh, which was seconded by Prof. W. G. ADAMS, and the meeting then dispersed to examine the apparatus and appointments of the Cavendish Laboratory.

CORRESPONDENCE.

AMMONIA FROM ATMOSPHERIC NITROGEN.

To the Editor of the Chemical News.

SIR,—I suppose Mr. Thompson hardly expects everyone to accept his interpretation of the two patents referred to in his note, viz., the patents of Messrs. Rickman and Thompson, and Clarke and Smith. With your permission I should like to say a word or two on the subject.

If I understand Mr. Thompson's note rightly, the gist of it is that in his opinion the process under R. and T.'s patent differs widely from that of C. and S.'s patent, and mainly on the following counts. Mr. Thompson says—

First. That the former take "N as it is in the atmosphere," and raise its temperature to that of incandescent coal.

Secondly. That they use "vapour of water," not superheated steam (S.H.S.); again necessarily at a temperature not lower than incandescent coal.

Thirdly. That they employ "incandescent coal as dust or slack," not made into bricks.

Fourthly. That they work at a "comparatively low" heat.

Mr. Thompson further asserts that ammonia and cyanides cannot be formed or exist at high temperatures. One would suppose from this that C. and S. do not take "N as it is in the air," nor use "vapour of water" nor "incandescent coke," and so forth; yet, according to their patent,—

First. They use "N as it is in the air," and raise it to the temperature of incandescent carbonaceous fuel.

Secondly. They use "vapour of water" in the form of S.H.S., and decompose it by means of an incandescent material, essentially the same as R. and T.; for it is idle to argue that "vapour of water," impinging upon an incandescent body, differs in any way from S.H.S. under similar conditions, or that the result of the reaction differs.

Thirdly. They employ "incandescent coal or coke," in conjunction with certain salts,—by preference, however, and for the sake of economy, pressed into bricks.

Fourthly. As to temperature, the expressions "comparatively low" and "intensely high" are very vague and indefinite, and depend entirely upon the circumstances of the case. It would appear, however, that the "comparatively low" heat of R. and T. is of an "intensely high" type, for they decompose their water vapour at—indeed all their reactions are necessarily carried on at, and are fundamentally based upon—a temperature not lower than that of coal in an "incandescent state," and so intensely heated as to bring about the decomposition of "water vapour"—at least this is Mr. Thompson's statement. But such a heat can only by straining the meaning of words be described as "comparatively low," and is utterly inconsistent with the actual words of their patent, viz., "a moderate heat, in a slow-burning furnace" supplied "with a small amount of air mingled with water vapour." My experience, the result of much experimental work, leads me to the conclusion that it is only at high temperatures—say from full red upwards—that atmospheric N is rendered available; whatever NH_3 is formed at low temperatures comes from the nitrogenous fuel employed, or some such analogous source; certainly not from that most inert and intractable of all chemical elements, atmospheric nitrogen.

With respect to the decomposition of cyanides and ammonia at high temperatures, which Mr. T. so positively affirms, it is well to bear in mind that the only available source of NH_3 at the present moment is the "gas liquor" resulting from the dry distillation of coals at high heats. The temperature of gas retorts ranges from full cherry-red to nearly if not quite a white heat, and the higher the heat the greater the yield of NH_3 . Further, it is well known that, in the exceedingly high temperatures of iron blast-furnaces, ammonia, cyanides, and even free hydrocyanic acid, are abundantly produced; indeed the two latter have an important bearing on the actual process of reduction going on in the furnace. According to Bunsen and Playfair, who fully investigated the subject, an ordinary English blast-furnace, fed with coal as fuel, produced daily 225 lbs. of metallic cyanides, whilst the gases evolved from the furnace contained such a large amount of ammonia that the presence of that gas in the lower part (the hottest therefore) of the blast-furnace was even perceptible to the smell. They proposed to pass the gas into HCl , and thus obtain sal-ammoniac. Experiments at Alfreton Iron-Works proved that 2.44 cwts. of sal-ammoniac could be produced daily in this way. (*Vide* Wagner's "Technology," pp. 15 and 16.)

On the whole, I am inclined to think it would prove a not uninteresting experiment if Mr. Thompson would substitute a non-nitrogenous medium to decompose his water vapour, adhere to his "moderate" heat, small supply of air, &c., and note the result.

One word more. Mr. Thompson goes a little out of his way to indulge in a vein of prophetic banter, which, by the way, I rather enjoy. I think it was Punch who suggested "Don't prophesy, unless you know." I hope, without impropriety, I may recommend this maxim to the notice of Mr. Thompson. So long as he speaks of that he knows I suppose he is on tolerably safe ground, but the moment he goes beyond he is at once "up a tree," and is lost "in the clouds." I trust it will not be too startling to him to hear that an experimental furnace, some 6 or 7 feet high, that had been in pretty constant work of a severe character for some months, and which had necessarily to be removed, not only still held together, but the difficulty was to get it asunder. Considering the ordeal it had passed through it was found to have suffered little, and to be still capable of doing a very considerable amount of work.

Perhaps I ought to explain that the leading idea in Clarke and Smith's patent is to decompose water, utilising the H in the formation of NH_3 , and the oxygen for oxidising purposes, notably in the production of H_2SO_4 .—I am, &c.,

E. S.

May 25, 1880.

ERROR IN VAPOUR DENSITY DETERMINATIONS.

To the Editor of the Chemical News.

SIR,—I have noticed in nearly all the descriptions of Dumas' Method of Vapour Density Determinations an error, which appears not only in old books, but has been repeated in standard publications up to a recent date without, I believe, general attention having been drawn to it. It is rather remarkable that it has become so universal seeing that it is so obvious.

In making the first weighing of the globe, importance is attached to pressure and temperature observations, assuming that the weight of the vacuous globe is required, to obtain which the weight of the contained air must be subtracted. It is evident, however, that the numbers obtained in this first weighing will be the same whatever the density of the atmosphere may be, providing, of course, that both halves of the balance are similar and neglecting small variations resulting from the difference in the specific gravity of the weights and material of the globe. When, however, the globe is sealed and weighed the second time, variations of atmospheric density will cause variations of numbers obtained, because the density of the contents of the globe is now no longer at liberty to vary with the surrounding medium, and it is at this stage of the experiment only (excepting of course pressure observation at time of sealing) that the conditions of the atmosphere have to be noticed.

The only publications I remember to have noticed containing the correct method are,—*"Organic Chemistry,"* by W. M. Watts, published by W. Collins Sons and Co., and *"The New Chemistry,"* by J. P. Cooke Junr., published by H. S. King and Co.

In the description therein given, instead of considering the vacuous globe and making a deduction from the first weighing, a correction for buoyancy is added to the second. The results will be the same, which of the two methods he used, providing the weight of air deducted or added is governed by the temperature and pressure observed at the second weighing, though the latter is certainly more logical.

Not only have I observed that the above fallacy has occurred in nearly all publications for at least thirty years hitherto, *e.g.*, *"Regnault's Chemistry,"* *"Rose's and Fresenius' Quantitative Analysis,"* *"Watts' Dictionary,"*

"Deschanel's Natural Philosophy," &c., but in one book, which is certainly an excellent and masterly treatise on the elements of organic chemistry, an unexpected and amusing statement to this effect—"To correct the volume for temperature and pressure, multiply by—

$$\frac{273 \times P \text{ m.m.}}{(273 + t^\circ) \times 760} \quad \text{or} \quad \frac{273 \times 760}{(273 + t^\circ) \times P \text{ m.m.}}$$

accordingly as P m.m. is greater or less than 760."

These facts suggest that whatever importance is attached by all the text-books to the determination of vapour densities, yet the actual experiment is very rarely included in the practical course of a student. Had the experiment been often repeated by students in their course of laboratory instruction, surely the directions for their guidance would have been rectified ere this.—I am, &c.,

J. BARNES.

RIVER WATER.

To the Editor of the Chemical News.

SIR,—I beg leave to supplement the abstract of my remarks on "River Water," contained in the *CHEMICAL NEWS*, vol. xli., p. 248, with some brief additional reference to their completer form.

(1.) The ratio—

Oxygen consumed

Sum of organic carbon and nitrogen

has the following mean values in the cases specified, viz.:—

	Ratio.
Filtered Thames	3.42
Filtered Lea	4.09
Shannon	2.74

These numbers are taken from Dr. Tidy's recent paper (*Journ. Chem. Soc.*, May, 1880). It is not, therefore, possible to calculate, from the "oxygen consumed" by any given water, how much organic matter it contains. Moreover, as Dr. Tidy's figures show, the ratio is not constant even for the same water, being sensibly higher in winter than in summer. Thus the "oxygen process," unless in special cases, furnishes us with little more than a mere suggestion.

(2.) Peaty water, if stationary and exposed to sunlight, is most undoubtedly bleached.

(3.) River waters, and especially peaty waters, contain a jelly-like constituent, which, if present in quantity, is gradually deposited on the rough surfaces or particles through which, or with which, they flow. I have myself seen peat jelly and observed its deposit. This deposition might account, in my opinion, for some part of the purification of a peaty or other water by flowing through a rough channel.

(4.) The weeds in a river's bed, into which sewage is poured, constitute a natural sewage farm, capable of absorbing chlorides, phosphates, and organic matter in the ordinary way. A great deal of the purification of the Thames must be owing to this cause, ordinarily never considered. I should regard with grave apprehension any proposal to dredge the bed of the Thames above London.—I am, &c.,

EDMUND J. MILLS.

Glasgow, May 31, 1880.

THE PROPOSED SOCIETY OF INDUSTRIAL CHEMISTRY.

To the Editor of the Chemical News.

SIR,—In your article (*CHEMICAL NEWS*, vol. xli., p. 243) the definition of a "Chemical Engineer" that I proposed is not quite correctly reported. Kindly allow me to put myself right.

I defined a "Chemical Engineer" to be "a person possessing chemical and mechanical knowledge, and who

applies that knowledge to the utilisation of chemical action."

To omit chemistry from the equipment of a chemical engineer seems to me, to me an old simile, like the play of *Hamlet* with *Hamlet* left out.—I am, &c.,

EUSTACE CAREY.

Widnes, June 1, 1880.

PROPOSED TECHNICAL SOCIETY.

To the Editor of the Chemical News.

SIR,—As a supplement to your remarks in the *CHEMICAL NEWS* (vol. xli., p. 243) relative to the above Society, would you kindly allow me to inform your readers of the composition of the committee and a few other matters.

The meeting held at the Owens College, Manchester, was well attended by technical chemists, from whom the following were selected to form the committee:—

Prof. Roscoe (Chairman), Mr. E. K. Muspratt (Vice-Chairman), Prof. Thorpe, Dr. Angus Smith, Dr. Campbell Brown, Dr. Hewitt, Dr. Hurter, Dr. Schunck, Mr. J. F. Allen, Mr. E. Carey, Mr. H. Crossley, Mr. A. E. Fletcher, Mr. J. C. Gamble, Mr. H. Gaskell, Mr. J. Hargreaves, Mr. W. D. Hermann, Mr. Ludwig Mond, Mr. J. L. Muspratt, Mr. Peter Spence, Mr. George E. Davis (Hon. Sec., *pro tem.*).

The above committee was formed at the suggestion of Mr. Eustace Carey, who brought forward a proposition of Mr. Hugh Lee Pattinson for the formation of a Society of Chemical Engineers. Mr. Carey described the Society as:—

"A Society to promote the application of chemical science to manufacture, and the acquisition of that species of knowledge which constitutes the profession of a Chemical Engineer." And—

"A Chemical Engineer is a person who possesses chemical and mechanical knowledge, and who applies that knowledge to the utilisation, on a manufacturing scale, of chemical action."

From what I could gather from the speeches made at the meeting, there were many present who considered it advisable to form a Society of Chemical Engineers, though a great many were in favour of calling such a society The Society of Technical Chemists; but the majority were fairly agreed that not mere chemists were meant, but chemists with a thorough knowledge of physics combined with a fair knowledge of mechanics. Such men are not very plentiful we know, but they would, by forming such a society, help to diffuse that knowledge through the next generation, if not in the present. In the existing state of chemical industry the need of competent men to advise is becoming more and more felt, and in proof of this we have only to contrast the following processes carried out successfully by competent advisers, chemical and mechanical, with a whole host of bogus concerns, some of which, though based on sound chemical principles, have gone to the wall.

The successful processes which may be held up as examples of skill in chemical engineering are:—The Weldon Chlorine Process, the Deacon Chlorine Process, the Ammonia Soda Process, the Mond Sulphur Process, the Young Paraffin Oil Distillation, Henderson's Copper Process, Bessemer's Steel Process, Tar Distilling, Tar Colour Manufacture, and many others.

Many processes can be carried on very successfully by chemists in the laboratory, but how few are able to make chemical processes go on the large scale, and simply because they have a lack of physical and mechanical knowledge combined with their chemistry.

I am not one of those who believe that our advisers in these great matters of chemical industry should be mere thorough engineers with a smattering of chemistry, but rather that chemistry should form the central pillar and be propped up by all the collateral sciences. A little chemistry is a dangerous thing, and if we could start a new

Society with men who have a thoroughly sound chemical basis, and who have also founded their experience upon the fundamental principles of mechanics, the elements of mechanism and machine construction, I am sure we should be doing much to drive out rule-of-thumb, and with it most of the quaint, expensive, and often futile, machinery which unfortunately exists in too many works of the present day.

Theoretical chemistry is well represented by the Chemical Society, trade and professional chemistry by the Institute of Chemistry and by the Society of Arts also, but a Society of Chemical Engineers would occupy entirely new ground; its field of work would be quite distinct from any of the others.

I could give many instances in which processes have failed from want of knowledge, either of physics or of mechanical principles, on the part of those who had essayed to carry them out; but, on the other hand, the chemical knowledge required was so thorough, that I am sure no mechanical or civil engineer with a mere smattering of chemistry, would have been one whit more successful.

In conclusion, I shall be glad either of the opinions or the support of technical chemists who may be interested in this matter, to lay before my committee, and I shall also be happy to give any information respecting the projected Society which I possess.—I am, &c.,

GEORGE E. DAVIS,

Hon. Sec., *pro tem.*

Dagmar Villa, Heaton Chapel,
Stockport, June 1, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 17, April 26, 1880.

The Law of Distribution according to the Altitude of the Atmospheric Substance which absorbs the Ultra-violet Solar Rays.—A. Cornu.—The mass of the absorbent matter at every altitude is proportional to the barometric pressure, and is consequently in a constant relation to the mass of the atmosphere. If the absorption of the ultra-violet rays were due exclusively to the action of the watery vapour, distributed in height according to the experimental law, the increase of the visibility of the solar spectrum would be unity (one-millionth of a millimetre) on the scale of wave-lengths for an increase in height of 286.9 metres. As direct observation shows a quantity three times larger, that is to say, a unity for 868.2 metres watery vapour, cannot be admitted as the sole cause of the absorption of these radiations. It is, on the contrary, very probable that the other elements of the atmosphere, whose proportion is regarded as constant at all heights, have the power of absorbing the highly refrangible radiations. Atmospheric dust, to which several physicists ascribe the greater part of the absorption of the ultra-violet rays, plays only a secondary part. It is very curious that watery vapour, which takes a predominant part in absorbing the less refrangible rays of the spectrum, should not behave in a corresponding manner with the more refrangible rays.

Explosive Properties of Mercury Fulminate.—MM. Berthelot and Vieille.—The authors have determined experimentally the nature of the products of the explosion of the fulminate, the quantity of heat liberated, whence its formation-heat and the measure of its explosive efficacy has been deduced, its density, and the pressure exerted during explosion in closed vessels.

On a Meteorite which fell May 10, 1879, at Estherville, Iowa.—J. Lawrence Smith.—The metallic nodules occurring in the meteorite are composed of:—

Iron	92.00
Nickel	7.10
Cobalt	0.69
Copper	trace
Phosphorus	0.112

The insoluble portion of the earthy matter contains:—

Silica	54.12
Ferrous oxide	21.05
Magnesia	24.50
Soda, with traces of potassa and lithia	0.09
Chromium oxide	traces
Alumina	0.03

99.79

Mineralogically the meteorite is composed of bronzite, olivine, a silicate not yet determined, nickeliferous iron, chromite, and troilite.

Dependence of Two Electro-magnetic Gyroscopes submitted to the same Induction-circuit.—W. de Fonvielle.—The author concludes from his experiments that the symmetry of the attractions exerted is broken by the work of analogous reactions developed by the movement between the movable magnetisable molecules and the fixed magnetic centres, as well as between the former and the induction currents. He suggests that the theory based on the intervention of the coercitive force is insufficient for the explanation of phenomena offering such complication and capable of acting at a distance by a kind of sympathy.

Theory of Induction-currents.—M. Mascart.—A mathematical paper, not susceptible of advantageous abstraction.

An Experimental Method for Determining the Lines of Level in the Stationary Efflux of Electricity along Conducting Surfaces.—A. Guebhard.—This paper requires the four accompanying diagrams.

Absolute Measurement of Peltier's Phenomenon on the Contact of a Metal with its Solution.—E. Bouty.—This paper does not admit of useful abstraction.

Measurement of the Difference of the Potential of Two Metals in Contact.—H. Pellat.—The author has investigated the changes which the difference of potential undergoes in regard to the physical state of the metallic surfaces. He has also studied the influence of a change of temperature, and of the gases surrounding the plates.

The Theory of Circular Double Refraction.—M. Gouy.—The phenomenon discovered by Fresnel is only a necessary consequence of rotatory double polarisation. The remarkable splitting-up, which, according to Fresnel's ideas, is a double refraction, becomes, if viewed with reference to the facts themselves, a phenomenon of diffraction of a peculiar kind.

Influence of Temperature on the Compressibility of Gases under High Pressures.—E. H. Amagat.—The following laws are proposed:—(1.) When a gas is more compressible than Mariotte's law indicates, its compressibility increases with a rising temperature. (2.) When a gas is less compressible than the law indicates its compressibility grows with the temperature. (3.) This increase, very rapid in the neighbourhood of the least ordinate where the gas follows accidentally the law of Mariotte, quickly slackens, so that under constant pressures the effect of temperature becomes less and less considerable.

Researches on the Passivity of Iron (Second memoir).—L. Varenne.—The determination of passivity is always preceded by chemical action of variable duration. The passivity of a rod of iron may be occasioned by the immersion of a part only in concentrated nitric acid.

Passivity may be produced by the prolonged exposure of iron to nitric oxide under strong pressure.

Proportion of Iron in the Mineral Waters of Rouen and of Forges-les-Eaux.—A. Houzeau.—The author's method is to mix a known volume of the water, immediately after being drawn at the spring, with pure sulphuric acid, evaporate to dryness, and expel all the excess of acid, treat the saline or carbonaceous residue with pure hydrochloric acid, bring the iron to the ferrous condition with pure metallic zinc, and titrate with permanganate.

Isomers of Phloro-glucine.—A. Gautier.—The author asks if the glucoses are apt to furnish phloroglucine by simple dehydration, and if distinct phloroglucines correspond to the various glucoses which enter into the composition of the vegetable glucosides. He has specially examined the aromatic sugar of the colouring-matter of Carignane wine, $C_{24}H_{20}O_{10}$, the aromatic sugar derived from quercitine, and that obtained by the oxidation of phenol.

The Products Contained in the Coke of Petroleum.—L. Prunier and E. Varenne.—The authors admit the presence in the coke of petroleum of a complex totality derived from a pyrogenous equilibrium (see *Comptes Rendus*, 88, p. 386).

A Singular Explosion Produced during the Heating of Wine, and a New Method for the Determination of Alcohol.—V. Wartha.—A quantity of Tokay wine, about 6 hectolitres, containing 15 per cent of alcohol by volume, was being submitted to Pasteur's process. When almost all the wine had passed through the apparatus, a violent explosion took place, shattering the staves of the cask to fragments. It is supposed to have been caused by the ignition of a mixture of alcoholic vapour and atmospheric air contained in the empty part of the cask. He considers that if wine is heated to 70°, the vapours may reach the dangerous limit of 43° (the "flashing-point"), and that when Pasteurising strong wines they should be refrigerated with ice as they flow from the apparatus. The method for the determination of alcohol is not described.

Synthetic Reproduction of Aluminous Silicates and Alkaline Alumino-silicates.—S. Meunier.—The author describes his methods and results at considerable length.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.

No. 75, March, 1880.

Report Presented by M. Aimé Girard on M. Olivier Lecq's Apparatus for the Analysis of Beet-roots.—The description of the apparatus cannot be usefully reproduced without the ten accompanying figures.

The French Patent Law.—Ch. de Laboulaye.—The writer argues in favour of a preliminary examination of applications for patents, such as is practised in America and in Germany.

Chemiker Zeitung.

No. 9, February 26, 1880.

Determination of Thein.—C. Patrouillard.—The author digests 15 grms. tea with boiling water to complete exhaustion. The aqueous extracts, united and filtered, are evaporated to dryness, with the addition, towards the last, of 2 grms. calcined magnesia, and 5 grms. of powdered glass. The residue is finely pulverised and digested with ether in a flask for twenty-four hours with occasional agitation. Fresh ether is continually substituted as long as anything is taken up. The united ethereal extracts are distilled, the crystals of thein redissolved in a little chloroform, and the solution evaporated in a tared capsule. The proportion of thein in tea which has not been treated with water ranges from 1 to 3½ per cent.—*Repertoire Pharm.*

Qualitative Separation of Copper, Bismuth, and Cadmium.—M. Hes.—To the slightly acid solution of the three metals, potassium ferricyanide is added in slight excess, when all three are thrown down as ferricyanides. Potassium cyanide is added in excess and the mixture is gently heated. The copper and cadmium compounds dissolve, whilst the bismuth remains as a white flocculent hydroxide. The filtrate is divided into two portions; the one is tested for copper with hydrochloric acid, which occasions a brown-red precipitate of copper ferrocyanide, whilst to the other is added a little ammonia and ammonium sulphide. On the application of a gentle heat, yellow cadmium sulphide is precipitated.—*Pharm. Zeitung*

New Process for Bleaching Vegetable Fibres.—O. Bayle and R. Pontiggia add to a basic bath of alkali and chloride of lime, margaric, stearic, or oleic acids, or the mucilage of flax, or of Panama bark. This addition is said to limit the destructive action of the chloride, and promote the solution of the gummy and resinous matters which adhere to the raw fibre.—*Färb. Zeitung*.

Petroleum and Coal Benzines.—The crude petroleum, after treatment with sulphuric acid, is fractionated into naphtha, refined petroleum, and petroleum grounds. The naphtha, which forms from 5 to 10 per cent of the products, is limpid and mobile, boiling at from 50° to 150°, and having the sp. gr. 0.65 to 0.72. It is again fractionated. The most volatile portion is petroleum ether, boiling at 40° to 70°, and having the sp. gr. 0.64 to 0.65. It serves for dissolving fatty matters. The second portion, which distils from 65° to 90°, is gasoline, used for the production of gasoline gas. Petroleum benzine, the third portion, passes over between 90° and 110°, and serves as a solvent. The residue in the still is known as turpentine-substitute.

Turkey-red Oil, Soluble in Water.—To 3 kilos. castor oil are added, with constant stirring and in a very thin gradual stream, 650 grms. sulphuric acid at 66° B. A rise of temperature must be carefully avoided. The whole is let stand for twelve hours, diluted with 3½ kilos. water, and soda-ash added in small quantities (about 650 grms.) till the mixture no longer reddens litmus. To dissolve the white emulsion thus obtained ammonia is added till a portion dissolves in distilled water. It is then allowed to settle, and the clear liquid drawn off for use. Sodium sulphate is found in crystals at the bottom.—*Schweiz. Gew. Bl.*

New Process for the Manufacture of Sulphuric Acid.—A. Houzé.—The author mixes sulphurous acid with steam and superheated air. The temperature must not reach that at which sulphuric acid is decomposed. The condensation is carried on in stoneware vessels.

No. 10, 1880.

This issue contains nothing suitable for abstraction.

No. 11, 1880.

This number contains a very long original article by O. Cordel on the manufacture of animal charcoal.

Chemisches Central-blatt.
No. 11, March 17, 1880.

Agreement of the Oxygen-stere, 5.4, with the Stere of Chlorine, Bromine, and Iodine.—H. Schröder.—After the author had explained by a number of examples the idea of the "volume proportion" or stere, and the fact that in the solid as well as in the gaseous state, the elements combine only in whole volumes, *i.e.*, in volumes which bear to each other the proportions of simple integral numbers, he pointed out that he had assigned the stere 5.4 both to oxygen and to the halogens. He considers that the volumes of these bodies in their solid state, as well as the behaviour of their vapours at high temperatures, seem to indicate that chlorine, bromine, and iodine are probably oxygenous bodies.—*Tageblatt des Natur. Vereins Baden-Baden*, 1879, 191.

Behaviour of Sulphuretted Hydrogen with the Salts of Heavy Metals.—M. Dellfs.

Annales de la Société des Sciences Industrielles de Lyon.
No. 4, 1879.

This number contains no chemical matter.

NOTES AND QUERIES.

Extracts of Dyewoods.—Can any reader refer me to a full description of the methods used in this country and on the Continent for preparing extracts of dyewoods, with details of the apparatus, and an account of the composition of the extracts produced? Or where can I find any part of this information.—A CONSTANT READER.

MEETINGS FOR THE WEEK.

MONDAY, 7th.—Royal Institution, 5. General Monthly Meeting.
TUESDAY, 8th.—Photographic, 8.
— Royal Medical and Chirurgical, 8.30.
WEDNESDAY, 9th.—Geological, 8.
— Microscopical, 8.
THURSDAY, 10th.—Royal, 4.30.
— Royal Society Club, 6.30.
FRIDAY, 11th.—Quekett, 8.
— Astronomical, 8.
SATURDAY, 12th.—Physical, 3.

TO CORRESPONDENTS.

Alex. M'Uvor.—Consult any standard work on quantitative analysis.
H. Rayner.—Information asked for through our columns is intended to be made public for the benefit of all thereaders of the **CHEMICAL NEWS**.

THE JOURNAL OF SCIENCE

for June (Price 1s. 6d.), includes—

Insanity and its Difficulties.
The History of Antozone and Peroxide of Hydrogen. By Albert R. Leeds, Ph.D.
The Origin of Falling Motion. By Charles Morris.
The Aurora.
Flight Aspirations. By Fred. W. Brearey, Hon. Secretary to the A.S.G.B.
Analyses of Books. Correspondence. Notes.
London: 3, Horse-Shoe Court, Ludgate Hill.

INSTITUTE OF CHEMISTRY.

PRIZES.—DR. FRANKLAND has offered a Prize of £50 for the best Original Investigations involving GAS ANALYSIS; and Dr. C. MEYMOTT TIDY has also offered a Prize of £25 for the best Original Investigation on SPECIAL REACTIONS OF THE ALKALOIDS, AND THEIR SEPARATION FROM ORGANIC MIXTURES. These Prizes will be open to Associates, and to all persons (except Fellows of the Institute) who shall, before the 31st December next, have qualified for the Associateship in all respects short of passing the prescribed practical Examination, and successful competition for these Prizes will be accepted in lieu of such practical Examination. Further information may be obtained on application to the Secretary, Mr. C. E. GROVES, Somerset House Terrace, W.C.

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Leicester Square (President, H.R.H. the Duke of Cambridge, K.G.), and THE LONDON SCHOOL OF DENTAL SURGERY. The Posts of Lecturer on Metallurgy and on Mechanical Dentistry at the above School are vacant. Candidates for the appointments are requested to send in their applications to the Honorary Secretary, at the Hospital, on or before July 1st next. The Candidates for the post of Lecturer on Mechanical Dentistry must hold the Dental Diploma of the Royal College of Surgeons of England.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1072.

OBSERVATIONS UPON DR. TIDY'S PAPER ON RIVER WATER.

By R. WARINGTON.

As the time available for the discussion of Dr. Tidy's recent paper at the Chemical Society proved quite insufficient for a full consideration of the many important questions raised in that communication, I may perhaps be allowed to make a few remarks on some aspects of the subject in the pages of the CHEMICAL NEWS.

Both in Dr. Tidy's paper, and in the discussion which followed, it was remarkable that the destruction of sewage in river water was in every case referred to the oxidising influence of the air. Dr. Tidy, who contended that sewage was destroyed in the course of a river's flow, spoke of that sewage as being destroyed by contact with atmospheric oxygen; while Dr. Frankland, who held that this destruction of sewage took place to a very limited extent, supported his argument by experiments in which diluted urine or sewage was found to be tolerably permanent when agitated with air. Now, if we are to take an exact view of this important question, we must surely bear in mind that modern investigations plainly teach us that in nature organic matter is resolved into inorganic matter not by mere contact with atmospheric oxygen, but by the agency of organic life. Such organic fluids as urine and milk have been repeatedly shown to be capable of preservation through long periods, in free contact with air, if only all germs of organic life be excluded. We cannot for a moment suppose that Drs. Tidy and Frankland were not well acquainted with these facts, but the idea of the primary importance of organic life for the destruction of sewage has certainly been strangely absent in their discussion of the question.

The decay of organic matter is the joint work of a number of independent organisms of different functions, the action of one class following that of another, and carrying the process through a further stage. We are too imperfectly acquainted with the organisms effecting these changes, and their particular functions have been too little studied, for any accurate sketch of these processes to be given. In the first rank we must probably place the fungi, whose main function is apparently the rapid oxidation of carbon. Following these we have the innumerable army of bacteria, embracing many families of very similar physical structure, but endowed with very different chemical powers. One class of these bacteria attacks nitrogenous organic matter, and liberates the nitrogen in the form of ammonia; while another class of bacteria determines the conversion of carbonaceous organic matter and ammonia into simple inorganic bodies—carbonic and nitric acids. Lastly, we have the chlorophyll-bearing plants, which consume the carbonic acid, ammonia, and nitric acid produced by lower organisms, and are also capable of assimilating urea and other amide bodies, and a large number of inorganic ash constituents.

For organic matter to be destroyed it is necessary that these organisms should exercise their functions in a certain order. The nitrification bacteria would produce no nitric acid in a mass of fresh nitrogenous matter; the fungi and the putrefaction bacteria must first run their course and prepare the way for the nitrifying organism. In the same way green vegetation is impossible till every stage of putrefactive change is completed.

The bearing of these ideas on the question of the destruction of sewage in river water is obvious. That sewage will finally be destroyed in a normal river of adequate temperature and sufficient length surely admits of no doubt. It is

only when the natural agents of oxidation are excluded by addition to the water of chemical refuse fatal to organic life, or when vegetation is prevented by artificial means, that the conditions necessary for purification are absent. The practical question whether in any particular stream the sewage has been efficiently destroyed can, however, only be determined by an actual examination of the water; and we must admit, apart from any analysis, that the condition of our principal rivers is certainly unfavourable for complete purification, seeing the vast quantity of sewage introduced within comparatively short distances of their outfall, and the artificial character of the streams maintained for the purposes of navigation.

The conditions of laboratory experiments cannot easily be made comparable with those of a natural river, if organic life rather than mere exposure to air is the true agent in the destruction of sewage. In any experiments it would seem advisable to employ natural river water and not an artificial mixture, as the needed organisms would more probably occur in the natural fluid. In such a mixture as that of fresh urine with deep well-water it is obvious that organisms have been as far as possible excluded. When working with any recent dilution of sewage, time must always be given for the multiplication of the organisms before any appreciable effect can be expected from their action. A solution seeded with a small quantity of the nitrifying bacteria will remain for many days without any apparent change; but after a while nitrification commences, and proceeds from day to day at an accelerating rate.

If the oxidation of sewage is effected by living organisms, temperature and light become important factors in the process. It may truly be said that the activity of bacteria is a function of temperature. It is a very familiar fact that at a low temperature no putrefactive change will take place. As the temperature rises the activity of the putrefaction bacteria rapidly increases; it reaches a maximum at a tropical summer heat, and then gradually diminishes to nothing as the temperature approaches 60°C. In the case of the nitrification bacteria, Schloësing and Müntz have shown that nitrification is ten times more rapid at 37° than at 14°. An example of the little weight hitherto attached to the action of these low organisms was afforded by Dr. Frankland's remark at the Chemical Society that it was "a curious thing" that the oxidation of organic matter in the Irwell river was shown by his experiments to be far more rapid at 17° than at 12°; if the oxidation in question was the work of bacteria the increased rapidity of action with a rise in temperature would be only what we should expect. The influence of light must also not be forgotten. Both the putrefaction and nitrification bacteria flourish best in darkness; a turbid polluted stream is thus apparently a better field for the activity of such low organisms than any small bulk of fluid exposed to light in a laboratory.

I am unable to understand on what grounds Dr. Tidy has placed fish among the purifying agents of river water. As they cannot exist in the most polluted part of the stream, it is difficult to imagine that they consume any of the solid organic matter brought down by the sewage, as this must surely have been deposited, or become far advanced in decomposition, before the cleaner portion of the river is reached in which fish exist. The respiration of the fish must also necessarily diminish the amount of oxygen in the water. Lower forms of animal life, inhabiting the mud of the river, are probably efficient scavengers of solid matter, and become themselves in turn the food of fish.

Turning, now, for a moment to the question of the removal of peaty matter from water, it may perhaps be of service to remind those working on the subject that humic acid is stated by Detmer to be a colloid body, and that it forms insoluble compounds with lime and ferric oxide. Apocrenic acid is according to the same investigator a diffusible substance, and capable of being assimilated by plants.

A large part of the interest belonging to Dr. Tidy's

paper is connected with those alterations in the composition of river water, which he shows to occur regularly in the course of the year. In the case of Thames and Lea water a maximum contents of dissolved solid matter is found in February; this amount regularly diminishes till a minimum contents is reached in August, or, in the case of the Lea, in September. As a rule every constituent of the solid matter participates in this rise and fall, with the single exception of the chlorine, which is on the whole rather greater in the summer months when the other constituents are near their minimum. No cause is suggested in Dr. Tidy's paper for this regular alteration in composition, and the question was for want of time left undiscussed at the Chemical Society.

It is difficult to imagine that these regular changes in composition are determined by differing amounts of flood water. The months of March and April are certainly not months in which floods are common, yet they lie quite near to the period of maximum impurity; nor is it easy to imagine that floods commence in the Thames as early as September, though in this month the impurities begin distinctly to rise. The times of maximum and minimum impurity are, on the other hand, in close accordance with the alteration of the temperature of the water, the maximum impurity following hard on the minimum temperature of the river, and the minimum impurity succeeding closely the maximum temperature. This fact seems to suggest that the alteration in composition is chiefly brought about by the organic life in the water. When the water is at its coldest, all forms of life are most inactive, and the water at this period possesses its greatest impurity. As spring advances life awakes, and the water becomes purer month by month, reaching a maximum degree of purity in August or September, a natural result of the accumulated effect of active life during the summer months. After this point, life rapidly diminishes in activity, and the water as rapidly returns to its winter composition.

The effects of life on the composition of water are two-fold, direct and indirect. In pure waters the indirect influence of green submerged vegetation may be very considerable. My father made monthly determinations for some years of the hardness of the water of a fresh-water aquarium standing in the window of his sitting-room. The aquarium contained *Vallisneria* and fish; the water was never changed. The result of seventeen months' observations will be found in the *Proceedings of the Royal Society*, 1868, 189. The hardness in the water in this aquarium was 12.5° in July, 1861, from which point it gradually rose to 25° in the following February, then slowly sank to 12° in August, after which it again commenced to rise. The maximum hardness was thus in February, and the minimum in July or August. The cause of this change was clearly due to the consumption of the carbonic acid dissolved in the water by the green vegetation during the months of greatest light and heat. This removal of carbonic acid from the water determined a precipitation of carbonate of calcium on the glass sides of the aquarium, and the water in consequence diminished in hardness. As the supply of light and heat diminished the carbonic acid furnished by the respiration of the fish gradually overbalanced the amount decomposed by the plant, and the carbonate of calcium was re-dissolved.

It is evident that an effect of the kind just stated affects only the temporary hardness of a water. The analyses of Lea water given by Dr. Tidy show a marked maximum of temporary hardness in February, and a considerable diminution during the summer months; but the rise and fall are not regular. With the Thames water there is little indication of such a change as that just described. A reason, however, is at once afforded by the fact recorded by Dr. Tidy on page 274, that Thames water contains more carbonic acid dissolved in it in summer than in winter. Under these circumstances we can therefore expect no diminution in the temporary hardness.

The direct influence of vegetation on the water in which

it exists is to remove from it the materials necessary for plant growth, these constituents afterwards passing in many cases into the bodies of animals which feed on the vegetation. It is to this direct action of vegetation that the changes in the inorganic constituents of river water are most probably due. The nitrates, sulphates, and all the ash constituents necessary for plant tissue diminish as the season advances, being taken up by the actively growing vegetation, while the chlorides—which are well known to agricultural chemists as unessential to plant life—remain untouched, or indeed somewhat increase, from the concentration of the water during the heat of summer. As autumn sets in, not only does the taking up of matter by the plant cease, but the plants on decay restore to the river more or less of the constituents previously removed. All the changes from maximum to minimum are more marked in the Lea than in the Thames, the former river having probably a greater proportion of vegetation in relation to its volume of water.

It remains to glance, in conclusion, at the diminution of organic carbon and nitrogen in river water during the summer months. This seems probably due to the action of the bacteria, which becomes intensified as the temperature of the water rises. Dr. Tidy's gas analyses, already referred to, show that in Thames water the dissolved oxygen is greatly diminished during the summer months while the carbonic acid increased, a result, doubtless, of the more rapid oxidation then in progress.

ON THE INFLUENCE OF SUPERFUSION ON THE MOLECULAR ARRANGEMENT OF CUPELLED GOLD.

By Dr. A. D. VAN RIEMSDIJK,
Assayer-General of the Netherlands Mint.

As an appendix to my research on "Flashing"* it remains for me to mention the remarkable influence of superfusion on the physical state, or rather the molecular arrangement of cupelled gold. The abrupt and sudden change of state marked by the flashing, which puts an end to the abnormal condition of the superfused metallic mass, produces without exception a button of solidified gold, possessing in the highest degree the softness, the ductility and the malleability which characterise the chemically pure metal. A button of cupelled gold which has flashed, if flattened with the hammer upon a steel anvil, forms a disk with a perfectly regular outline, without the least crack. On passing this disk several times through the laminating rollers we obtain a slender fillet which preserves its entire malleability: the circumference of this fillet is perfectly regular.

But the case is very different if the superfusion is prevented, either by contact with a fragment of solid gold before the liquid button has cooled down to the normal point of solidification, or by the previous admixture of traces of iridium, osmium-iridium, &c. Cupelled gold which solidifies without flashing, *i.e.*, in the ordinary manner, gradually losing the latent heat of fusion, presents under the hammer or the rollers a metal more or less brittle. The disk and the band offer irregular outlines, cracked in several places.

The cause of this non-malleability should be attributed to a slight proportion of lead (about 0.7 mill.), which cupellation, even at a strong heat, has not been able to separate from the gold which has been passed through the assay furnace. With bismuth the same results are obtained. Pure gold, cupelled with bismuth in excess, retains always traces of this metal, which considerably diminish the malleability of the button if its superfusion has been hindered. On the other hand, cupelled bismuthiferous gold which has "flashed" is perfectly soft and malleable.

* CHEMICAL NEWS, vol. xli., p. 126

The classic researches of Charles Hatchett,* executed at London at the beginning of this century, have shown us the injurious influence of several metals added to gold, whether pure or alloyed, even in very limited quantities. It appears from these researches that a proportion of lead or bismuth of 1 part in 1920 = 0.521 mill., is enough to destroy the malleability and the ductility of gold. According to our own researches this is perfectly correct, in the case when gold, mixed in the liquid state with a little Pb or Bi, sets according to the ordinary laws of solidification. But if the metal has undergone superfusion, "flashing" before becoming solid, this anomalous change of condition completely destroys the injurious influence of the lead or bismuth which remains in the cupelled gold.

It is proper here to remark that in gold, pure or alloyed (with copper or cupriferous silver), which has not been submitted to cupellation with Pb or Bi, the manner of solidification has no influence on the physical condition of the metal in the solid state, the great malleability of which is retained under all conditions. What is true as regards pure gold or cupriferous gold cupelled with lead, holds equally good for its alloy with cupriferous silver,—e.g., for an ordinary assay at the standard of 0.900, composed of 250 Au, 25 Cu, and 625 Ag. If the cupelled alloy, when taken from the furnace in the liquid state, has "flashed" before setting, the button of the plumbiferous alloy behaves perfectly under the hammer and the roller; it is malleable in all respects. If, on the contrary, superfusion has been prevented, and the button has set gradually without "flashing," we obtain a brittle metal, which yields a band irregular in outline and often cracked, the prominences of which are easily detached from the bulk of the band, and may occasion a mechanical loss in the cornet. It is evident that the phenomena of superfusion and of flashing observed in assay-buttons augment remarkably our confidence in the standard found, since they produce intact bands and cornets free from all cracks; on the other hand, all the causes which annul superfusion, and consequently "flashing," detract from the exactness of the method of assay generally practised.

There is an effectual method for restoring complete malleability to gold cupelled with lead or bismuth, and which has set without "flashing." It is merely necessary to re-melt it in a new cupel in the assay furnace, and to add to the liquid metal chloride of copper, $\text{CuCl}_2 + 2\text{H}_2\text{O}$, about 50 m.grms. wrapped in a bit of paper, to $\frac{1}{2}$ grm. of gold. During the very brisk and short reaction between the copper chloride and the gold contaminated with Pb or Bi,—which is characterised by a brilliant blue flame,—these latter metals are completely eliminated in the form of volatile chlorides. The purified gold, taken from the furnace in a liquid state, sets without flashing, and forms a button of perfect malleability.

The copper chloride process may serve to show that a slight proportion of iridium does not appreciably interfere with the malleability of gold. If we submit to cupellation $\frac{1}{2}$ grm. of pure gold mixed with 5 m.grms. of finely-divided iridium, we obtain a button which sets without presenting the phenomena of superfusion and of flashing, and constitutes a metal brittle under the hammer and the rollers. But this defect in malleability may be completely removed by contact with copper chloride after having melted the cupelled gold a second time in the assay furnace. It is easy to prove that in this operation the iridium resists, if not entirely, at least mainly, the action of the copper chloride; for iridiferous gold which has been submitted to fusion with this chloride, if returned to the cupel with excess of lead, sets immediately after being withdrawn from the muffle in the liquid state. Superfusion and consequently "flashing" are prevented by the traces of iridium which the copper chloride has not been able to separate from the button.

The same facts are observed on the addition of small

traces of iridium or of osmium-iridium to pure gold in a state of fusion, without calling in the intervention of lead to effect the mixture. Iridiferous gold, prepared by a direct method, preserves its malleability.

But it must be admitted with reason that the presence of iridium, osmium-iridium, &c.,—that is to say, of metals of the platinum group which are not capable of forming a homogeneous alloy with the gold in which they are mixed,—destroys in part its continuity, and therefore its ductility, properties which characterise gold when pure or alloyed with copper or with cupriferous silver.

A METHOD FOR THE PROXIMATE ANALYSIS OF PLANTS.*

By HENRY B. PARSONS.

(Concluded from page 258.)

VI. Estimation of Eighty per cent Alcohol Extract.

That part of the plant not dissolved by benzole should be dried at 100°C ., and then completely exhausted by eighty per cent alcohol (specific gravity 0.8483 at 15.6°C .). This requires from twelve to fourteen hours' continuous treatment with the solvent. Remove, dry, and weigh any crystals or powder that may separate upon concentrating and cooling the alcoholic percolate. Make the clear liquid to a definite volume by adding more eighty per cent alcohol. In an aliquot part of this liquid determine *total organic matter* and *ash*: in another equal portion determine *total organic matter* and *ash soluble in water*, and, by difference, *total organic matter insoluble in water*.

The remaining clear alcoholic liquid should be evaporated carefully to dryness, pulverised, and treated with several considerable portions of absolute alcohol (specific gravity 0.7938 at 15.6°C .).

A. SOLUBLE IN ABSOLUTE ALCOHOL.

a. Soluble in water.

a 1. Precipitated by subacetate of lead.

Tannin and most organic acids; some extractives; some inorganic acids of the ash. Weigh in Gooch's filter, ignite cautiously, and again weigh; loss equals organic matter precipitated.

a 2. Not precipitated by subacetate of lead.

Alkaloids, glucosides, some extractives and colours. Determine by difference between *a* and *a 1*.

b. Insoluble in water.

b 1. Soluble in dilute hydrochloric acid.

Alkaloids, glucosides, (rarely) some extractives. Determine by difference between *b* and *b 2*.

b 2. Insoluble in dilute hydrochloric acid.

b 3. Soluble in dilute ammoniac hydrate.

Most acid resins, some colours. Determine by difference between *b 2* and *b 4*.

b 4. Insoluble in dilute ammoniac hydrate.

Neutral resins, some colours, albuminoids (in some seeds). Re-dissolve in alcohol, evaporate and weigh.

B. INSOLUBLE IN ABSOLUTE ALCOHOL.

c. Soluble in water.

c 1. Precipitated by subacetate of lead.

Some colours, extractives, albuminoids (rarely), organic acids and inorganic acids of the ash. Weigh in Gooch's filter, ignite cautiously, and again weigh; loss equals organic matter precipitated.

c 2. Not precipitated by subacetate of lead.

Alkaloids, glucosides, sucrose, glucose, some extractives. Determine by difference between *c*

* Phil. Trans. of the Royal Society of London for the year 1803 Part 1, p. 43.

* From the American Chemical Journal, vol. i., No. 6.

and c r. Remove Pb by H_2S , H_2SO_4 , Na_2CO_3 , or other means, and titrate for sucrose and glucose.

d. Insoluble in water.

d 1. Soluble in dilute hydrochloric acid.

Some alkaloids and glucosides. Determine by difference between *d* and *d 2*.

d 2. Insoluble in dilute hydrochloric acid.

Few resins, some extractives, and colour substances. Dissolve in alcohol, evaporate and weigh in a tared dish.

In some cases it may be preferable to use the following method for analysis of the eighty per cent alcohol extract; it is more desirable when the plant examined contains a considerable amount of sugars, tannic acid, &c.,

Alcohol Extract, dilute to 200 c.c. with eighty per cent alcohol.

1. In 20 c.c. determine *total organic matter* and *ash*.

2. In 20 c.c. determine *total organic matter* and *ash* that are *soluble in water*, and, by difference, *total organic matter insoluble in water*.

3. Evaporate the remaining 160 c.c. to dryness, treat with water, filter, and make the filtrate measure 160 c.c. Reserve the insoluble matter on the filter for examination. (10).

4. In 20 c.c. of the aqueous solution determine tannin gravimetrically by A. Carpeni's method;* precipitate by ammoniacal acetate of zinc, use a Gooch's filter, wash the precipitate with very weak ammonia, dry at 120°C ., weigh, ignite cautiously, again weigh. The loss by ignition equals tannic acid, in absence of certain interfering substances.

5. Precipitate 20 c.c. by normal acetate of lead, and determine, as before described, the amount of organic matter after drying at $100-120^\circ\text{C}$. This precipitate will contain, if the substances are present in the plant, *tannic, gallic*, and most other *organic acids*, some *colours*, rarely *albuminous substances*, some *extractives*, and most *inorganic acids* of the ash. Determine, by difference, the amount not precipitated by this treatment.

6. In 20 c.c. determine in like manner the amount precipitated by basic acetate ("subacetate") of lead. This reagent precipitates a greater number of acids, colours, and extractives than are precipitated by the normal acetate; hence it is frequently possible to estimate such substances by subtracting the amount precipitated by one reagent from the amount precipitated by the other. To the filtrate add a slight excess of dilute hydrochloric acid, boil gently for half an hour, and determine in the liquid *total glucose* by use of Fehling's solution.

7. Precipitate 20 c.c. by subacetate exactly as in 6, and use the precipitate as a duplicate to check the amount there estimated. To the filtrate add a very slight excess of solution of carbonate of sodium, filter from the carbonate of lead, wash well with water containing a little alcohol, and in the filtrate estimate *actual glucose*. If the glucose thus found is appreciably less than that in 6, subtract it from that amount; this glucose may be due to the presence in the plant of *sucrose* or some *glucoside*. If due to *sucrose*, the amount of the latter may be found by multiplying this residual glucose by 0.95; if to a *glucoside*, a fit subject for an extended investigation is presented. The properties, formula, and decomposition products of the newly-found glucoside should be carefully studied.

8. Precipitate 20 c.c. with subacetate of lead, as in 6 and 7, employing the precipitate as material from which to separate organic acids, after removal of lead by sulphuretted hydrogen. Acidulate the filtrate with sulphuric acid, add an equal volume of alcohol, allow to stand two hours, filter, wash the precipitate with fifty per cent alcohol, and evaporate the filtrate until all alcohol has been dissipated. Test the acid solution for *alkaloids, glucosides, sugars, extractives*.

9. Reserve the remaining 40 c.c. for duplicating any unsatisfactory determinations.

10. The residue mentioned in 3 as insoluble in water may contain *resins, albuminoids* (especially from seeds), *colours, alkaloids, glucosides*. Dilute acids remove *alkaloids* and some *glucosides*, dilute ammoniac hydrate will remove some *resins, colours*, and *glucosides*. Any still insoluble residue probably contains *albuminous* or *resinous* substances.

VII. Estimation of Cold Water Extract.

That part of the plant remaining insoluble after treatment with alcohol should be dried at 110°C . and completely extracted by cold water. When the plant contains considerable mucilaginous matter, this is best removed by placing the substance in a flask or graduated cylinder, and then adding a measured volume of cold water. Allow to macerate, with frequent agitation, for from six to twelve hours; then filter through fine washed linen, and evaporate an aliquot portion of the solution. In this residue determine *total organic matter* and *ash*. This residue usually contains little but *gum*; in analysis of fruits and fleshy roots *pectin bodies, salts of organic acids*, rarely a substance resembling *dextrin*, and small amounts of *albuminous substances* and *colouring matter*. Usually the separation of these substances is very difficult. The unevaporated liquid should be used for such qualitative reactions as are necessary to show the nature of the substances extracted. The insoluble residue should be well washed with water, transferred to a crucible, and completely dried at 110°C . This residue should be then weighed.

VIII. Estimation of Acid Extracts.

The dried residue insoluble in cold water should be transferred to a beaker containing 500 c.c. of water and 5 c.c. of concentrated sulphuric acid (specific gravity 1.84). Boil for six hours, on a gauze support, adding water to keep the volume of liquid unchanged; if the substance be very starchy, a longer boiling may be necessary. This treatment will convert *starch* and its *amorphous isomers* to dextro-glucose, and will occasionally remove some *salt of an organic acid* with usually traces of *albuminous* and *indeterminate* substances.

The *total amount extracted* may be found by washing, drying at 110°C ., and weighing the yet insoluble residue, and subtracting the weight from the one taken after extracting with cold water. The amount of *starch and isomers* may be found by determining in a given volume of the acid filtrate the amount of *glucose*, using Fehling's solution; the glucose thus found multiplied by 0.9 equals *starch and isomers*. The *total extract* minus *starch and isomers* equals *acid extract not starch*. This includes a small amount of *ash*, which may be approximately determined by evaporating and igniting a known volume of the solution.

Where it is wished to separate the extracted matter from the sulphuric acid, boil the liquid with an excess of powdered barium carbonate until no acid reaction remains. Filter, and evaporate to dryness. The residue consists chiefly of hydrated dextro-glucose ($\text{C}_6\text{H}_{12}\text{O}_6 \cdot \text{H}_2\text{O}$), with some ash.

IX. Estimation of Alkali Extract.

Wash well and dry at 110°C , the residue from treatment with acid, and record its weight. Boil this residue, for two hours, with 500 c.c. of a solution containing 20 grms. of sodic hydrate to the litre. Filter through fine washed linen, and wash the residue thoroughly with hot water, alcohol, and ether. Transfer it to a weighed crucible, dry at $110-120^\circ\text{C}$., and weigh the residue as *crude fibre and ash*: this weight subtracted from the previous one shows the *total alkali extract*. This extract is largely *albuminous matter* and various modifications of *pectic acid*, Fremy's "*cutose*," and various *colouring humus*, and *decomposition compounds* in small amounts. Most of the extracted substances may be precipitated by excess of an acid with or without the presence of alcohol.

* CHEMICAL NEWS, vol. xxxii., p. 19, from *Gaz. Chim. Ital.* 1875, No. 3; *Pro Am. Ph. Assn.* 1875, p. 341.

X. Cellulose.

The crude fibre from IX. should be treated with from 50 to 100 c.c. of U.S.P. solution of chlorinated soda and allowed to stand twenty-four hours. If not then bleached white, slightly acidulate with hydrochloric acid, and set aside for another day. Filter through fine linen, or Gooch's filter, wash with hot water, dry at 110–120° C., and weigh, ash free, as *cellulose*. The loss of weight by this treatment state as *lignose* and *colour*.

Remarks.

It is advisable to determine always, in addition to what has already been directed, the amounts extracted directly from the sample by water, ether, alcohol of various percentages, methylic alcohol, naphtha, chloroform, carbon disulphide, &c. In each extract estimate *total organic matter* and *ash*, determine qualitatively, and quantitatively when possible, its constituents, by treating with such solvents and reagents as are indicated. Each extract being composed of certain distinct substances, it is necessary to account for them in every case.

The amounts present of some constituents may be found by subtracting the weight extracted by some one solvent from the weight extracted by some other. It will be seen that this is a method of limited applicability, which can only be applied in those cases where the difference between the solvent action of the two liquids is very sharply defined. Certain special methods for the estimation of single constituents may be used, care being taken that all interfering substances be first removed. The methods of preparation of known substances as given in Husemann's *Pflanzenstoffe*, and to a considerable extent in Watts's *Dictionary*, may serve as suggestions for work. Treatment with benzole, eighty per cent alcohol, and water, removes from nearly all plants the constituents of greatest chemical and medicinal interest, but in analyses of grains, fodder, and food materials those compounds extracted by dilute acids and alkalis have great value. There are substances in plants, seemingly isomers of starch and cellulose, which have properties more or less resembling those of cellulose, and are changed by boiling with dilute acids to glucose. In absence of an established nomenclature it has seemed best to use the term "starch isomers," or "amylaceous cellulose" for these substances,* while those constituents, *not albuminous*, which are removed by dilute alkali have been termed "alkali extract." These substances have been investigated by various chemists, but no definite and authoritative nomenclature has yet been adopted. Thomsen gives the name "holz-gummi,"† *wood-gum*, to a white substance extracted from plants by dilute sodic hydrate, while Fremy regards these various compounds as modifications of pectic acid, pectin, and "cellulose bodies."‡ Starch also may exist in some seeds (as of sweet corn) in a form soluble in water.§

It will be seen that the field for investigation is limitless, and almost unoccupied as yet, and that there is great need for improved methods for proximate analysis. The analyst will find that a study of any common plant will require of him much more than unthinking, mechanical habits of manipulation, while every careful investigation will reveal to him some constituents deserving more full and accurate study.

Reddening of Carbolic Acid.—H. Hager.—This change of colour which is occasionally observed in the purest carbolic acid, is ascribed to the absorption of ammonia, or rather of ammonium nitrite, from the atmosphere. In glass vessels hermetically sealed no change of colour takes place.—*Pharm. Central Halle*.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 3, 1880.

Prof. H. E. Roscoe, President, in the Chair.

THE PRESIDENT announced that a ballot would take place at the next meeting of the Society on June 17.

The following certificates were read for the first time :—
J. K. Crow, A. G. Chamberlain, H. A. Mott.

The PRESIDENT then called upon Mr. W. H. PERKIN to read a paper "*On some Products of the Oxidation of Para-toluydin*." In a previous paper on mauvein (*Journ. Chem. Soc.*, 1879, 728) the author briefly referred to some experiments on the oxidation of para-toluydin by chromic acid: the present paper continues the study of this reaction. A solution of the sulphate of para-toluydin was mixed with a solution of potassic dichromate. After twenty-four hours a large quantity of a brown precipitate formed. This was collected, washed, dried, and extracted with benzene. The benzene was coloured a rich orange-red, and on evaporation furnished a red crystalline product. After a great deal of trouble and many re-crystallisations this was separated into two new bases, one less soluble than the other. The least soluble gave on analysis numbers indicating the formula C_7H_7N . Its crystalline form has been described and figured by Mr. L. Fletcher. The substance melts at 216° to 220°: it crystallises in beautiful rhombohedral garnet-red crystals from benzol and ether. It readily dissolves in alcohol acidified with hydrochloric acid, forming a brownish red solution, changing to a red purple. With concentrated sulphuric acid this substance forms a magnificent blue colour. A platinum salt was formed, indicating as the formula for this substance $C_{21}H_{21}N_3$. It is designated by the author Tri-para-tolylene-triamine. The more soluble base was also obtained pure after much trouble. Analyses indicated the formula $C_{28}H_{27}N_3$. It may be derived from the preceding substance by replacing one atom of hydrogen by tolyl, and is therefore designated Toly-tri-para-tolylene-triamine. It melts at about 175° C., and dissolves in alcohol acidified with acetic acid with a purple colour; it separates from its ethereal solution in small, flat, prismatic crystals; it is an organic base, forming easily decomposable salts. In concentrated sulphuric acid it dissolves with a grey violet colour, quickly changing to a pale yellowish green. A platinum salt was prepared and analysed. No special advantage was obtained by substituting acetic for sulphuric acid in the oxidation process. By using a solution of para-toluydin in glacial acetic acid and a solution of chromic acid in the same solvent a different reaction was obtained. Golden-yellow needles were deposited, which, when purified, proved to be parazo-toluen; it fused at 143°.

Dr. DUPRÉ then read a paper "*On the Detection of Foreign Colouring Matters in Wine*." In January, 1876, the author proved that the true colouring-matter of wine dialysed very slowly (*Analyst*, 2, 26, and 11, 186), and that the various colouring matters, said to be used in adulterating wine, dialysed freely; also that small cubes of jelly, $\frac{3}{4}$ " in the side, cut from a plate of jelly with a wet knife, could be used with great advantage in the analysis, as follows :—Such a cube immersed for a definite period in the wine is taken out, washed with a little water, and a central slice cut out. If the wine is pure it will be found that in 24 to 48 hours the colour has penetrated but a very little way into the cube, whereas many colouring matters will have reached the centre. Since then the author has extended his researches, and the present paper gives an account of experiments with many colouring matters, made as above described. The results confirm those already obtained. The colouring matter of pure wine dialyses very slowly. The only artificial colouring matter tried which does not dialyse quickly is that of alkanet root. Althæa, beet, bil-

* U.S. Dept. of Agric. Report, 1878, p. 189.

† Kolbe's *Journ. Prakt. Chem.*, band 19, p. 146.

‡ *Compt. Rend.*, lxxiii., 1136; *Journ., Chem. Soc.*, 1877, p. 229.

§ U.S. Dept. Agric. Report, 1878, pp. 153–155.

berry, Brazil wood, carnation, cherry (red and black), clematis (purple), cochineal, cranberry, currant (red and black), elderberry, indigo, litmus, logwood, inallow, raspberry, red cabbage, red poppy, rhatany root, rosanilin, saffron and strawberry, all dialyse, and penetrate rapidly into the jelly. Alkanet can be readily distinguished from the colouring matter of wine by giving an absorption spectrum of three bands in acid solution. Ammonia changes wine colouring matter to greenish brown, which gives one indistinct absorption-band in the yellow. Alkanet is turned blue, and gives two absorption-bands. The nature of the colouring-matter can often be detected by the colour of the gelatin cube, *e.g.*, indigo, logwood, &c.; if not, it is best to dialyse the wine through parchment-paper, and then apply the tests suggested by Gautier and others to the dialysate. The cubes of jelly are cut from a jelly containing 10 per cent of dry gelatin, to which it is preferable to add 10 per cent of glycerin.

The PRESIDENT asked if Dr. Dupré could account for the non-diffusibility of the colouring matter: whether it was of an albuminous nature?

Dr. FRANKLAND suggested that the colouring matter might be in suspension: that it did not subside was no evidence against this view, as gold had been obtained by Faraday in a state of suspension so fine that it did not settle after many years standing.

Prof. EMERSON REYNOLDS asked if the age of the wine did not alter the dialysing power of the colouring matter.

Mr. PAGE suggested, as a substitute for the sheets of paper stretched on hoops, usually employed in dialysis, tubes made of parchment-paper, which could be obtained of almost any length, and were very convenient, as no joint had to be made. They can be obtained of Karl Brandegger, Ellwangen, Würthemberg, at 4 to 5 shillings per 100 metres.

Dr. DUPRÉ, in reply, did not think that the colouring matter was albuminous, but that it resembled somewhat a tannin. In his opinion the colouring matter was not in suspension, in proof of which it did not colour the must until sufficient alcohol had formed to bring it into solution. He had examined 1834 port and two-year old port without detecting any difference in the dialysing power of the colouring matter.

The two following papers were read by Dr. FRANKLAND:—

"On the Action of Organo-zinc Compounds upon Nitriles and their Analogues. I. Action of Zinc-ethyl on Azo-benzene," by E. FRANKLAND and D. A. LOUIS. When azo-benzene is added to an ethereal solution of zinc-ethyl, and the mixture warmed to the boiling-point of the ether, a reaction commences, accompanied with the evolution of much gas. As soon as it ceases it can be continued by adding a fresh quantity of azo-benzene, and so on until the reaction becomes sluggish, care being taken to employ an excess of zinc-ethyl. An amber coloured jelly was thus obtained: it was decomposed by water, much gas being evolved; a reddish brown oil separated out with the zincic hydrate. By treatment with caustic soda the zincic hydrate was removed. The oil thus obtained was purified, and proved by analysis, &c., to consist mainly of aniline. The gas evolved during the reaction consisted of 3 vols. of ethylene and 1 vol. of ethylic hydride. From 80 grms. of azo-benzene 70 grms. of aniline were obtained. Besides the aniline, a small quantity of a high boiling-point viscid oil was obtained, the investigation of which is not yet complete.

"II. On the Action of Zinc-ethyl upon Benzo-nitrile," by E. FRANKLAND and J. CASTELL EVANS. Equal volumes of zinc-ethyl and benzo-nitrile were heated in a sealed tube to 150°. On cooling the contents solidified to a brownish mass. After treatment with alcohol and hydrochloric acid, white needles remained, sparingly soluble in alcohol, but dissolving readily in carbon disulphide, fusing at 229°. By analysis, &c., this substance was proved to be cyaphenine, $C_{21}H_{15}N_3$. The above reaction also takes place under ordinary pressure. On heating cyaphenine with concentrated hydrochloric acid in a sealed tube to

250° it can be converted entirely into benzoic acid and ammonia. The liquid obtained by treating the product of the zinc-ethyl reaction (after decomposition by alcohol) with hydrochloric acid deposited, on standing, faintly greenish six-sided plates. These, after purification, gave numbers indicating the formula $C_{17}H_{21}N_2Cl$. A further investigation of this hydrochlorate, which fuses at 257°, is promised. The gas evolved during the action of zinc-ethyl on benzo-nitrile consisted of equal volumes of C_2H_4 and C_2H_6 .

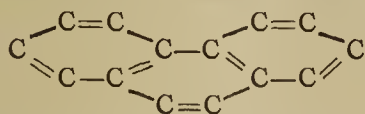
Dr. ARMSTRONG suggested that it would be better to get rid of such terms as ethylic hydride. The researches of Dale and Schorlemmer had proved that there was no difference between ethane and ethylic hydride: moreover, the latter name suggested a distinction between one atom of hydrogen and the other atoms of that element which did not exist.

Dr. FRANKLAND quite agreed with Dr. Armstrong as to the desirability of abolishing the term if the identity of the two substances was fully proved; but, in his opinion, we should be very careful in abolishing an idea because it did not conform to the theories of the day. He quite admitted that Schorlemmer had proved the identity of a fraction of the products of the action of Cl on the two gases, but nothing had been done with the remainder of the products. He had waited for several years for any further investigation of the subject, and at last had determined to take up the question himself.

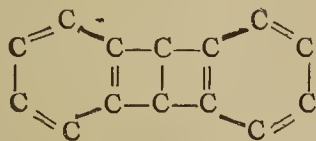
Dr. JAPP contended for the convenience of the term methylic hydride, irrespective of the question of isomerism, as in Butlerow's reaction.

Prof. HARTLEY then read a communication "*On the Relation between the Molecular Structure of Carbon Compounds and their Absorption-spectra.*" In a previous paper, in which the author was associated with Prof. A. K. Huntington, it has been shown (1) that every increment of CH_2 in a homologous series of alcohols or of acids effects an absorption of the more refrangible of the ultra-violet rays, so that the greater the number of carbon atoms in the molecule the shorter is the transmitted spectrum; (2) that the terpenes always transmit continuous spectra, but polymerisation largely increases their absorptive power; (3) that benzene and its derivatives invariably show, in addition to abnormally great absorptive power, the peculiarity of absorption-bands. After taking into consideration the numerous substances examined the conclusion seems inevitable that banded absorption-spectra were caused by the double linking of three pairs of carbon atoms in a compactly closed chain; and in the present paper the author has studied the effects of various atomic groupings on the absorption of ultra-violet rays. The first question was whether substances with two doubly-linked adjacent carbon atoms exhibit any peculiarities in their absorption-spectra. To decide this point, ethylene, amylene, and allyl alcohol were selected for examination, but in neither case were any absorption-bands seen. To ascertain the effect of the treble linking of two carbon atoms, the spectra of acetylene and valerylene were examined, but no absorption-bands were noticed. In all cases, therefore, where the carbon atoms are arranged in an open chain no absorption-bands are seen. In no carbon compound has any arrangement of the hydrogen or oxygen atoms been identified with the presence of absorption-bands. With hydrocarbons containing at least six atoms of carbon and their derivatives there are three possible arrangements which admit of the carbon atoms forming a closed chain:—(1) Three pairs of carbon atoms may be doubly linked, as is assumed to be the case in benzene; (2) two pairs may be doubly linked; (3) the six atoms may be singly linked. There are reasons for believing that oil of turpentine and terebene have two pairs of carbon atoms doubly linked, and that a closed chain, including these two pairs of atoms, forms the nucleus of such substances. These bodies exhibit no absorption-bands, so the author concludes that a closed chain of carbon atoms, in which only two pairs are doubly linked,

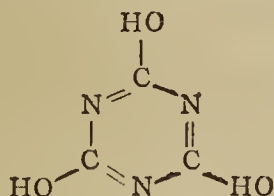
exhibits no absorption-bands. Now the constitutional formula of camphor is probably founded on a closed chain of carbon atoms. It is found to be very diactinic, more diactinic than the terpenes, so that probably its atoms are not so compactly united; this is consistent with a singly linked closed chain of carbon atoms. Camphoric acid similarly shows no absorption-bands. The author therefore concludes that no molecular arrangement of carbon atoms causes selective absorption unless three pairs are doubly linked together in a closed chain. The author next considers the absorption-spectra of condensed benzene nuclei. It was expected, from the generally accepted views as to the constitution of naphthalene and anthracene, that these substances would give a larger number of absorption-bands than benzene, and that the bands would be of greater intensity, *i.e.*, they would be capable of withstanding a great degree of dilution before they disappeared. The results obtained are rather remarkable; thus, a solution of naphthalene containing 1 in 60,000 shows four strongly-marked bands. Phenanthren contains the carbons of three benzene rings disposed as follows:—



It shows three strong absorption-bands in a solution containing 1 in 4000. Anthracene, which may be considered to have a structure—



was also examined. It was dissolved in glacial acetic acid. It has a considerable absorption when the dilution is carried as far as 1 in 50 millions. Pyridin gives an absorption-spectrum. Hydrocyanic acid is very diactinic. Cyanuric acid is not, and the author concludes that it has the formula—



Photographs of many of the spectra were exhibited.

The PRESIDENT congratulated Prof. Hartley on his interesting results; such work was of the greatest importance. The question as to the connection between structure and physical properties would probably form one of the most interesting chapters in the chemistry of the coming time.

Mr. GROVES asked if Prof. Hartley had investigated the effect of substitution in the side chains.

Prof. EMERSON REYNOLDS asked if the absorption-spectra were sufficiently definite to identify the various substances. The work seemed to him to be most important; it gave some air of reality to the three links and two links of which chemists now speak so frequently.

Dr. JAPP suggested the investigation of the class of trimolecular nitriles, so as to have compounds with a closed chain like the benzene ring.

Prof. HARTLEY, in reply, said that if the substances were pure it was perfectly easy to identify them, and the quantity of some substances, even in mixtures, could be roughly estimated.

"On a Simple Method of Determining Vapour-densities in the Barometer Vacuum," by C. A. BELL and F. L. TEED. Notwithstanding the simplicity of the apparatus recently proposed by Meyer, many cases remain—*e.g.*, those of easily decomposable substances of high boiling-

point, &c.—in which the older method of Hofmann would be preferable. Moreover most liquids volatilise in a vacuum at 100°, and this temperature can easily be maintained for any length of time. The authors have introduced two new devices:—(1) By varying the external pressure, or otherwise the vapour is made to occupy a known volume; and (2) its pressure is directly determined by a single observation, which is independent of the atmospheric pressure. Thus the calculations are enormously simplified, and errors of observation are avoided. The simplest form of the apparatus consists of a glass cylinder, 34 c.m. long and 3.3 c.m. internal diameter, closed at its upper end. To its lower end is fused a stout glass tube, 8 m.m. internal diameter and 83 c.m. long. 5 c.m. below the junction with the cylinder a glass tube is fused in so as to form a T, and bent up so as to be parallel with the cylinder; it is sealed off a little below the upper end of the cylinder. The lower end of the long glass tube is closed by an india-rubber cork, through which passes a glass stopcock. The whole of the upper part of the apparatus is surrounded by a steam jacket. A fine line is etched on the glass tube about 1 c.m. below its junction with the cylinder, and from a point on exactly the same level the side tube, which serves as a barometer, is graduated upwards in millimetres. The apparatus is filled with mercury, and after various precautions, the liquid introduced in a thin glass bulb, converted into vapour, and the pressure in the barometer tube observed, at which the vapour depresses the mercury to the mark etched on the tube. The volume of the cylinder being known the calculation is extremely simple.

Mr. KINGZETT communicated verbally some results on the Oxidation of Phosphorus in Moist Air. He had proved that under these circumstances, both ozone and peroxide of hydrogen were formed. Details are promised in a future paper.

The Society then adjourned to June 17, when a ballot for the election of Fellows will be held, and the following papers read:—"On Penta-thionic Acid," by T. Takamatsu and Watson Smith; "On a Crystal of Diamond," by Harry Baker; "Some Orcinol Derivatives," by J. Stenhouse and C. E. Groves; "On the Determination of Carbon in Soils," by R. Warrington and W. A. Peake; "Note on Camphydrene," by H. E. Armstrong; "On the Action of Nitric Acid on Dipara-tolyl-guanidine," by A. G. Perkin.

MINERALOGICAL SOCIETY.

At a meeting of the above Society on June 1, at 116, Victoria Street, S.W., Prof. BONNEY, F.R.S., presiding, a paper on "Recent Mineral- and Metal-Growth at Ordinary Temperatures, under Ordinary Conditions," was read by Mr. READWIN, F.G.S., who illustrated his novel views by the exhibition of interesting examples in gold, silver, copper, iron, diamond, &c.

CORRESPONDENCE.

HYDROMETERS.

To the Editor of the Chemical News.

SIR,—In testing the strengths of liquors, both in the laboratory and the works, it is often necessary to use the utmost expedition—not a moment is to be lost in getting hold of the right hydrometer. At present, in cases where five or six Twaddle hydrometers of different strengths are in use, most valuable time is frequently lost in hunting for the number upon the stem till the proper hydrometer can be got hold of. I beg, therefore, to offer the following simple suggestion to enterprising makers of hydrometers,

knowing that it would be of substantial service to their customers:—

The paper upon which the figures are printed to be continued right up to the top of the stem, and upon it, near the top, thick black bars to be printed, corresponding with the number upon the hydrometer—one for No. 1, two for No. 2, and so on. The bars to be placed in pairs—one pair for No. 2, a pair and an additional bar for No. 3, two pairs for No. 4, and so on, as the eye can more instantaneously read off this arrangement than bars at equal distances.—I am, &c.,

FRANK.

June 4, 1880.

SPENCE'S METAL.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 87, you give the substance of a paper read before the Society of Arts, by Mr. Granville Cole, Ph.D., in which he predicted an extensive field for the useful application of the so-called Spence's metal for many industrial purposes. Will you kindly inform me as to whether this compound has ever been tried on the large scale, and if so with what success; also, as to where it may be obtained in quantity sufficient to determine its value for manufacturing purposes?—I am, &c.,

W. GOLDSBOROUGH WHITTAM.

WATER ANALYSIS.

To the Editor of the Chemical News.

SIR,—I reserve what I have to say in answer to the criticisms of Dr. Frankland and others (and I have a good deal to say) on my paper on "River Water" until such time as I can reply in detail.

Dr. Mills's letter, however (CHEMICAL NEWS, vol. xli., p. 260), calls for one remark. He says:—"It is not therefore possible to calculate from the 'oxygen consumed' by any given water how much organic matter it contains." Granted: and if Dr. Mills had added, "nor by any other known process" he would have been equally as correct. For the combustion process is no more an *absolute* test of the quantity of organic matter in a water than the oxygen process.

Let me state a case in illustration. If I were to send Dr. Mills for analysis a sample of distilled water containing sufficient hydrocyanic acid to poison a household, he would report to me that, judged by the combustion process, the water was perfectly wholesome and free from organic matter of an injurious nature.—I am, &c.,

C. MEYMOTT TIDY.

3, Mandeville Place, Manchester Sq., W.,
June 5, 1880.

WATER ANALYSIS.

To the Editor of the Chemical News.

SIR,—I read Mr. Kingzett's paper on "Organic Matter in Water" (CHEMICAL NEWS, vol. xli., p. 254) with much interest. Mr. Kingzett conceives "that under certain circumstances a water might be found containing such a small amount of organic matter in a given volume that it would be passed by Tidy's process as of 'great organic purity,' and yet might under given conditions pass into a state of putridity, and therefore become pernicious." He is quite right in his supposition. A fairly large experience, extending now over a good many years of cases of typhoid fever caused by impure water, enables me to affirm unhesitatingly that the worst waters are frequently those that contain the smallest amount of organic matter; possibly, as Mr. Kingzett says, for the reason "that as it becomes less in quantity it becomes more pernicious in quality." My experience as a chemist entirely bears out

Prof. Huxley's opinion, as a biologist, that so far as organic matter is concerned "a water may be as pure as can be as regards chemical analysis, and yet be as regards the human body as deadly as prussic acid." As I have recently published my views on this subject at some length ("Potable Water," published by J. and A. Churchill), I will only say here that to lay the chief stress on organic matter by whatever process it may be estimated is worse than useless. It is the *nature* of the organic matter as testified to by collateral chemical evidence that is the all-important item.—I am, &c.,

CHARLES EKIN, F.C.S.

June 8, 1880.

AMMONIA FROM THE NITROGEN OF THE ATMOSPHERE.

To the Editor of the Chemical News.

SIR,—Will you kindly allow me to say a few "last words" in reply to the long letter of E. S., in the CHEMICAL NEWS, vol. xli., p. 259.

I wish to say that I am not anxious that my representation of this matter should be taken without question. I simply desire that those who are interested in the matter should examine the two specifications and my note in answer to the bare assertion in E. S.'s first note as to anticipation or infringement, and there I am content to leave the matter.

But as to this letter I should like to make a few observations. In the specification of T. C. and E. S. it is directed that the nitrogen be heated to an "intensely high heat," and "as high a heat as possible," and also that the nitrogen and hydrogen be kept at a "prolonged white heat" to bring them to the "nascent or atomic state"! that it may be possible for combination to take place. But in this letter E. S. says his experience tells him that a "full red heat" is all that is necessary, and this, he says now, is a high heat. But surely he does not mean to call this red heat white, either prolonged or otherwise, which the specification asserts to be necessary. Now a white heat is far from as "high a heat as possible," even in a fire-clay furnace, which heat the specification also directs to be produced.

According to the experiments of M. Pouillet on heated furnaces, a full red heat is equal to about 916° C.; a white heat is equal to about 1316° C.; and a dazzling white heat to about 1516° C.

Next, as to the decomposition of ammonia at a red heat. About this there can be no doubt; the fact is stated in every chemical primer. But if a red heat destroys ammonia, then free ammonia cannot exist at that heat; but ammonium chloride can, and this is why we prefer to make that salt direct.

We have long ago made the experiments suggested by E. S. We have used as a deoxidising medium coke, wood-charcoal of different kinds, and peat-charcoal, which are non-nitrogenous. But besides these, we have used various other substances, which we have taken care were absolutely free from nitrogen, and in all cases have found the result the same under similar conditions. The cheapness of coal-dust alone determined us in favour of it.

Finally, E. S. says that the leading idea in the patent of T. C. and E. S. is the decomposition of water and utilising the H in the formation NH_3 ; but this is precisely the leading idea in very many patents for the last thirty years, so that I am afraid T. C. and E. S. have been "anticipated" many times over.—I am, &c.,

J. B. THOMPSON.

Reducing Agents in Refining Copper and Obtaining Copper Alloys.—M. Levitzki.—Manganese, in the form of copper manganese, is the best agent for reducing the oxides mixed with the copper of commerce.—*Berg. u. Hütten Zeitung.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 18, May 3, 1880.

Gases retained by Occlusion in Aluminium and Magnesium.—M. Dumas.—On submitting aluminium in a vacuum to the action of a temperature gradually raised to the softening-point of porcelain, and causing the mercurial pump to act upon the retort containing the metal till it is completely exhausted, considerable quantities of gas are withdrawn. The liberation of the gas from the metal seems to take place suddenly towards a white-red heat. 200 grms. aluminium, occupying 80 c.c., gave 89.5 c.c. of gas at the temperature of 17° and the pressure of 755 m.m. The gas consisted of carbonic acid 1.5, and hydrogen 88 c.c. Carbonic oxide, nitrogen, and oxygen were absent. Magnesium, on similar treatment, gave off, weight for weight, double the volume of gas evolved by aluminium, 20 grs. of the metal giving 12.3 c.c. hydrogen and 4.1 c.c. carbonic oxide. Another specimen of magnesium evolved hydrogen 28.1 c.c., carbonic oxide 1.9 c.c., and carbonic acid 1.5 c.c. The magnesium which is volatilised in the retort crystallises on condensing, and may serve for re-determining the atomic weight of the metal, which is open to some doubts. Further, the vapour-density of magnesium may be determined by volatilising it in a vacuum. It appears, then, that hydrogen is occluded by magnesium and aluminium, as is oxygen by silver. It is possible that other gases may be selected by other metals.

Experimental Researches on the Decomposition of Certain Explosives in Closed Vessels; Composition of the Gases Formed.—MM. Sarrau and Vieille.—The explosives experimented upon were pure gun-cotton, gun-cotton mixed with equal parts potassium nitrate, mixture of 40 parts gun-cotton and 60 ammonium nitrate, nitro-glycerine, and common mining powder. The composition of the resulting gases is given in a table. The mining powder alone yielded a hydrocarbon and hydrogen sulphide, and in common with pure gun-cotton, carbonic oxide, and free hydrogen. The other three compounds evolved free oxygen, and their carbon was entirely liberated as dioxide.

General Equation, giving the Relation for all Liquids between their Temperature and the Maximum Tension of their Vapours at this Temperature.—Raoul Pictet.—A mathematical paper, not susceptible of useful abstraction.

Laws which Govern Matter in the Spheroidal State.—P. H. Boutigny.—1. The temperature of bodies in the spheroidal state is always below their ebullition-point; that of water is +97°. 2. A body in the spheroidal state never places itself in an equilibrium of temperature with the containing vessel; its temperature is always in a state of stable equilibrium, where it is in a capsule in the open air, or in the muffle of a cupelling furnace. But if the body in the spheroidal state does not place itself in an equilibrium of temperature the emanating vapour always does. These two phenomena are very distinctly manifested on operating in a hollow sphere. 3. Matter in the spheroidal state reflects radiant heat. 4. The volumes of matter in the spheroidal state are inversely as their densities. 5. There is a repulsive force at a sensible distance, so that the water, &c., is not sustained in the capsule by being enveloped in watery vapour.

Dissociation of Butyl-chloral Hydrate.—MM. R. Engel and Moitessier.—The authors consider that their experiments give further confirmation to the law that the dissociation of a body, both of whose constituents are

volatile, does not take place in presence of the vapour of one of the components at a tension higher than the dissociation tension of the compound.

Determination of Glycerin in Wines.—M. Raynaud.—The determination of glycerin is easily effected with exactness in pure wines, but the same process gives erroneous results in plastered wines containing potassium sulphate. The glycerin holds in solution potassium carbonate and extractive matter, even in presence of a mixture of ether and alcohol. The author therefore proceeds as follows:—He evaporates the wine to one-fifth of its volume, adds hydro-fluosilicic acid, and then alcohol. The alkalies are thus thrown down, and the fluo-silicates may be collected if it is intended to determine the potassium and sodium. A slight excess of barium hydroxide is then added, and the whole is evaporated to dryness with a quantity of quartz sand. The residue is exhausted with a mixture of alcohol and ether, absolutely pure, the solution is slowly evaporated, and the residue is left for twenty-four hours in a dry vacuum over phosphoric anhydride. It is pure glycerin.

On Legumin.—A. Bleunard.—The reaction of barium hydroxide upon legumin yields the same results as upon albumin. There is a little more carbonic acid produced and a little less oxalic and acetic acid. There is in the fixed residue of legumin an excess of the gluco-proteins, $C_7H_{14}N_2O_4$, and $C_{10}H_{20}N_2O_4$, whilst the gluco-protein, $C_9H_{18}N_2O_4$, which is dominant in albumen is rarer in legumin.

On Gelose.—M. Porumtaru.—The author assigns to gelose the formula $C_6H_{10}O_5$, which is analogous to that of the starch of lichenin, inulin, and tunisin.

Chemiker Zeitung.
No. 12, 1880.

Physiological Action of Tar Colours and their Organic Raw Materials.—Dr. Grandhomme.—Already noticed.

New Reaction with Gum.—C. Reichel.—The carbohydrates yield coloured compounds with phenols in presence of acids. If gum and orcin are boiled for a time in concentrated hydrochloric acid a red colouration appears, which passes into violet, and a blue colouring-matter is ultimately deposited. On the addition of alcohol a greenish blue solution is produced, which, on the addition of alkalies, turns violet with a green fluorescence. Similar is the behaviour of cherry-gum and bassorin, whilst dextrin, starch, cellulose, grape-sugar, cane-sugar, and milk-sugar, if similarly treated, yield yellow or brownish colourations, which dissolve in alcohol with a yellow or orange colour. Their alkaline solutions have a greenish fluorescence.—*Ber. Oest. Ges. Förd. Chem. Industrie.*

Detection of Mercury.—H. v. Jüptner.—Scarlet mercuric iodide appears perfectly white in the light of the sodium flame.

Die Chemische Industrie.
No. 1, January, 1880.

Regulations concerning the Sale of Alcohol Duty-free for Industrial Purposes.—To each litre of absolute alcohol present in the sample, at least 0.1 litre of wood-spirit must be added. Only such wood-spirit is to be used which has been sanctioned by the excise authorities, and has remained in bond until required for use. The wood-spirit must be tested as to its specific gravity, boiling-point, miscibility with water, and soda-lye, and power of taking up bromine.

Notes on the Chemical Manufactures of the South of France.—Sulphate of soda is manufactured by Pechiney and Co. by the application of artificial cold. The mother-liquors of the salt works containing sodium chloride and magnesium sulphate (*sel mixte*) are cooled down to -6°

by the aid of a Carré freezing machine. The Glauber's salt is deposited as a crystalline mud, and is raised by elevators into draining receptacles [This seems to be a very close approach to Fournier's patent.—*Ed. C.N.*], and is then dehydrated as follows:—About 1500 kilos. are melted in a little mother-liquor in a wooden cistern, heated by means of a copper worm, and provided with an agitator. About 250 kilos. of common salt are then added. In proportion as the salt dissolves, anhydrous sulphate of soda is precipitated, and at the end of the process 85 per cent of the total amount are found thrown down free from water. The hot saline paste flows down from the precipitation tank into a filter cask with a double perforated bottom, and after it has drained it is placed in centrifugal machines, from which it is removed containing only 0.5 sodium chloride and 5 per cent water.

Comparison of the Apparatus for the Concentration of Sulphuric Acid according to the Systems of Delplace and Prentice.—G. Delplace.—The author contends that his arrangement requires less platinum and less fuel, and yields a larger quantity of concentrated acid.

MISCELLANEOUS.

Chemical Specimens.—We hear from Dr. Theodor Schuchardt, of Goerlitz, that he has prepared metallic potassium in fine large crystals (*octahedre quadratic*), 1 to 1.1 centims. long, preserved in closed tubes in an atmosphere of purified hydrogen; metallic sodium, but in smaller crystals; the alloy of 1 equivalent of K with 1 equivalent Na, like mercury, solidifying at -8°C ., preserved in hydrogen; metallic chromium in small regular octahedra, by fusion of sesquichloride of chromium, zinc, and chloride of sodium.

MEETINGS FOR THE WEEK.

SATURDAY, 12th.—Physical, 3. Dr. R. C. Shettle, "On the Earth's Rotation as Influenced by Solar Energy." R. H. Ridout, "Note on a Simple Method of Amplifying Small Motions."

MONDAY, 14th.—Royal Geographical, 8.30.

TUESDAY, 15th.—Zoological, 8.30.

WEDNESDAY, 16th.—Meteorological, 7.

THURSDAY, 17th.—Chemical, 8. T. Takamatsu and Watson Smith, "On Penta-thionic Acid." Harry Baker, "On a Crystal of Diamond." J. Stenhouse and C. E. Groves, "Some Orcinol Derivatives." R. Warrington and W. A. Peake, "On the Determination of Carbon in Soil." H. E. Armstrong, "Note on Camphydrene." A. G. Perkin, "On the Action of Nitric Acid on Dipara-tolyl-guanidine." Ballot for the Election of Fellows.

Royal, 4.30.

Philosophical Club, 6.30.

TO CORRESPONDENTS.

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JOS. HERON,
Town Clerk.

Town Hall, Manchester,
May 15, 1880.

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TESTIMONIAL.

"We have used the Blue Staffordshire Brick or Sulphuric Acid Towers, &c., manufactured by Messrs. Field, for the last fifteen years, during which time they have given great satisfaction. We recommend them to the Chemical Trade, believing there is nothing better in the market."—(Signed) WILLIAM HUNT and SONS, Lea Brook Alkali Works, near Wednesbury, Staffordshire, February, 1878.

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THE CHEMICAL NEWS.

VOL. XLI. No. 1073.

ON A FOURTH STATE OF MATTER.*

IN A LETTER TO THE SECRETARY OF THE ROYAL SOCIETY.

By W. CROOKES, F.R.S.

7, Kensington Park Gardens, London, W.,
April 29, 1880.

DEAR PROFESSOR STOKES,

In introducing the discussion on Mr. Spottiswoode and Mr. Moulton's paper on the "Sensitive State of Vacuum Discharges," at the meeting of the Royal Society on April 15, Dr. de la Rue, who occupied the Chair, good-naturedly challenged me to substantiate my statement that there is such a thing as a fourth or ultra-gaseous state of matter.

I had no time then to enter fully into the subject; nor was I prepared, on the spur of the moment, to marshal all the facts and reasons which have led me to the conclusion. But as I find that many other scientific men besides Dr. de la Rue are in doubt as to whether matter has been shown to exist in a state beyond that of gas, I will now endeavour to substantiate my position.

I will commence by explaining what seems to me to be the constitution of matter in its three states of solid, liquid, and gas.

1. First as to solids:—These are composed of discontinuous molecules, separated from each other by a space which is relatively large—possibly enormous—in comparison with the diameter of the central nucleus we call *molecule*. These molecules, themselves built up of *atoms*, are governed by certain forces. Two of these forces I will here refer to—attraction and motion. Attraction when exerted at sensible distances is known as gravitation, but when the distances are molecular it is called *adhesion* and *cohesion*. Attraction appears to be independent of absolute temperature; it increases as the distance between the molecules diminishes; and were there no other counter-acting force the result would be a mass of molecules in actual contact, with no molecular movement whatever—a state of things beyond our conception—a state, too, which would probably result in the creation of something that, according to our present views, would not be *matter*.

This force of cohesion is counterbalanced by the movements of individual molecules themselves, movements varying directly with the temperature, increasing and diminishing in amplitude as the temperature rises and falls.

The molecules in solids do not travel from one part to another, but possess adhesion and retain fixity of position about their centres of oscillation. Matter, as we know it, has so high an absolute temperature that the movements of the molecules are large in comparison with their diameter, for the mass must be able to bear a reduction of temperature of nearly 300° C. before the amplitude of the molecular excursions would vanish.

The state of solidity therefore—the state which we are in the habit of considering *par excellence* as that of *matter*—is merely the effect on our senses of the motion of the discrete molecules among themselves.

Solids exist of all consistences, from the hardest metal, the most elastic crystal, down to thinnest jelly. A perfect solid would have no viscosity, *i.e.*, when rendered discontinuous or divided by the forcible passage of a harder solid, it would not close up behind and again become continuous.

In solid bodies the cohesion varies according to some unknown factor, which we call chemical constitution; hence each kind of solid matter requires raising to a dif-

ferent temperature before the oscillating molecules lose their fixed position with reference to one another. At this point, varying in different bodies through a very wide range of temperature, the solid becomes liquid.

2. In liquids the force of cohesion is very much reduced, and the adhesion or the fixity of position of the centres of oscillation of the molecules is destroyed. When artificially heated, the inter-molecular movements increase in proportion as the temperature rises, until at last cohesion is broken down, and the molecules fly off into space with enormous velocities.

Liquids possess the property of viscosity—that is to say, they offer a certain opposition to the passage of solid bodies; at the same time they cannot permanently resist such opposition, however slight, if continuously applied. Liquids vary in consistency from the hard, brittle, apparently solid pitch, to the lightest and most ethereal liquid capable of existing at any particular temperature.

The state of liquidity, therefore, is due to inter-molecular motions of a larger and more tumultuous character than those which characterise the solid state.

3. In gases the molecules fly about in every conceivable direction, with constant collisions and enormous and constantly varying velocities, and their mean free path is sufficiently great to release them from the force of adhesion. Being free to move, the molecules exert pressure in all directions, and were it not for gravitation they would fly off into space. The gaseous state remains so long as the collisions continue to be almost infinite in number, and of inconceivable irregularity. The state of gasity, therefore, is pre-eminently a state dependent on collisions. A given space contains millions of millions of molecules in rapid movement in all directions, each molecule having millions of encounters in a second. In such a case, the length of the mean free path of the molecules is exceedingly small compared with the dimensions of the containing vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed.

What, then, are these molecules? Take a single lone molecule in space. Is it solid, liquid, or gas? Solid it cannot be, because the idea of solidity involves certain properties which are absent in the isolated molecule. In fact, an isolated molecule is an inconceivable entity, whether we try, like Newton, to visualise it as a little hard spherical body, or with Bosovich and Faraday, to regard it as a centre of force, or accept Sir William Thomson's vortex atom. But if the individual molecule is not solid, *a fortiori* it cannot be regarded as a liquid or gas, for these states are even more due to inter-molecular collisions than is the solid state. The individual molecules, therefore, must be classed by themselves in a distinct state or category.

The same reasoning applies to two or to any number of continuous molecules, provided their motion is arrested or controlled, so that no collisions occur between them; and even supposing this aggregation of isolated non-colliding molecules to be bodily transferred from one part of space to another, that kind of movement would not thereby cause this molecular collocation to assume the properties of gas; a molecular wind may still be supposed to consist of isolated molecules, in the same way as the discharge from a mitrailleuse consists of isolated bullets.

Matter in the fourth state is the ultimate result of gaseous expansion. By great rarefaction the free path of the molecules is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the containing vessel, the properties which constitute gasity are reduced to a minimum, and the matter then becomes exalted to an ultra-gaseous state.

But the same condition of things will be produced if by any means we can take a portion of gas, and by some extraneous force infuse order into the apparently disorderly

* A Paper read before the Royal Society, June 10, 1880.

jostling of the molecules in every direction, by coercing them into a methodical rectilinear movement. This I have shown to be the case in the phenomena which cause the movements of the radiometer, and I have rendered such motion visible in my later researches on the negative discharge in vacuum tubes. In one case the heated lamp-black and in the other the electrically excited negative pole supplies the *force majeure* which entirely or partially changes into a rectilinear motion the irregular vibration in all directions; and according to the extent to which this onward movement has replaced the irregular motions which constitute the essence of the gaseous condition, to that extent do I consider that the molecules have assumed the condition of radiant matter.

Between the third and fourth states there is no sharp line of demarcation, any more than there is between the solid and liquid states, or the liquid and gaseous states; they each merge insensibly one into the other. In the fourth state properties of matter which exist even in the third state are shown *directly*, whereas in the state of gas they are only shown *indirectly*, by viscosity and so forth.

The ordinary laws of gases are a simplification of the effects arising from the properties of matter in the fourth state; such a simplification is only permissible when the mean length of path is small compared with the dimensions of the vessel. For simplicity's sake we make abstraction of the individual molecules, and feign to our imagination *continuous* matter of which the fundamental properties—such as pressure varying as the density, and so forth—are ascertained by experiment. A gas is nothing more than an assemblage of molecules contemplated from a simplified point of view. When we deal with phenomena in which we are obliged to contemplate the molecules individually, we must not speak of the assemblage as *gas*.

These considerations lead to another and curious speculation. The molecule—intangible, invisible, and hard to be conceived—is the only true *matter*, and that which we call matter is nothing more than the effect upon our senses of the movements of molecules, or, as John Stuart Mill expresses it, “a permanent possibility of sensation.” The space covered by the motion of molecules has no more right to be called matter than the air traversed by a rifle bullet can be called lead. From this point of view, then, matter is but a mode of motion; at the absolute zero of temperature the inter-molecular movement would stop, and although *something* retaining the properties of inertia and weight would remain, *matter*, as we know it, would cease to exist.

Believe me,

Dear Professor STOKES,

Very sincerely yours,

WILLIAM CROOKES.

ON THE COMPOSITION AND ANALYSIS OF THE WELDON MUD.

By Dr. JUL. POST, in Göttingen.

AN article by Dr. Lunge, re-published in the *CHEMICAL NEWS*, vol. xli., pp. 129 and 141, appeared first in *Dingler's Polyt. Journ.* (1880, 235, 300), and has called forth a discussion between us in that magazine, the results of which I wish briefly to communicate as follows.

First of all Dr. Lunge undertakes to defend the process of examining the Weldon mud, in use at most of the works, against me, although I have not only never used a single part of the process, but also have never hitherto expressed an opinion about its worth.

The method of determining oxygen with ferrous sulphate and potassium permanganate has never been brought into use by me in the examination of the Weldon mud. I have tested the matter in the course of some of my syn-
thetical experiments on the voluntary oxidation of manganese, in which the analysis had to be made under the

various conditions:—Presence of much free hydrochloric acid, often also of substances which favoured the formation of foam (as saponine, soap, &c.); further, in state of strong concentration and at high temperatures (dilution with cold water would have impaired the accuracy in the presence of such a large quantity of liquid); as to whether, in order to get rid of the filterings, &c., which the Bunsen method makes necessary, the amount of so-called disposable oxygen could not be determined directly in the whole mass with ferrous sulphate solution and re-titration with potassium permanganate solution. I found always by comparison with the results with the Bunsen method that I had used too little permanganate. I attributed and still attribute this circumstance to the presence of considerable quantities of hydrochloric acid principally, because the hydrochloric acid occasioned too small amount of permanganate to be used; but I observed that the pink colour in the titration with permanganate, which in this case was difficult to recognise, disappeared much more rapidly than is usual under normal conditions, and explained this in this way: That the reaction at this apparently stationary point could not have reached its real end, and that a too small amount of permanganate was thus conditioned.

A further objection to my work, namely, that I had worked on specimens changed in their composition through washing with water, is met by the fact that specimens which could not have been changed in this way gave the same results. In an exact analysis made for theoretical purposes, through which we will ascertain the composition of the solid particles suspended in the calcium chloride solution, it does not appear to me sufficiently reliable to measure the mud off as such with a 5 c.c. pipette, as Dr. Lunge did, and then to make the different determinations in the specimens taken in this way. I am sustained in this opinion by the judgment of others (*CHEMICAL NEWS*, vol. xli., p. 179). We should take larger quantities of the specimens, filtered off and dried over sulphuric acid in analysis, and thus exclude very small values, and which cause inaccuracies. If we take larger specimens of the mud (25 c.c., for example), the larger bulk of precipitate (of chloride of silver) which has to be separated out, causes an inaccuracy, because of the difficulty of washing it out perfectly. After I had satisfied myself of this, I analysed only such specimens from which the CaCl_2 solution had been filtered off. The specimens in this condition were so difficult to dry and to weigh (over sulphuric acid) on account of the large amount of CaCl_2 they still contained, that I then washed them for a short time with cold water (as described in my article in the *Verhandlungen des Vereines für Gewerbefleiss*), that is, only so long that the wash-water and precipitate (1.8 per cent., as the analysis shows, calculated for dried substance) still showed CaCl_2 . There is then no occasion for reproaching me with having examined a substance changed through treatment with a 5000 fold quantity of water. I shall, in what follows, in order to remove all pretext, make these specimens alone the basis of remark.

There can be raised against the result of my analyses only one sort of objection—the methods used must be declared impracticable, or the work must be characterised as inexact. In order to meet the last objection, I have always made several analyses of similar specimens. When it is remembered that with the specimens containing calcium chloride a varying amount of water was not to be avoided, and that with the heated specimens the manganese peroxide, &c., must have varied according to the degree of the heating, the results agree satisfactorily among each other. As regards the methods, Dr. Lunge believes that the method of separating manganese and calcium with bromine, which I used, is very inexact. Under the circumstances, I had to choose between this method and the method with ammonium sulphide, as the other methods were first brought forward during and after my publications were made. Dr. Lunge gives the ammonium sulphide method the preference. This method is open to two errors.

Lime is precipitated very easily and manganese dissolved. One can correct the first error by repeated precipitations, but the second error grows thereby, while the amount of lime precipitated in the separation with bromine is so small that this process is in extensive use here in Germany, especially among metallurgists. The opponents of the method agree too that the error can be corrected by repeated precipitation, that is, with similar proportions as those in the regeneration-products in the Weldon process, viz., much manganese and very little lime. I have, moreover, in order to make this matter clear, weighed out manganese as carbonate, and calcium as calc spar, in proportions which corresponded nearly to those in which I had found the two elements in the Weldon mud. The separation was then made in exactly the same way as in the analyses under discussion. Analyses made by myself and others established the high degree of accuracy of this process for the given proportions (see Analyses in *Dingler's Polyt. Journ.*, 236, 228).

Should there be any doubt further as to whether my statement of the low lime content is right, this can be proven directly from the analysis communicated by Dr. Lunge himself; that is, supposing that he separated the lime and manganese quantitatively, and that the mud contained no alkali (as is often the case). It results from this analysis that the lime found there, although, as Dr. Lunge himself says, an abnormally high amount of "base" was present, does not even suffice to combine the hydrochloric acid, carbonic acid, and sulphuric acid found; that, then, no trace of CaO could have been present. We must suppose that the largest part of the magnesium present in the Weldon mud was combined with oxygen, and that, consequently, the lime was united with the acids present. Since, first, in the lime used the Mg is without doubt present in the form MgO alone, and, secondly, the MgCl_2 dissolved in the manganous chloride solution is precipitated by the lime when added, and only the small amount present in the end solution, in case the charge has not lime enough to complete the precipitation, can as MgCl_2 come in question.

Dr. Lunge lays apparently great weight upon the proportion of MnO_2 to "base." Regarding this, the objection relative to the inexact separation of manganese and calcium would not come into question. A too small amount of lime would correspond to too high a content of manganous oxide. In calculating the amount of manganese from the weight of Mn_3O_4 found, and the calcium from CaO, we multiply by nearly the same fraction (about $\frac{2}{3}$), so that the result does not experience any essential change in this way.

Finally, Dr. Lunge attacks my analyses by saying that "according to his knowledge" specimens with so low proportion of "base" as I have found do not occur in the practice. This appears to me incorrect, because the varying composition of the products of the Weldon process is caused chiefly by the different content of MnO; in other words, the varying degree to which the oxidation is carried in the process. It is clear that the oxidation must go forward most rapidly at first, that it then becomes slower and slower until finally a point is reached where the continuation of the process does not pay, and where it is broken off before probably the theoretical completion of the reaction has been reached. I have heard often from the managers that the final gain of "disposable oxygen" has been increased by the improvements in the process with time. The content of "base" must have decreased then.

When my results are expressed in the same terms as those in which the results of the Weldon method of examination are usually expressed,—that is, when the carbonic acid present is left out of account—the variation of proportion of MnO_2 to "base" from that assumed as the normal by Dr. Lunge is very small. In one of the two analyses of mud, only a little washed with cold water—to which the objection of a change in composition cannot be applied—there was found:—36.51 per cent MnO_2 , 6.72 per

cent MnO, 3.02 per cent Ca, 0.58 per cent Mg, 0.60 per cent Fe_2O_3 , 1.41 per cent CO_2 , 0.59 per cent Cl, 0.14 per cent SO_4 . After deducting the amount of calcium required by the chlorine and the sulphuric acid there remains 2.63 per cent. These require, in order to satisfy the Weldon formula, 11.44 per cent MnO_2 , the manganous oxide requires 16.47 per cent MnO_2 , the magnesium oxide 4.20 per cent MnO_2 , the iron sesquioxide 1.305 per cent MnO_2 . There is required thus a total of 33.42 per cent MnO_2 : 36.51 per cent, or 3.09 per cent more was found. Now, when for 36.51 per cent MnO_2 3.09 too much was found, the proportion for 1 part of MnO_2 is 0.0846 too much MnO_2 ; the proportion of MnO_2 to "base" is here, therefore, 1.0846 to 0.50, or 1 to 0.46.

Dr. Lunge draws conclusions from my results against which I tried to guard myself in advance in each of my publications on this subject, by repeatedly laying stress on the fact that a conclusion as to the question about the existence of a "manganous" acid and its salts could only be reached after the completion of synthetical experiments, with which I am still occupied. It is understood, of course, that with this expressed reservation the proposal of provisional theories is not only admissible but requested. How can a leading line for the further necessary investigation be found and fixed otherwise? The circumstance that only a small amount of CaO is present in the Weldon mud suggests the question, What becomes of the so-called excess of lime which is added? According to information I have received this excess is equal to one-half of the amount necessary for the precipitation (one-half is added immediately after the precipitation, and about one-half of this is carried off in the end solution). This led me to the conjecture, which I afterwards established in reality, that the precipitated Mn(OH)_2 still contains chlorine, which takes up the so-called excess of lime in the process. Still I cannot regard the circumstance as fully explained herewith. The quantity of chlorine present does not appear by any means sufficient to neutralise the excess.

The conclusions which it appears to me can be drawn with certainty from my experiments as far as completed, and which, too, have found only support through the criticism here discussed, can be grasped together in these two propositions:—(1) In the mud, even when there is an unusually large content of the so-called "base," there is too little lime (CaO) present for the admission of the theory that the mud consists essentially of a calcium manganite, $\text{CaO(MnO}_2)_2$. (2) A mud also now occurs in the practical working of the Weldon process, in which the content of "base" lies under 0.5 (where $\text{MnO}_2=1$).

As regards the "thick batches," whose production was discussed at a recent meeting of the Newcastle Chemical Society (CHEMICAL NEWS, vol. xli., p. 179), I will note briefly here in addition my observation (a detailed report will be made soon), that the formation of the "thick batches" can be prevented and a satisfactory result obtained without an excess of CaCl_2 if the milk of lime (the "excess") is added very slowly in very small amounts, according to the weakening of the alkaline reaction in the filtrate. They never proceed in this way in the works to be sure, because an excess of CaCl_2 hinders the formation of the "thick batches" without causing the slightest inconvenience. It appears that they always proceed in this way here on the Continent, and they never experience any "thick batches." An experiment in a large model apparatus (made after the Weldon pattern) made at the ordinary temperature (and without excess of CaCl_2) yielded the "thick batches" likewise.

Göttingen, May 15, 1880.

Alkalies in *Æthusa Cynapium*.—W. Bernhardt.—*Cynapine* was first obtained from this plant by Ficinus; it is a yellowish red oily liquid, with a strongly alkaline reaction and a very penetrating and offensive odour. Qualitative examination showed the presence of nitrogen.—*Arch. Pharm.*

SOME REMARKS ON SIEMENS-MARTIN STEEL.

By SERGIUS KERN, M.E., St. Petersburg.

THE chief steel works in Russia are situated near St. Petersburg. The Obouchoff Steel Works have erected lately two 10-ton Siemens-Martin furnaces. The furnaces have no muffles for heating previously the charges introduced into the furnace, and the mode of working adopted here differs from the Terre-Noire method, as described by Mr. A. L. Holley. The author desires to give a short description of the *modus operandi* and a full account of some charges. The charge consists of steel scrap, ladle scrap, and manganese pig-iron. A pig containing 9 to 12 per cent of manganese is ordinarily used. It is well known that big cast-steel ingots have a piping in their top part; and as such a part is of no use for forgings, therefore a certain part of the ingot, when forged into the required shape, is always cut off under the steam hammer. Such ends are also used for the Siemens-Martin furnace. Very often ends weighing about 2 to 2½ tons are introduced among other steel scrap in the cold state into the furnace. During the process of charging the furnace is kept as hot as possible. All the required materials, viz., pig-iron, steel scrap, and ladle scrap (about 8 tons altogether) are charged at once, and no additional charges are introduced after the metal is in a melted state, save if the testing of a sample out of the furnace shows that the metal is too hard; then, ordinarily, some 15 to 20 cwts. of good puddled iron blooms are introduced into the metallic bath. The blooms are left for some time on the banks of the furnace doors, and when well heated are thrown down into the metallic bath. But it has been found to be cheaper and quicker, in order to soften the metal, to introduce some 2 to 5 cwts. of good magnetic iron ore in the form of a fine powder. The steel tests taken afterwards soon show that the steel has turned mild; but if it happens that even then the steel is not so soft as desired, another charge of 3 to 4 cwts. of magnetic ore with 1 cwt. of ferromanganese quickly brings the steel to the desired softness. Certainly, much depends on the state of the gas and on the quality of the materials, but with a well-going furnace and good materials the above holds good.

Before the casting, if soft steel is wanted, ½ to 1 cwt. of ferromanganese is added, and for harder steel, containing 0.50 to 0.65 per cent of carbon, often 3 to 10 cwts. of spiegeleisen is the additional charge. As there is no handy direct process to ascertain the amount of carbon in the samples taken out of the furnace, the engineer is often in a difficulty in calculating the amount of the last charge of ferromanganese or spiegeleisen to be introduced into the metallic bath. A very good test is the following:—The sound part of a sample is heated to light welding heat, and hammered to the following dimensions (approximate): 5" × 0.5" × ⅛". This small bar cooled slowly in ashes, or sand, must bend nearly double, and if it stands the test tolerably well it may be supposed to contain not more than 0.25 to 0.30 per cent of carbon. The sample after being drawn, by hammering before being cooled, as mentioned above, must be re-heated to a red heat.

The following charges give an idea of the mode of working:—

Hard Steel.—Charge: steel scrap, 120 cwts.; ladle scrap, 17 cwts.; pig-iron, containing 12 per cent of manganese, 17 cwts.; all charged at once; charging commenced 4.30 a.m., finished 6 a.m. Melted, and one sample taken out 8.15 a.m.; 17 cwts. puddled iron blooms charged 9 a.m.; sample hammered well, cooled, bent double; 15 cwts. pig-iron, containing 9 per cent of manganese, charged 10.20 a.m.; casting took place at 11 a.m. Analysis of the steel: carbon, 0.67; manganese, 0.40 per cent.

Medium Steel.—Charge: steel scrap, 135 cwts.; ladle scrap, 23 cwts.; pig-iron, containing 12 per cent of manganese, 10 cwts.; charging commenced 1 p.m., finished 2.45 p.m.; melted, 0.5 cwt. ferromanganese added, and first test taken 7.40 p.m. The steel was hard. 2.5 cwts.

of magnetic iron ore added 7.55 p.m. Test taken 8.30 p.m.; sample bar bent nearly double, giving only a slight crack. 0.5 cwt. of ferromanganese added. Casting 9.15 p.m. Analysis of the steel: carbon, 0.35; manganese, 0.18 per cent.

Soft Steel.—Charge: steel scrap, 113 cwts.; ladle scrap, 26 cwts.; pig-iron, containing 12 per cent of manganese, 6.5 cwts.; charging commenced 11 a.m., finished 12.30 p.m. Melted, and first test 6 p.m.; 2 cwts. ferromanganese added 6.30 p.m.; test bar bent double, after being hardened; ⅓ cwt. ferromanganese added 7 p.m.; casting, 7.15 p.m. Analysis of the steel: carbon, 0.16; manganese, 0.14 per cent. The steel was prepared for boiler plates.

ANALYSES OF SOME HAIR-DYES.

By J. F. BRAGA.

BEING solicited to ascertain for some clients in Portugal the general composition of the various hair-dyes largely sold, and I suppose used, in England, I have from time to time submitted specimens to analysis; the results may be of some interest to your readers.

The dyes may be classified as those for making the hair dark, and those for making it light, though this latter operation consists in bleaching, not dyeing, the hair.

For making the hair light I have found that the material used is peroxide of hydrogen, sold under numberless fanciful names, and usually at extravagantly fanciful prices. It is perfectly harmless to the hair, its action being limited apparently to the oxidation of the colouring-matter. It appears to be without action upon "red" hair, other materials, such as permanganate of potash, dilute nitric acid, and strong ammonia, exercising a more or less bleaching effect, and, though destructive to the hair, have been sold; but they appear to have been completely displaced by the peroxide of hydrogen.

The substances used for making the hair dark, the true dyes, seem, so far as my experiments go, to be limited to solutions of lead, silver, and copper.

Of the lead solutions, that of the hyposulphite is about the best. I found the sample I analysed had been prepared by mixing acetate of lead with an excess of hyposulphite of soda, the solution containing glycerin and a little alcohol. After accurately determining the amount of lead, and approximately the amounts of the other constituents, I successfully imitated the original by the following formula:—

Acetate of lead	5.7 grms.
Hyposulphite of soda	11.5 grms.
Glycerin	50.0 c.c.
Spirits of wine	100.0 c.c.
Distilled water	850.0 c.c.

The salts being separately dissolved, and the glycerin and alcohol mixed with the hyposulphite, the solution of lead was then gradually poured into the mixture. The resulting compound should be kept in the dark.

Another consisted simply of a solution of oxide of lead and glycerin, with precipitated sulphur in suspension; its composition being:—

Oxide of lead	17.0 grms.
Glycerin	300.0 grms.
Water	to 1 litre
Precipitated sulphur	17.0 grms. in 1 litre

A third closely resembled this, being composed as follows:—

Acetate of lead	12.5 grms.
Glycerin	125.0 grms.
Distilled water	to 1 litre
Precipitated sulphur	10.0 grms.

This was stated to be unsatisfactory in its action. The last which I shall mention of the lead dyes consisted

of a very dilute solution of lead in caustic potash. I was enabled to imitate it after analysis by dissolving 1.25 grms. of acetate of lead in hot water, and adding thereto a hot freshly-prepared solution of caustic potash, until the precipitate at first formed *just* re-dissolved, and then diluting that solution up to 1 litre.

The perfume of these liquids is due to the employment of scented waters, prepared either by distillation, or after an alternative method, such as that given in the "Pharmacopœia."

NOTE ON THE ACTUAL STATE OF THE DETERMINATION OF ZINC.

By W. ALEXANDROWICZ.

THE separation of zinc from the metals of the copper and the iron group presents considerable difficulties. The quantity of zinc thrown down along with copper by sulphuretted hydrogen is not appreciable if the solution is sufficiently acid. Where great exactitude is required a double precipitation is recommended.

The complete separation of cadmium and zinc by means of sulphuretted hydrogen is impossible, especially in presence of copper. In separating arsenic from zinc, if the solution is distinctly acid the zinc is not thrown down by sulphuretted hydrogen.

In separating iron and zinc the author recommends that the solution of the mixed metals should be poured drop by drop into the ammonia, and not *vice versa*. The zinc remains in solution. The precipitate is then washed with ammoniacal water.

In separating manganese from zinc the author acidifies with acetic acid and precipitates with sulphuretted hydrogen. All the manganese remains in solution.—*Revue Universelle des Mines*.

THE BEHAVIOUR OF SULPHURETTED HYDROGEN WITH THE SALTS OF THE HEAVY METALS.*

By H. DELFFS.

THE author called attention to the different precipitability of the metallic salts in presence of a strong mineral acid on the one hand, and of acetic acid on the other. Just as the limit between precipitable and non-precipitable salts is altered by the use of acetic acid instead of hydrochloric acid, it is further modified if formiates are treated with sulphuretted hydrogen. In that case the salt of zinc is precipitated, but the compounds of cobalt, nickel, iron, and manganese are not affected. Manganese cannot be precipitated by sulphuretted hydrogen from propionic, butyric, and valerianic solutions. In precipitations by this last reagent several metals are never simultaneously converted into the corresponding sulphides, but the precipitation ensues in such a manner that one metal is first completely separated before the removal of another begins. Upon this fact is founded a very convenient method for obtaining cobalt and nickel in a state of purity. As sulphuretted hydrogen first completely precipitates cobalt acetate, and then acts upon nickel acetate, a solution of the two nitrates is mixed with sodium acetate in quantity insufficient for complete double decomposition, and sulphuretted hydrogen is introduced, so that, according to the respective proportions of the two metals, either nickel free from cobalt is obtained in solution or cobalt free from nickel as a precipitate. The required quantity of sodium acetate may be calculated from the relative quantities of

the two metals which are mostly known. The more electro-positive a metal the later it is precipitated from a mixture of its salts with those of other metals, and thus a series of "chemical tension" may be established, which, on account of the electromotoric action of the liquids, does not always agree with the series of "physical tension."

ON AN AUTOMATIC MERCURIAL PUMP.

By M. G. COUTTOLENC.

THIS apparatus is a mercurial pump, acting in the usual manner, but without cocks; it can also work automatically and indefinitely with the same quantity of mercury. This pump is only intended to complete the vacuum, and works under a pressure of at most 40 to 60 m.m. of mercury. As will be seen a water pump serves to begin the vacuum, provided it is connected to the apparatus by a desiccating tube. The following is the description of the pump:—A reservoir, capable of a lateral movement of about 0.30 m. is connected with a vertical tube of 0.80 m. At the upper part of the glass tube there is seen first a lateral junction fitted with a valve of a very simple construction, the utility of which will appear further. Immediately above this junction there is cemented a tube of a smaller diameter (0.25 m.) plunging into the former. To this second tube is blown a reservoir pump-body of a suitable form, and at the lower part of this is a tube of 0.002 m. in diameter, which forms an elbow and rises perpendicularly, extending some centimetres past the reservoir, where it has an enlargement, and leads at last to the receiver to be exhausted. The upper part of the reservoir pump-body ends in a very fine glass tube ($\frac{1}{4}$ to $\frac{3}{8}$ millimetre in internal diameter), which re-descends parallel to the reservoir after a double elbow, and plunges into a small vessel fixed close by. The end of this tube and the vessel are inserted and cemented into a glass reservoir communicating on the one hand with the water-pump, and on the other with the valve mentioned at first.

When thus fitted up the apparatus works as follows:—The gas of the instrument being once rarified by the water-pump, the mercury rises in the vertical tube sufficiently to isolate the two concentric tubes. If, at this moment, the movable reservoir is raised up, the mercury ascends first into the lateral junction, where it is soon stopped by the valve, and at the same time into the interior tube; passing before the narrow tube leading to the receiver to be exhausted the mercury closes it and fills the reservoir pump-body, driving out the residual gas by the upper capillary tube. Here it chokes the pipe, returns into the small vessel, and the excess falls on the side of the second reservoir above the valve. If, in the meantime, the movable reservoir re-descends, the mercury will leave in the fine tube a column sufficient to close it, and then retiring from the reservoir pump-body it produces there a barometric vacuum, which is placed in connection with the receiver to be exhausted, when the mercury unstops the tube leading to it. But at the same time when this effect is produced the mercury, which had fallen into the other reservoir above the valve, returns into the outer tube. If the movable reservoir is then raised again, the same effects are re-produced, and each time the receiver to be exhausted is brought in connection with the barometric vacuum. The pump works, therefore, by means of a simple up and down movement, which is easily obtained from any source of power.—*Comptes Rendus*.

Melting-points of Minerals and Metals.—A. Schertel. —The determinations were made with the aid of Prinsep's alloys. The melting-point of nickel was found to be 1392° to 1420°.—*Berg. u. Hütten Zeitung*.

* A Paper read before the Chemical Section of the Congress of German Naturalists and Physicians, at Baden-Baden, Sept. 19, 1879.

ON THE ELECTROLYTIC DETERMINATION
OF METALS.

By LUDWIG SCHICHT.

THE author's researches refer to the metals of the group uranium, thallium, indium, and vanadium, and to the group molybdenum, selenium, and tellurium. Uranium is not precipitated from its solutions in mineral acids, but by the secondary action the effect of the nascent hydrogen uranic oxide is reduced to uranous oxide. From neutral solutions it is separated in very small quantities, of a yellow colour. Alkaline solutions containing organic acids (tartaric, citric, acetic), or mixed with sugar, deposit likewise very small quantities of uranium. The precipitated uranium does not readily re-dissolve in dilute acids. Sulphuric and nitric solutions of thallium were prepared from the pure metal. The acidulated solutions were not precipitated by the current. From ammoniacal solutions metallic thallium was deposited at the cathode with a brisk disengagement of gas, whilst at the positive pole there appeared blackish brown thallium oxide, much resembling lead peroxide. For the decomposition, the author used at first the current from 4 Meidinger Pincus elements, giving hourly 160 c.c. of detonating gas, but this was found too strong, as the thallium was deposited in a spongy state and of a darker colour. On using 2 or 3 elements fine, permanently adhesive metal was obtained. From neutral solutions the metal is imperfectly precipitated on account of the acid which is liberated. In alkaline solutions the separation is complete and the metal is bright and solid. It re-dissolves readily in sulphuric acid. The oxide dissolves in hydrochloric acid, evolving chlorine. Indium is completely precipitated as metal at the negative pole, both from acid and alkaline solutions; in the latter case the metal is very bright and firm.

Vanadium.—Vanadium chloride was dissolved in water containing hydrochloric acid and electrolysed. No precipitation took place in the blue solution, the vanadic acid being merely reduced to oxide. Sulphurous acid, organic acids, &c., reduce merely to vanadium tetroxide, V_2O_4 . The same reduction occurs in the alkaline solution.

Palladium.—The nitrate, dissolved in water and acidified with a little nitric acid, deposited at the negative pole a bronze-coloured coating, which on continued action of the current became darker and finally black. It re-dissolves easily in nitric acid. Some reddish oxide was formed at the positive pole. Alkaline solutions of palladium behave in a similar manner, but the deposition is slower and more adhesive.

Molybdenum is precipitated from the ammoniacal solution of molybdic acid as molybdous oxide, which appears at the negative pole at first in coloured rings, which gradually thicken and become blue-black. The first blue precipitation is molybdic molybdate; then follows molybdic oxide and molybdous oxide. The precipitation is complete and adheres very firmly. In acid solutions there is no precipitation; in ammonium molybdate acidified with free molybdic acid the precipitation is imperfect.

Selenium is readily and completely reduced and thrown down, both from acid and alkaline solutions. The current should not be strong (2 elements) or the deposit is pulverulent. In order to determine selenium electrolytically it is oxidised by boiling with nitric acid, and the solution of a metal is added, which occasions the separation of the selenium in a solid combination. A solution of copper is suitable for this purpose.

Tellurium behaves like selenium, but it is reduced much more readily. From an acid solution it is easily deposited with a blue-black colour. From alkaline solutions it is thrown down in a very loose state at the positive pole, with strong disengagement of gas. If much metal is present it floats on the surface of the liquid.

Gallium, like zinc, is thrown down completely at the negative pole in a pure state.—*Berg. und Hüttenmännische Zeitung*, 39, p. 121.

DETECTION AND DETERMINATION OF
CHLORINE
IN PRESENCE OF BROMINE AND IODINE

By G. VORTMANN.

THE author has discovered a method by means of which even small quantities of chlorine along with the other halogens can be easily and quickly detected. It depends on the different behaviour of the chlorides, bromides, and iodides with peroxides of manganese and lead in presence of acetic acid.

Iodides are partially decomposed by the above-mentioned peroxides, even in neutral solutions, and if they are boiled with the addition of acetic acid the iodine is completely eliminated. Lead peroxide oxidises a part of the iodine to iodic acid, but with manganese peroxide no iodic acid is formed.

In a neutral solution bromides are not decomposed either by manganese- or lead-peroxide. In an acetic solution the lead peroxide only acts, bromine escapes, but bromic acid is formed only if bromides are present in considerable quantities. Manganese peroxide has no action in the acetic solution, even on prolonged heating.

Chlorides are not attacked by either of the peroxides in presence of acetic acid. In testing for chlorides in presence of bromides or iodides it is sufficient to boil the substance in an acetic solution with lead peroxide till the liquid on settling is colourless, and has not the slightest odour of bromine or iodine. The bromine and a part of the iodine escape as such; the remainder of the iodine remains as lead iodate along with the excess of the lead peroxide. On filtering and washing the precipitate, all the chlorine is found in the filtrate free from bromine and iodine. In this manner the chlorine may be determined quantitatively. If the quantity of chlorine accompanying the iodine is considerable, manganese peroxide is preferable to lead peroxide, as otherwise the liquid must be largely diluted with water to prevent lead chloride from depositing. In determining large quantities of chlorine in presence of bromine, it is well to add along with the lead peroxide some potassium sulphate so that all the chlorine may be found in the filtrate combined with potassium.

In order to expel the liberated bromine and iodine more rapidly a moderate current of air may be passed through the solution on the water-bath.—*Berichte der Deutschen Chem. Gesellschaft*.

NOTICES OF BOOKS.

Potable Water: How to form a Judgment on the Suitableness of Water for Drinking Purposes. Addressed to Medical Officers of Health and Sanitary Authorities. By CHARLES EKIN, F.C.S. London: J. and A. Churchill.

WE have here a small but sensible book on a subject which, though it has often been enquired into, is not yet exhausted. The author points out that when sitting in judgment upon water we have to deal not so much with facts as with the conclusions to be drawn from those facts, and that the question of the probable physiological action of a water is rather one of medicine than of chemistry. How is it, we may here ask, that the medical profession had no representative in the second Rivers' Pollution Commission?

On the suitableness of rain-water for domestic uses we agree with Mr. Ekin rather than with the Commissioners. We have known persons who have used it for years and who have been totally free from zymotic disease. The impurities in the upper regions of the air where rain condenses are doubtless slighter than in the strata nearer the ground. In prolonged rains the atmosphere is washed by the first downfall, and the subsequent supply will be practically pure.

Certain inconsistencies of the Rivers' Pollution Commissioners are skilfully touched upon. We all know to what an extent these authorities are enamoured of sewage irrigation and "intermittent downward filtration." In a passage quoted by Mr. Ekin they declare, speaking of certain water near a churchyard, that "the exhaustive filtration to which it had been subjected in passing downwards through so great a thickness of material, and the rapid oxidation of the dissolved organic matter in a porous and aerated medium, afford considerable guarantee that all noxious constituents had been removed." Yet elsewhere they say that "though the purification of water polluted by human excrements may reasonably be considered to be some safeguard against the propagation of epidemic diseases *there is not in the form of actual experience a tittle of trustworthy evidence* to support such a view." They also quote the case of the epidemic of typhoid at Laussen to show that filtration through a mile of earth and rock does not keep back disease germs.

The real question, as Mr. Ekin very judiciously points out, is not so much the quantity as the quality of the organic matter present in water. Carbon, nitrogen, &c., may be derived entirely from innocent matter, or wholly or partially from some very dangerous source, and no organic analysis will *per se* solve the difficulty. The author differs from Professor Wanklyn in considering that the presence of nitrogen as nitrates in quantities exceeding 0.3 per 100,000 should be regarded as an unfavourable symptom, even if little organic matter is present, and that if the proportion reaches 0.9 to 1.1 it warrants condemnation. This view, Mr. Ekin remarks, was forced upon him by experience in spite of a bias in a contrary direction. Of chlorine he is also strongly suspicious, excepting in coast localities, and of course in districts where salt-springs occur. It will be remembered that according to Dr. Tidy water for drinking purposes should not require more oxygen to burn up its organic matter than from 0.05 to 0.15 parts per 100,000, whilst if the amount rises to 0.21 part the water is doubtful, and if higher it is to be condemned. On this view Mr. Ekin observes that "in the case of an infected water six typhoid germs are harmless, whilst nine would be hurtful."

The microscopic examination of water and the determination of phosphates the author has purposely avoided, considering that "in the present state of our knowledge they afford only additional evidence where the evidence is already complete." We can scarcely agree with this view: the microscope is surely calculated to inform us as to the nature and origin of the organic matter present, and phosphoric acid is surely a safer guide than chlorine, which occurs more frequently in nature in a soluble state without being traceable to animal pollution.

On the Properties, Preparation, and the Uses of Methyl Chloride. Lille: Ducoulombier.

METHYL chloride, or the hydrochloric ether of methylic alcohol is represented by the formula $C_2H_3Cl=50.5$. It is gaseous at common temperatures, possesses an ethereal odour and a saccharine taste; its specific gravity is 1.738 (air=1.0 at 0°). The weight of a litre of this gas=2.261 grms. Water dissolves 2.8 vols. methyl chloride at 16° and under a barometric pressure of 0.765. Glacial acetic acid dissolves 40, and absolute alcohol 35 vols. under the same conditions. Methyl chloride burns with a white flame edged with green, forming water, carbonic acid, and torrents of hydrochloric acid gas. If methyl chloride is compressed, it is easily converted into a colourless and highly mobile liquid boiling about -23° under the normal pressure of 0.760 m. The slight vapour tension of this product renders its liquefaction, its management, and its carriage easy. Thus the total tension of its vapour is at—

0°	2.48 atmospheres.
+15°	4.11 ..
20°	4.81 ..
25°	5.62 ..
30°	6.50 ..
35°	7.50 ..

From these figures, one atmosphere must be deducted to find the pressure really exercised upon the vessels containing the liquid.

Hitherto chloride of methyl, as prepared by the mutual reaction of common salt, sulphuric acid, and methylic alcohol, has not been suitable for industrial applications on account of the difficulty of preparing it in a state of purity at a moderate price. Quite recently M. Camille Vincent has made known a process which enables it to be procured in abundance and free from impurities.

This process consists in heating the hydrochlorate of tri-methylamine which is obtained industrially from the refuse of beet-root. The hydrochlorate of tri-methylamine is thus decomposed into free trimethylamine, ammonia, and methyl chloride. Washing in acid water removes every trace of alkali, and the dried gas may then be liquefied by compression. The product thus obtained, perfectly pure, is manufactured on the large scale by MM. Brignonnet and Son, at Saint Denis.

Uses of Methyl Chloride.

As a frigorific agent, methyl chloride may be employed either in the laboratory or in manufactures. If it is allowed to issue into an open vessel, it enters into a brisk ebullition for a few moments. Its surface then becomes tranquil, forming a bath at -23°, into which the objects to be refrigerated may be plunged. If the evaporation is intensified by the injection of dry air, the temperature of the bath may be reduced to -55° in a few minutes, so as to freeze mercury. M. Vincent has designed a small apparatus for the laboratory in which a bath of 1 litre of an incongelable liquid (*e.g.*, alcohol) may be kept for some hours at a temperature of -23°, or of 50—55°. It is composed of a cylindrical vessel of copper with double walls.

M. Vincent has also constructed a more complete apparatus, suitable either for experimental or domestic purposes, and so arranged that the methyl chloride may be recovered. Large machines are also made adapted for cooling water, air, &c., on the great scale. They have the advantage of employing, as vehicle of heat, a neutral liquid incapable of attacking metal work and free from poisonous or offensive unexplosive properties.

It must be remembered that the chloride of methyl can be used in any machine for the production of ice with the exception of those constructed to work with liquid ammonia.

Preparation of Methylic Products.

Methyl chloride is a product which offers great advantages to the manufacturers of colouring-matters derived from coal-tar. It may serve in the manufacture of coloured or colourable products in which methyl plays any part soever, such as methylaniline, methyldiphenylamine, methyl-toluidin (which yields a red-violet, Hofmann's violet, methyl-green, methylic eosin, &c.).

It is an advantageous substitute for the other methylic compounds commonly employed, such as methyl bromide, iodide, and nitrate, the two former of which are costly and the latter dangerously explosive. Liquefied methyl chloride being a pure and anhydrous compound is well adapted for exact operations which lead to perfect regularity in the quality of the product.

The methylated compounds which have been already prepared with liquid methyl chloride are, dimethylaniline, methyldiphenylamine, Hofmann's violet, and methyl-green. We will briefly describe the preparation of the two latter according to the process of MM. Monnet and Reverdin, of Laplaine, near Geneva.

To obtain green with methyl chloride, we place in an autoclave a solution of methylaniline violet in methylic alcohol rendered basic by the addition of soda. The apparatus is closed and a known quantity of methyl chloride is introduced by means of the manipulation already described. The autoclave is placed in a water-bath heated by a jet of steam and the temperature is raised to 4 or 5 atmospheres. When the reaction is complete, the hot water is let out and its place is taken by a stream of cold

water, the excess of pressure being destroyed by slackening the screw of the cock.

The produce of the reaction is heated with an alkali and filtered to separate the base of the violet: the filtrate containing the base of the green is treated with an acid and a salt of zinc to form the corresponding double salt of the green and of zinc, and the colouring matter is then precipitated by common salt. By substituting in the preceding operation rosaniline for methylaniline violet we obtain Hofmann's violet.

A Course of Qualitative Chemical Analysis. By the late W. G. VALENTIN, F.C.S. Revised and Edited by W. R. HODGKINSON, Ph.D., &c. Fifth Edition. London: J. and A. Churchill.

MR. VALENTIN'S character as a meritorious and successful teacher of chemistry was widely and justly appreciated, and the work before us in its various editions has been prized by several generations of students. But the question may, we submit, be fairly raised whether a treatise on analytical chemistry is the proper place for a consideration of nomenclature and notation, and whether the formulæ which occur here in such profusion are necessary, or even beneficial? What bearing, for instance, has the graphic formula of magnetic pyrites, otherwise pentaferrous diferric octasulphide, on the recognition of iron in its combinations? It seems to us that this matter must in a manner mask the really essential portions of the work, whilst they greatly increase its bulk and add to its cost. The reactions of the rarer metals are given as an appendix—a judicious arrangement, since it prevents the beginner from being overwhelmed in the outset with a multitude of elements, and enables him to proceed from the easier to the more complex and difficult parts of the subject.

Price List of Dr. Theodor Schuchardt's Chemical Works, Goerlitz. Summer Half-year, 1880.

A most extensive and complete catalogue of chemicals, including many of the rarest kinds. The prices are given both in German and in French money. It is a curious feature that, with few exceptions, the various compounds are made to figure under Latin names, by which they are known merely to pharmacutists. In addition, the catalogue includes chemical apparatus and collections of specimens for a variety of purposes. Among these may be mentioned sets of phosphorescent bodies; a collection of preparations for spectroscopic examination; a collection of alkaloids; physiological preparations, both animal and vegetable; a set of 68 coal-tar colours and intermediate products, accompanied by specimens of dyed tissues; collections illustrative of crystallography; a set of 40 minerals from Vesuvius, &c.

On Shoyu. By T. ISONO. Tokio: The Department of Science.

"SHOYU" is a liquid formed by the fermentation of a mixture of beans and wheat in a saline solution, and appears to play a very important part in Japanese cookery. It contains glucose, dextrin, small proportions of alcohol and acetic acid, and 1.43 per cent of nitrogen in organic combination.

The treatise contains an account of the salt works of Japan, which at present use sea-water as their raw material.

Non-poisonous Colours.—The following colours are officially sanctioned by the Public Health Department for use in confectionery, &c.:—For white, flour and starch; for red, cochineal, carmine, madder-red, and the juice of carrots and cherries; for yellow, saffron, safflower, and turmeric; for blue, indigo and litmus; green, juice of spinach and mixtures of the above blues and yellows; for brown, burnt sugar and Spanish juice, and for black, Indian ink.—*Reimann's Farber Zeitung.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 19, May 10, 1880.

Crystalline Form of Magnesium.—M. des Cloizeaux.—The normal form of these crystals is a regular hexagonal prism terminated by a base rather less brilliant than the lateral faces. Among the rhombohedric metals magnesium next to zinc affords the most acute primitive form. The crystals are very malleable and sectile, and no cleavage was observed.

Experimental Researches on the Decomposition of Certain Explosives: Analysis of the Products.—MM. Sarrau and Vieille.—Under a pressure differing little from that of the atmosphere all the explosives experimented upon gave different results from those obtained under strong pressure (see *Comptes Rendus* for May, 1880). All yielded nitric oxide and carbon monoxide. Pure gun-cotton and nitro-glycerin evolved C_2H_4 .

Theory of the Phenomena of Interference where Rotatory Polarisation Intervenes.—M. Gouy.—A mathematical paper, not admitting of useful abstraction.

Equipotential Lines of a Plane formed of Two Halves of Unequal Conductibility.—A. Guébbard.—This memoir requires the accompanying diagram.

Mutual Action of Magnetic Needles Plunged into Liquids.—M. Obalski.—The needles approach each other by their submerged portion. If the level of the liquid is raised the mutual approach becomes more distinct, and when the third or the fourth of the needles is submerged they rush into contact.

No. 20, May 17, 1880.

On Saccharin.—E. Peligot.—Saccharin is dextro rotatory, like common sugar, with which it agrees in percentage-composition. Its rotatory power is represented by 93.5° ; that of sugar with the same apparatus and under the same conditions being 67.18° . The essential characteristic of saccharin is its relative stability and inertness as regards the reagents which act in a special manner on the group of sugars. It does not ferment; it is volatile almost without decomposition; it does not reduce that alkaline copper tartrate solution, and it is not transformed into fermentible sugar by prolonged ebullition with sulphuric acid. It is not attacked by a hot and concentrated solution of caustic potassa.

Researches on the Proportion of Carbonic Acid in the Air.—J. Reiset.—The apparatus used by the author cannot be described without the accompanying cuts; it is a modification of that used by Schloësing for the determination of atmospheric ammonia. He finds in 100,000 parts of atmospheric air, 29.78 parts by volume, the greatest differences observed only reaching hundred thousandths. The air in the night contained more carbonic acid than in the day. The maxima observed correspond to foggy weather. He has not detected any decrease in the proportion of carbonic acid as a weather indication.

The Tensions of Saturated Vapours have Different Methods of Variation according as they are given off above or below the Point of Fusion.—M. Paul de Mondesir.—The author's observations prove, in his opinion, that the passage of the fusion-point involves a very marked change in the mode of variation of the tensions. Whether this change results really from the physical phenomenon or from the presence of foreign bodies, or from isomeric changes, the conclusion remains that vapours given off by a liquid must be distinguished from vapours given off by a solid.

Refrigerating Mixtures formed by an Acid and a Hydrated Salt.—A. Ditte.—In this case the refrigeration is not due to a simple solution of the salt; there is always a double decomposition in conformity with the law of maximum work. The fall of temperature observed springs from the circumstance that the salts employed contain a large quantity of water, which takes no part in the reaction, but is separated from the solid hydrated salt, of which it formed a part, and the result is as if it became liquid. This change of state absorbs the heat which the reaction liberates, and borrows from the liquid itself the surplus of *vis viva* necessary for its completion. Hence results a considerable reduction of temperature.

Variations of Urea in Cases of Phosphoric Poisoning.—M. Thibaut.—At the outset of the experiments the daily secretion of urea was 15.66 grms.; it fell afterwards to 5.77 grms., rose again to 11.59, and sank finally to 0.20 gm. As the urea diminished in the urine it increased in the blood. The proportion of urea in the liver was also increased. The liver seems the principal focus for the production of urea.

Influence of Fattening Animals upon the Constitution of the Fats formed in their Tissues.—A. Muntz. As animals are fattened, their fats become more fusible and contain a smaller proportion of solids. Hence they possess a lower industrial value.

Fixity of the Composition of Plants: Analysis of Soya hispida.—H. Pellet.—In specimens of soya grown respectively in China, Hungary, and France, the proportions of phosphoric acid and potash, which together constitute three-fourths of the ash, fluctuate very little. There is rather more variation in the soda, lime, and magnesia.

Chemiker Zeitung.
No. 13, 1880.

Azo and Naphthol Colouring-matters.—G. Auerbach.—The first part of a memoir, in which the author describes the preparation of this class of colours, but shows that few of them have become of real commercial importance.

Die Chemische Industrie.
No. 2, February, 1880.

Formation of Graphite by the Decomposition of Cyanogen Compounds.—W. Thalheim.—The author maintains that the graphite which appears during the preparation of caustic soda from the mother-liquors of the Leblanc process is due, not, as supposed by Pauli, to the destruction of cyanogen compounds, but is extracted out of the cast-iron pans. If the lye is concentrated and the sulphur compounds oxidised in a silver capsule no graphite is deposited.

Apparatus for Testing Petroleum.—C. Engler.—This paper cannot be reproduced without the accompanying illustrations.

The Production of Glycerin.—The total annual production of glycerin in Europe is 9 million kilos., 4 millions of which fall to the share of France. England yields merely 300,000 kilos.

Valuation of Reverted Phosphoric Acid in Manual Phosphates.—At the congress of directors of agricultural experimental stations, held in September last in Carlsruhe, Prof. Petermann gave a detailed account of the reasons which induce him and his Belgian colleagues to value phosphates soluble in ammonium citrate as highly as those soluble in water. He explains that the phosphates of commerce which have been treated with sulphuric acid may contain phosphoric acid, either as free hydrated acid, as mono-calcium phosphate, as dicalcium phosphate, as neutral iron phosphate, as neutral aluminium phosphate, as tricalcium phosphate which has escaped the action of the sulphuric acid, and lastly as tricalcium phos-

phate formed by reversion. The two first mentioned are soluble in water, the neutral phosphates in ammonium citrate, but the last two states in neither. The equal value of the phosphates respectively soluble in water and in ammonium citrate has been proved experimentally. The author prepares ammonium citrate for this purpose by dissolving citric acid in ammonia to neutrality, bringing the solution to sp. gr. 1.09, and adding per litre 1.50 c.c. ammonia.

No. 3, March, 1880.

A local association for promoting the interests of the chemical arts has been established in Berlin.

The Position of the German Match Trade.—A protectionist paper.

Annual Report on Alcohols and Organic Acids.—An account of the sources and the consumption of these preparations in the chemical arts.

Retrospect of the Progress of the Coal-tar Industry for the last Ten Years.—Dr. R. Nietzki.—Rosaniline has lost much of its importance, and of its former derivatives only one aniline-blue (triphenyl-rosanilin) is now manufactured, Hofmann's violet aldehyd and iodine green having been respectively abandoned in favour of methyl-violet and methyl-green. An important step has been the conversion of rosaniline and methyl-violet into sulphoacids, in consequence of which they gain in permanence, and can now be used along with acid. Malachite green, and especially benzoyl green, are proving formidable rivals even to methyl green. Victoria orange (binitro-cresol) and aurantia (hexan-nitro-diphenylamin) are rarely if at all employed, the azo colours having taken their place. The phthaleines (erythrosin, phloxin, cyanosin, &c.) are no less distinguished for their beauty than for their want of permanence.

Valuation of Reverted Phosphates (Conclusion).—M. Fleischer.—After a prolonged discussion it was resolved that the congress recognises the value of reverted phosphoric acid for certain soils, and does not object to its determination when desired by consumers or producers, but considers the evidence as to its valuation insufficient without further experiments.

Process for the Quantitative Determination of Stearin and Olein in Tallow.—M. Dallican.—A portion of the tallow is decomposed and the fatty acid obtained is examined for its congealing- and melting-points, which are then compared with those of mixtures of stearic and oleic acids in different proportions, as given in an accompanying table.

L'Orosi, Giornale di Chimica, Farmacia, e Scienze Affini.
No. 1, January, 1880.

Reciprocal Relations between the Atomic Weights of the Elements and their Properties.—Dr. Dario Giberitini.—This memoir, which is not concluded in the present issue, criticises the hypothesis of Prout and the speculations of L. Gmelin and Dumas on the progressions traceable in the atomic weights of elements belonging to certain groups.

The Mean Composition of the Saliferous Waters and of the Various Products of the Salt Works of St. Leopold, near Volterra, and the Theory of the Extraction of Salt there Practised.—A. Brasseur.—The rock-salt and the waters contain mere traces of iodides and bromides.

Santonin Contaminated with Strychnin.—Torquato Gigli.—Strychnin is stated to be a common impurity in commercial samples of santonin. For its detection the author recommends in preference to Flückiger's method, the following process:—1 gm. of the sample is placed in a small beaker, covered with a little distilled water, and acidulated with a few drops of sulphuric acid. On agitating with a glass rod the strychnin dissolves, while the santonin remains insoluble. The liquid is filtered, the

residue washed with a little water, and the washings added to the filtrate, which is then distributed in several test-glasses, and examined for strychnin by means of the usual reagents.

Reply to Prof. Ugo Schiff's Memoir on Certain Observations made in a former Paper of the Author's entitled "Researches on the Constitutional Formulæ of the Ferric Compounds."—Dr. D. Tommasi.—The author points out that he has not called in question the existence of the oxychlorides and basic nitrates of iron upon merely theoretical grounds, but on the faith of facts universally known and admitted.

Chemisches Central-blatt.
No. 12, March 24, 1880.

Nearly all the papers in this number relate to apparatus, and cannot be intelligibly reproduced without the accompanying engravings.

No. 13, March 31, 1880.

Action of Benzoyl-chloride and Benzo-trichloride upon Phenols.—O. Döbner.—Benzo-trichloride acts upon phenols very differently from benzoyl-chloride. Without the presence of a metallic chloride it acts upon the nucleus of the phenols, 1 mol. combining with 2 mols. of the latter, whilst 3 mols. hydrochloric acid are eliminated. The products formed are all yellow or red, and are closely connected with the phthaleines and rosolic acid. The resorcin compound in solution displays a beautiful fluorescence, and yields with bromine a body much resembling eosin.—*Tageblatt des Natur. Vereins zu Baden-Baden*, 1879, p. 192.

Presence of Vanillin in Crude Sugars.—E. O. v. Lippmann.—The author has separated vanillin from certain kinds of raw beet-sugars.

No. 14, April 7, 1880.

Constitution of Liquids.—G. Cantoni.—The author maintains that at a given temperature every liquid contains in its intramolecular spaces vapours of its substance of the highest tension corresponding with such a temperature.

On Ultramarine.—H. Heumann.—A theoretical examination of the formation of ultramarine.—*Tageblatt des Natur. Ver. zu Baden-Baden*, 1879.

On Pilocarpine.—Alex. Pöhl.—An account of the optical and especially the spectroscopic behaviour of this base.—*Tageblatt des Naturf. Ver. zu Baden-Baden*, 1879.

Solubility of the Metals in Petroleum.—C. Engler.—The author, referring to the experiments of Dr. Stevenson Macadam, maintains that lead, zinc, tin, copper, magnesium, and sodium are only attacked by petroleum under the joint influence of air or oxygen, when acid compounds are formed, which the author is at present examining. Petroleum washed with caustic alkalies and distilled in carbonic acid has no solvent action on metals. The oxidising effect of oil of turpentine upon lead, does not, according to the author, depend upon the formation of ozone.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 13, March 25, 1880.

Megascope and Magic Lantern.—M. Lefevre.—The convertible instrument here described and figured is admittedly a toy.

Meteorological Optics.—M. Montigny.—After a prolonged observation of the scintillation of the stars, in which he has made use of a special instrument, the scintillometer, the author concludes that he has obtained a clue to the prevision of the weather. Very vivid scintillation indicates agitation or a fall of temperature in the upper regions of the air, and consequently the approach of bad

weather. For the purpose of studying the supposed periodic variation in the colour of certain stars he has contrived a means of registering the colour of stellar light so as to obtain a series of comparable observations. It is also pointed out that the height of the barometer is influenced by the speed of the wind, and that altitudes taken in stormy weather may be erroneous to the extent of one-third.

Process of Sewage Purification.—M. Vivien de Saint Quentin.—A solution of sulphates and chlorides of iron and aluminium as obtained, e.g., from the mother-liquors of the alum and copperas works is added to the sewage, and is then followed up by milk of lime. [Where is the novelty?]

No. 14, April 1, 1880.

Considerations on the Experimental Possibility of the Dissociation of Certain Non-Metallic Elements (Continuation).—Raoul Pidet.—The author concludes that in our laboratories we are limited both as regards the production of high temperatures and the quantity of heat available. In the analytical investigation of simple bodies we are stopped by the material impossibility of dissociations.

No. 15, April 8, 1880.

Antisepticity of Copper.—Dr. Burq.—The author considers it as proved that copper is a prophylactic against cholera and typhoid fever.

No. 16, April 15, 1880.

Nascent Hydrogen.—Dr. Donato Tommasi.—A continuation of the author's discussion with Dr. Phipson. He maintains that nascent hydrogen is calorified hydrogen, and that all the reductions produced with nascent hydrogen may be obtained as well with ordinary hydrogen at an elevated temperature.

Report on the Scientific Researches of the Members of the Learned Societies in the Departments Published in the Year 1879.—M. Emile Blanchard.—A most valuable collection of scientific investigations, many of which might otherwise escape notice.

Reimann's Färber Zeitung,
No. 8, 1880.

Copper in Silk.—Francezon has detected in raw silk a trace of copper, which he considers to be an original constituent of the fibre. It may probably have been derived from reeling the cocoons in copper vessels.

Nos. 9, 10, and 11, 1880.

These numbers contain nothing of general interest.

No. 12, 1880.

This number gives an account of the rise and progress of calico printing at Iwanowo Wosnessensk, in Russia, where there are at present 31 print-works in operation.

No. 13, 1880.

This number is almost entirely taken up with the correspondence between the Dyer's Association of Berlin and a representative of the yarn merchants.

NOTES AND QUERIES.

Stearate of Soda.—Would any of the readers of the *CHEMICAL NEWS* kindly oblige me with some information as to the manufacture of stearate of soda, and where it can be obtained in large or small quantities?—IGNORAMUS.

Analysis of Cinchona Bark.—Your correspondent, in his article on "Analysis of Cinchona Bark," in the *CHEMICAL NEWS*, vol. xl., p. 209, in reply to former query of mine, stated very clearly his method of determining the different alkaloids, with the exception of the cinchonidine. If the same correspondent would state his method of estimating the percentage of this alkaloid, and also give the rule for valuing the bark when the analysis and price of sulphate of quinine are known, it would much oblige.—L.

THE CHEMICAL NEWS.

VOL. XLI. No. 1074.

A NEW PATENT BILL.

LONG and elaborate patent laws have been passed, and others discussed, but each one seems to have been on the principle that inventors as a body are criminals to be fined, and they are fined accordingly. They seek protection for the property of their creation, which they think is more their own than any land can be, but they are told that the Government only gives them a right to protect it themselves by legal means, and they are ruined in great numbers in the process. We know of a certainty that there are men who refuse to tell their ideas to the public because they will obtain nothing in return, except some reply such as that they are not the true inventors, and proof is expensive. There are even men who believe that no man ought to have any patent, because it is supposed to be better that the public should obtain all inventions free. It is said that ideas cannot be property. We say that they are the truest of all property; the only property which a man can take with him at last, and property that needs protection quite as much as a house.

A bill has been brought into Parliament by Mr. Anderson, Mr. Alexander Brown, Mr. Hinde Palmer, and Mr. Broadhurst, which has the advantage of being short. The bill puts the whole office of patents under three commissioners, who shall devote themselves to the work; they are to be appointed by the Lord Chancellor. The provisional protection is proposed to be extended to twelve months, the term of the patent to twenty-one years, and the intermediate payments to be made at the end of the 7th, 12th, and 17th years. Provision is also made for additions to patents on making payments of half the amount for the whole previous payments.

The progress of invention in America is stimulated by patents. Here patents are most unsatisfactory. We tax invention; we prevent men from working as long as they please, whilst we speak of free trade. We cannot do better than add here the letter of a distinguished and successful inventor:—"I Consider that the bill, if made law, would be a great boon. To give protection for seven years at the expense of £4 is a very great boon indeed to the working man. Things are occurring every day to mechanics which are entirely lost. As the law is at present it is totally out of the question for a working man to get a patent, but this Act would give him a patent for a price he could afford, and so stimulate his ingenuity to the great advantage of himself and the public; and giving twenty-one years instead of fourteen is a very great matter in many cases. There are many years spent in working and experimenting before any benefit accrues to the patentee. I think you will agree with me that these are great things. There are other things I think we have a right to, but let us get this if we can."

We think the advantages obvious, and it is strange that in a land of so-called liberty a man should find it harder to find protection for an idea which may enrich the world in time, rather than for some small piece of ground fit only to grow a few potatoes. It must be remembered that the idea is frequently of no value to the public unless it is protected for a few years, during which the growth takes place, and the slight germ becomes visible to all, or powerful for much good or for a little, according to circumstances.

We do not enlarge on this; we only mention the fact of the bill existing, as this does not seem to be well known, although we find that at a meeting in Glasgow of inventors, patentees, merchants, and manufacturers, a petition in its favour was drawn up, and it is to be presented to Parliament, we are informed, by Dr. Lyon Playfair.

NOTE ON THE BEARING ON THE ATOMIC WEIGHT OF ALUMINIUM OF THE FACT THAT THIS METAL OCCLUDES HYDROGEN.*

By J. W. MALLET, F.R.S.

In a recent communication to the Académie des Sciences† it has been pointed out by Dumas that metallic aluminium and magnesium, as well as silver, may contain sensible quantities of occluded gas, that obtained from aluminium being almost pure hydrogen.

In a paper on the atomic weight of aluminium, read before the Royal Society on the 22nd of April last, I gave, among other data, the results of several experiments on the quantity of hydrogen liberated by a known weight of aluminium from a strong solution of sodium hydrate. In examining beforehand the purity of the specially-prepared metal used, warned by Dumas's previous results as to gaseous occlusion by silver, of which due account was taken in the paper, I did not neglect to test in like manner the aluminium, but obtained an entirely negative result. As, however, I heated the metal in a Sprengel vacuum only to the highest temperature which a hard Bohemian glass tube would bear, while M. Dumas says that gas is only given off suddenly "*vers le rouge blanc*," I have since seeing his paper repeated the experiment in a porcelain tube, and at this higher temperature, obtained by means of an excellent gas-furnace, with a small remnant of the same material used in the atomic weight determinations.

4.783 grms. of the metal thus treated gave a small bubble of gas, which measured but 0.75 c.c. at 24° C. and 752 millims. pressure. Transferred to a miniature eudiometer it was exploded with oxygen, and seemed to be pure hydrogen. This is but little more than one-third the quantity of gas obtained by Dumas, but his experiments and my own with silver prove that the amount occluded varies with the conditions under which the metal has been fused, and two circumstances in the preparation of my aluminium probably tended to reduce the proportion of hydrogen taken up—namely, that, fearing possible contamination of the metal by its alloying itself with sodium at the moment of reduction from the bromide, I used a considerable excess of the latter, and that, not only were the ingots of sodium wiped free from naphtha with a cloth, but the outside surface was pared off with a knife, and only quite clear and solid pieces of the alkaline metal were used for the reduction.

The above volume of hydrogen only represents a weight of 0.000061 gm.; the corresponding weight from the largest amount of aluminium used in the atomic weight experiments of Series 3 A (in which the gaseous hydrogen evolved was measured), would be but 0.000010 gm., and the corresponding weight from the largest amount of aluminium used in Series 3 B (in which the hydrogen evolved was burned and weighed as water), would be 0.000067 gm.—both quantities too small to be determined by the balance used.

Even if it were possible to correct for this occluded hydrogen the weight of metallic aluminium taken, the greatest error in the volume of hydrogen obtained in Series 3 A would only represent 0.12 c.c., which would be barely measurable in the flask used, and would affect the atomic weight deduced for aluminium to an extent less than one-tenth of the probable error of the mean result. And in Series 3 B the greatest error in the weight of the water produced by combustion of the hydrogen would be 0.0006 gm., measurable by the balance, but affecting the result by less than three-fourths of the probable error of the mean.

Hence it appears that, although there is undoubtedly a constant error due to the cause pointed out by the great French chemist, its influence is in the present case in-

* A Paper read before the Royal Society, June 17, 1880.
† *Comptes Rendus*, 3 Mai, 1880, p. 1027.

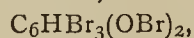
appreciable by existing means of measurement, and no correction of the numbers obtained for the atomic weight in question can be applied which shall have any real meaning.

The other series (1 and 2) of experiments made to determine this atomic weight do not involve the question of gaseous occlusion, at any rate in the same form, as in them the metal itself was not used, but certain of its compounds only.

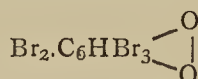
PRELIMINARY NOTE ON SOME ORCINOL DERIVATIVES.

By J. STENHOUSE, LL.D., F.R.S., and CHARLES E. GROVES.

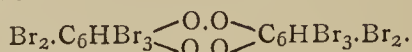
A FEW years ago some halogen derivatives of orcinol were described by one of us (Stenhouse, *Proc. Roy. Soc.*, xx., 72), in which five hydrogen atoms were replaced by the halogen, giving rise to penta-chlororcinol, $C_7H_3Cl_5O_2$, and penta-bromorcinol, $C_7H_3Br_5O_2$. It was thought at first that two, or perhaps three, of the halogen atoms occurred in the methyl group forming the side chain, but the preparation and examination of the corresponding resorcinol compounds at once showed that this could not be the case: apparently both of the hydrogen atoms in the OH groups of resorcinol, $C_6H_4(OH)_2$, were displaced, giving rise, when bromine was used, to the derivative—



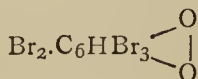
the orcinol compound being $C_6MeBr_3(OBr)_2$. This, however, was considered to be so improbable that the reaction was re-examined by Liebermann and Dittler (*Annalen*, clxix., 252), who fully admitted the correctness of the empirical formula above given: but as they found that penta-bromo-resorcinol readily parted with a molecule of bromine when it was heated—giving a compound of the formula $C_6HBr_3O_2$, which they called tribromo-resorquinone—and being unwilling to admit that the hydrogen in the OH groups had been directly displaced by the halogen, they regarded penta-bromo-resorcinol as an addition-compound of this tribromo-resorquinone, and assigned to it the formula—



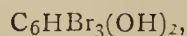
or perhaps—



There is, however, an objection to this view, inasmuch as although penta-bromo-resorcinol, according to them, is an addition-product—



of tribromo-resorquinone, and it is known to be converted with the greatest readiness into tribromo-resorcinol,—

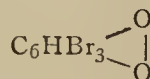


by the action of reducing agents; yet tribromo-resorquinone,—

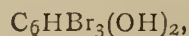


itself is quite unacted on; moreover, the latter is not converted into penta-bromo-resorcinol by the action of bromine.

It seemed probable that if the tribromo-resorquinone had the composition expressed by the formula—



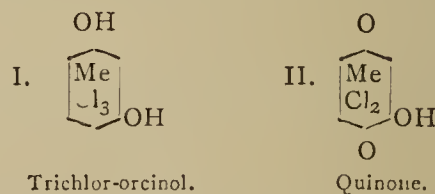
it would be produced from tribromo-resorcinol,—



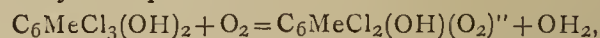
by the action of oxidising agents. Trichlor-orcinol, as in many respects the most convenient compound, was accordingly treated in alkaline solution with potassium ferricyanide, but the result obtained was not in accordance with what would have taken place if the above supposition were correct. On mixing the solutions they acquire a deep purple colour, and if sufficiently concentrated, or if sodium chloride is added, long needles of a deep purple colour are deposited, which consist of the potassium or sodium derivative of a new compound having all the characters of a quinone. On adding hydrochloric acid to the aqueous solution of the purple needles, the quinone itself is precipitated in beautiful deep yellow scales having a brilliant lustre. These are sparingly soluble in water, very soluble in alcohol and in ether, but only moderately so in carbon bisulphide, which is the best medium from which to crystallise it. It melts at 156° , and when crystallised from hot benzene, in which it is readily soluble, separates on cooling in orange-red prisms: these give off benzene when exposed to the air, and effloresce to a brilliant yellow powder. The results of the analyses point to the formula $C_7H_4Cl_2O_3$. When this yellow compound is treated with reducing agents, such as sulphurous acid solution, it is converted into the corresponding hydroquinone or quinol, $C_7H_6Cl_2O_3$, which crystallises from benzene in colourless prisms. The quinhydrone produced by the union of the quinone with the quinol is a crystalline substance of a deep purple-brown colour. The quinone also yields a beautiful crystalline compound when acted on by bromine in presence of water, but we have not yet examined the nature of the reaction.

A resorcinol compound analogous with that above described has also been obtained from trichloro-resorcinol, but the result is not nearly so satisfactory, a large quantity of the trichloro-resorcinol being converted into by-products. It forms yellow crystals, which melt at about 65° . Corresponding bromine derivatives have also been obtained from tribrom-orcinol and from tribromo-resorcinol.

A consideration of the nature of the reaction which takes place in the formation of these compounds throws great light on the probable constitution of orcinol. The strongly-marked quinonic character of the derivative $C_7H_4Cl_2O_3$ points to the presence of the group $(O-O)''$ in the para position, as in benzo-quinone itself, whilst the OH group will occupy the meta position with respect to one of the oxygen atoms, I. representing trichlor-orcinol and II. the hydroxyquinone obtained from it:—

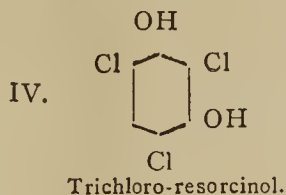
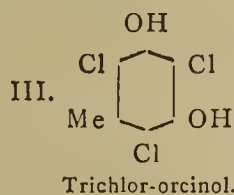


The reaction which takes place, therefore, may be represented by the equation—



one of the chlorine atoms being displaced by an OH group, the hydrogen of that group and of another OH group occupying the para position with respect to it being simultaneously removed by oxidation.

If we remember that when a halogen or the NO_2 group displaces one or more of the hydrogen atoms in phenol such hydrogen atom is in the ortho or para position with respect to the OH group, and not in the meta position, it would seem probable that none of the chlorine atoms in trichlor-orcinol occupies the meta position with respect to either of the OH groups, in which case it would have the formula III. This is confirmed by the fact that in resorcinol, although there are four hydrogen atoms directly united to carbon atoms, only three of these are displaced by the action of halogens, the one which is unaffected being in all probability in the meta position IV.

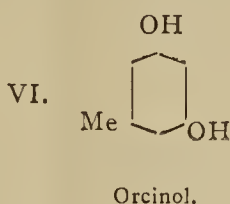
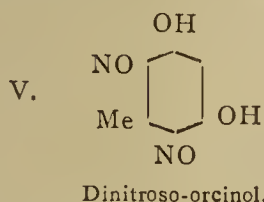


Additional evidence in favour of this view is that phenol forms a *mononitroso* derivative by direct substitution in which $[\text{OH}:\text{NO}=1:4]$, whilst orcinol yields a *dinitroso* derivative, which, from analogy, should have the NO groups in the same relative position with respect to the OH groups, as in V.

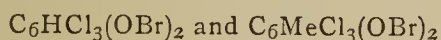
The formation of trinitro-resorcinol and trinitro-orscin, and the behaviour of betorcinol in forming only a *mononitroso* derivative, as recently described by us, all tend to show that in orcinol the CH_3 group occupies the meta-position with respect to the two OH groups, and therefore that it has the constitutional formula—



as represented in VI.



Although Liebermann and Dittler (*loc. cit.*) found that penta-bromo-resorcinol and penta-bromorscinol lose a molecule of bromine when heated, yielding the compounds $\text{C}_6\text{HBr}_3\text{O}_2$ and $\text{C}_6\text{MeBr}_3\text{O}_2$ respectively, they were unable to prepare the corresponding chlorine compounds, as both of the penta-chlorinated derivatives volatilise unchanged. It seemed probable, however, that the trichloro-dibromo-compounds of the formulæ—



might behave differently. These compounds have accordingly been prepared by the action of bromine in presence of water on trichloro-resorcinol and trichlor-orscinol respectively. They are both colourless crystalline compounds, which give off bromine when heated, and yield crystalline products, in all probability the chlorinated derivatives corresponding with tribromo-resorquinone.

We are at present investigating the nature and properties of these interesting compounds.

THE ABSORPTION OF SUGAR BY BONE-BLACK.

By HENRY A. MOTT, Jr., Ph.D., E.M.

It is a pretty generally conceded fact amongst sugar chemists that bone-black absorbs sugar. The exact amount of absorption being variously stated, I determined to conduct a series of experiments to ascertain the fact for myself, as also to ascertain what influence the amount of bone-black usually employed in sugar tests had on the solution. Bone-black is used in two ways by chemists—it is either mixed directly with the solution to be decolourised and shaken up with it, or the solution is poured over a given amount of bone-black in a funnel.

It is evident, to determine the amount of sugar absorbed by bone-black, experiments should be conducted with perfectly pure sugar, as well as with impure sugars, as the latter contain other substances besides sugar, which affect polarised light, and which are apt to be absorbed, in part, by the bone-black, thus effecting the test of the sugar, and showing differences, which, in part, could be attributable to this fact.

It is also best in making comparative tests to dissolve

sufficient of the sugar to be examined at once, so that the same solution may be used for all the tests, otherwise errors due to weighing, variance of sample, and diluting, might creep in, and alter the results. In all my recent experiments I have been careful to observe these points.

To find what influence bone-black would have on sugar solutions, if the solution was poured over a given quantity of bone-black in a funnel, I conducted a number of experiments (about a year ago), which were as follows:—

A solution of granulated sugar was made, which tested directly:—

Aqueous solution	99.9 per cent.
Solution to which had been added 3 c.c. of sulphate of soda and 7 c.c. of tri-plumbic acetate	99.9 "

Solution filtered over 10 grms. of bone-black, tested as follows:—

First portion filtered	99.5 per cent.
Second "	99.7 "
Third "	99.9 "

An inferior sugar was next taken:

16.35 grms. were dissolved in water, to which were added 3 c.c. of concentrated solution of chloride of sodium and 7 c.c. tri-plumbic acetate, and the whole diluted to 100 c.c.

16.35 grms. more of the same sugar were dissolved in water, and sulphate of soda was added, instead of the chloride.

16.35 grms. more of the same sugar were dissolved in water, and 5 c.c. of tri-plumbic acetate were added, the whole being diluted to 100 c.c., and then filtered over 10 grms. of bone-black.

The first solution tested	85.3 per cent.
The second "	85.4 "

The third solution tested in three parts:

First part	84.9 per cent.
Second part	85.0 "
Third part	85.3 "

Many other tests were made, using the same bone-black with similar results. With different bone-black, perfectly free from water, much greater differences were observed in several experiments.

A sugar (decolourised with 3 c.c. Na_2SO_4 and 7 c.c. Pb sol.) tested, 89.1 per cent.

The same sugar, dissolved in water, and to which were added 5 c.c. Pb sol., and then passed over 10 grms. bone-black, tested in three portions, gave:

First portion	88.4 per cent.
Second "	88.7 "
Third "	89.1 "

Without any experiments—admitting the fact that bone-black absorbs sugars—I think it might reasonably be expected where sugar solutions are poured over a given quantity of bone-black in a funnel, that the first portion of the filtrate would be much weaker in saccharine strength than the last portion, for the bone-black would naturally saturate itself with the quantity of sugar it is capable of absorbing at once from the first portion, at a sacrifice of its strength. This is actually found to be the case, as the experiments I have just related clearly demonstrate.

Some experiments, conducted by the late Prof. Merrick, and reported in the CHEMICAL NEWS, arrived at the same results. It would be necessary to obtain an average if this method of decolourising were to be adopted—that either the whole solution be allowed to filter through the bone-black, and then be thoroughly mixed together, or that when one-half is filtered, it be poured on the bone-black and filtered again. This seems an utter waste of lime, when it is possible to mix the bone-black directly with the solution at the start. This method, therefore, must be abandoned as possessing no merit whatsoever.

The experiments which I have recently conducted have

been made with granulated, centrifugal, Muscovada, molasses and refined sugars, and in each case the bone-black was first mixed with the solution before filtering.

Before giving the results of these experiments, it may be interesting to present a few tests made by a number of chemists on the same solutions some few weeks ago, to determine the quantity of sugar bone-black is capable of absorbing. The experiments were conducted in Dr. Grund's laboratory. 130.240 grms. of a molasses sugar were dissolved in water, some tri-plumbic acetate was added, and the whole was diluted to 500 c.c., thoroughly shaken, and then filtered. A portion of the filtrate was tested. Then 50 c.c. of the filtrate were mixed and thoroughly shaken with 1 grm. of bone-black; 50 c.c. were mixed and thoroughly shaken with 3 grms. of bone-black; and to another 50 c.c. of the filtrate were added 5 grms. of bone-black, and the same was thoroughly shaken. Each was filtered separately and tested separately, by each chemist, as follows:—

	Grund.	Behr.	Messemy.	Mott.
The filtrate tested* ..	89.1	—	—	—
50 c.c. + 1 grm. bone-black	89.0	88.9	89.1	89.1
50 c.c. + 3 grms. „	88.6	88.8	88.9	88.7
50 c.c. + 5 grms. „	88.4	88.3	88.7	88.4

From these experiments it will be seen, taking Dr. Grund's test for the plain filtrate as 89.1, and his test for the filtrate containing 10 grms. of bone-black in 100 c.c. of solution (5 grms. to 50 c.c.), as 88.4 per cent, the saccharine strength of the solution was lowered 0.7 per cent.

This reduction in the saccharine strength I found too great when experimenting with larger quantities of solution, and using the same and greater proportions of bone-black.

For example: 260.48 grms. of granulated sugar were dissolved in water, and diluted to 1000 c.c., and a portion of the filtrate was tested (1).

To 500 c.c. of the solution were added 100 grms. of perfectly dry bone-black, the whole was thoroughly shaken, then filtered and tested, first by my assistant Mr. Smylie (who very kindly assisted me in all of the following experiments), and then by myself (2).

To 50 c.c. of the filtrate were added 5 c.c. of water, the whole was thoroughly shaken and tested (3).

To 50 c.c. of the original filtrate, 5 c.c. of water and 5 grms. of bone-black were added, and this was filtered and tested (4).

To another 50 c.c. of the first filtrate were added 4 c.c. of tri-plumbic acetate, 1 c.c. of acetic acid, and 5 grms. of bone black, the whole thoroughly shaken, filtered and tested (5).

The tests were as follows:—

Filtrate No.	Smylie.	Mott.
1.	99.60 per cent	99.60 per cent.
2.	98.90 „	98.90 „
3.	{ 90.50 „	{ 90.50 „
	{ 99.55 „	{ 99.55 „
4.	{ 90.10 „	{ 90.20 „
	{ 99.11 „	{ 99.22 „
5.	{ 90.30 „	{ 90.20 „
	{ 99.33 „	{ 99.22 „

From the first two tests it will be seen that the saccharine strength of the solution was reduced 0.70 per cent, by 20 grms. of bone-black to 100 c.c. of solution; this would prove that 10 grms. reduce it only 0.35 per cent instead of 0.70 per cent, as in the experiments quoted above. From comparison of filtrates 3, 4, and 5 in this experiment, it is also shown that 10 grms. of bone-black only reduce the strength of the solution 0.30 per cent.

Another experiment led to similar results:—

260.48 grms. of granulated sugar were dissolved in water, and diluted to 1000 c.c.

	Smylie. Per cent.	Mott. Per cent.
500 c.c. of filtrate + 50 grms. of bone-black	99.30	99.30
Filtrate alone	99.60	99.60
50 c.c. of filtrate + 5 c.c. water ..	{ 90.40	{ 90.40
	{ 99.44	{ 99.44
50 c.c. + 5 c.c. water + 5 grms. bone-black	{ 90.10	{ 90.10
	{ 99.11	{ 99.11
50 c.c. of filtrate + 4 c.c. tri-plumbic acetate + 1 c.c. acetic acid + 5 grms. bone-black	{ 90.10	{ 90.10
	{ 99.11	{ 99.11

From these experiments, the reduction in saccharine strength was 0.3 per cent, by 10 grms. of bone-black.

EXPERIMENTS WITH CENTRIFUGAL SUGAR.

260.48 grms. were dissolved in water and diluted to 1000 c.c., and filtered.

	Smylie. Per cent.	Mott. Per cent.
50 c.c. of filtrate + 2 c.c. of conc. solution of salt + 3 c.c. of tri-plumbic acetate	{ 87.20	{ 87.20
	{ 95.92	{ 95.92

500 c.c. of filtrate + 100 grms. of bone-black were filtered.

	Smylie. Per cent.	Mott. Per cent.
50 c.c. of this filtrate + 5 c.c. of water	{ 87.20	{ 87.20
	{ 95.70	{ 95.70
50 c.c. of same filtrate + 1 c.c. of acetic acid + 4 c.c. of lead solution ..	{ 96.90	{ 87.00
	{ 95.59	{ 95.70

From these experiments 100 grms. of bone-black to 500 c.c. of solution, or 20 grms. to 100 c.c. solution, only reduced the strength 0.20 per cent; therefore, 10 grms. would only reduce it 0.10 per cent.

EXPERIMENTS WITH REFINED SUGARS.

260.48 grms. of a refined sugar marked "D" were dissolved in water, and diluted to 1000 c.c.

	Smylie. Per cent.	Mott. Per cent.
50 c.c. of filtrate + 2 c.c. of salt + 3 c.c. of lead solution	{ 75.00	{ 75.00
	{ 82.50	{ 82.50
50 c.c. of filtrate + 1 c.c. acetic acid + 4 1/2 c.c. of lead solution + 5 grms. bone-black	{ 74.40	{ 74.40
	{ 81.84	{ 81.84

From this experiment, 10 grms. of bone-black reduce the saccharine strength of the solution 0.66.

EXPERIMENTS WITH GRANULATED SUGAR, USING PREPARED IVORY-BLACK INSTEAD OF ORDINARY BONE-BLACK.

260.48 grms. of the sugar were dissolved in water, and diluted to 1000 c.c.

	Smylie. Per cent.	Mott. Per cent.
50 c.c. + 5 c.c. of water	{ 90.60	{ 90.60
	{ 99.66	{ 99.66
50 c.c. + 2 c.c. of salt + 3 c.c. Pb sol.	{ 90.55	{ 90.60
	{ 99.60	{ 99.66
50 c.c. + 4 c.c. Pb sol. + 1 c.c. of acetic acid	{ 90.70	{ 90.60
	{ 99.77	{ 99.66
50 c.c. + 5 c.c. water + 5 grms. bone-black	{ 90.00	{ 90.00
	{ 99.00	{ 99.00

From this experiment, 10 grms. of ivory-black absorb 0.66 per cent of sugar.

Without giving any more of the experiments I have conducted, I will briefly state the conclusions I have arrived at:

1st. That 10 grms. of bone-black will absorb from a solution of perfectly pure sugar (260.48 in 100 c.c.), sufficient sugar to weaken the saccharine strength of the solution 0.30 per cent to 0.35 per cent.

2nd. That 10 grms. of bone-black will absorb sufficient sugar, and, also, other substances which effect polarised

* Owing to its being too dark, the tests were not considered reliable.

light, from a solution (26·048 in 100 c.c.) of an impure or raw sugar, so that the saccharine strength of the solution will show a loss of from 0·10 per cent to 0·66 per cent by the direct test, but only 0·30 to 0·35 per cent by the inverted test.

3rd. That some perfectly dried bone-blacks will absorb more sugar than others.

4th. That the greatest amount of absorption by 10 grms. of any kind of prepared bone-black on a solution of pure sugar (26·048 in 100 c.c.), is only sufficient to reduce the saccharine strength of the solution 0·70 per cent.

5th. Since 2 grms. of bone-black are quite sufficient to decolourise a filtered sugar solution which has been partly clarified and decolourised by means of tri-plumbic acetate, it can be used with safety, as the amount of sugar it will absorb would hardly effect the saccharine strength of the solution appreciably.

6th. Supposing 10 grms. of bone-black to reduce the saccharine strength of a solution (26·048 in 100 c.c. water) of pure sugar testing 100 per cent, exactly 0·35 per cent, then we can deduce the fact, that 1 ton of bone-black will absorb 18·2 lbs. of sugar, or, bone-black is capable of absorbing 0·0091168 per cent of sugar.—*Journal of the American Chemical Society.*

DISTILLATION OF GAS-TAR.

TAR DISTILLERS v. GAS COMPANIES.

AMONGST the many industries that have been pursued as the arts and manufactures have progressed few stand out so prominently, conspicuous in the strides made, and in the results that have accrued, as those presented to our contemplation in the discoveries achieved in dealing with a once refuse substance occurring in vast quantity, and which has been rescued and reclaimed from the opprobrium of "waste,"—a material which at one time, and not distantly removed from the present, was looked upon and regarded as utterly useless and worthless: we refer to the product widely known as gas-tar.

Formerly, at our gas works and in the distillation of coal everything obtained was looked upon as waste except the illuminating gas, carburetted hydrogen. True it is that the resultant tar was collected and even used, much of it serving as fuel under the retorts employed in producing the gas: but withal it was regarded as a most unpleasant companion, to touch which was to be defiled. From the period referred to to our own day what a change has passed over the interval and the spirit of men's minds as directed and governed by the impulse of progress. It has been said, from the "pine torch to the paraffin candle how wide the interval! between them how vast the contrast!" May we not extend the thought a little further, and, embracing our modern gas illumination, view it as a still wider and grander interval. Continuing our flight, and without in any way encroaching on the realms of romance, may we not further regard this reclaiming of a waste substance, and its history as exemplified in the endless variety of valuable substances—inclusive of the magnificent and beautiful colouring matters derived from it—as one of the most wonderful industries of modern times; wonderful alike to the scientific investigator and discoverer and the eager speculator, who has not been tardy in availing himself of its solid and substantial benefits, and which in its commercial aspect has, and will still continue to, command millions of capital. That this statement is in nowise exaggerated, we need only point our readers to public statistics of products derived from coal-tar during the past twenty years—historical on the one hand, widely popular and well known on the other.

We have been led to these thoughts from notice of a remarkable movement now taking place amongst our public gas companies, and which may be looked upon as the starting-point of an altogether new state of things in

the history of tar distillation; as one bidding fair to revolutionise the old and existing system, and as calculated to introduce reforms for which there can be no doubt there is a broad and wide latitude. We refer to the growing disposition on the part of these companies to erect plant and utilise their once refuse gas-tars.

As everyone knows, for a number of years past this industry has remained in the hands of the tar distiller; and it is not too much to say that in the majority of cases few branches have suffered more in remaining so long and lamentably in the background for want of scientific aid and direction.

If we have to take our coal to the gas-house—and there subject it to destructive distillation as the process is called, and in order to obtain the gas with which we illuminate our towns and dwellings—we have, or rather have had, to take our gas-tar to the tar distiller so-called, and there leave it in his hands in order to treat and obtain from it a moiety only of the valuable products existing in it.

That we are not, in thus expressing ourselves, overtopping the limit of fact we are tolerably well assured. Nay more; we have a lurking impression that the tar distiller has hitherto cared little either for the chemistry of his subject or what may have come out of it: in the majority of instances he has been quite content to jog on and distil tar, indifferent alike to the opportunities and advantages presented to him in material at command and the wide field for research and discovery accruing from the pursuit of his avocation. Whilst manufacturers in other directions, keenly alive to advantages resulting from the most trifling improvements in their processes, have been ready to adopt and develop them, the tar distiller notably has stood aloof, notwithstanding the remarkable discoveries of science, which have pointed to the products of his labour as a practically limitless and inexhaustible field of investigation—a mine of wealth presented perhaps by no other department of applied chemistry. As regards the pecuniary results attached to the pursuit of this special industry, and in a commercial sense, we have always been led to believe them as visionary if not *nil*. Notwithstanding, however, these assertions there has long existed a shrewd suspicion that behind the black and offensive material there has after all been a "silver lining," and a mine that has brought much grist to the mill of the distiller. Possibly this surmise has not existed without tangible foundation, regard being had to the manner in which some of our tar distillers have extended their operations in tar products, and in a direction which has undoubtedly invited attention to their doings and the magnitude of the issue. That our gas companies should at last have woken up from their lethargy, and to the fact that in disposing of their tar they have relinquished a vast and remunerative source of revenue is not at all surprising, and that some of them should even have reached the not altogether illogical reasoning that distillation and utilisation of their own tar is perhaps equally within their legitimate province, as the conversion of crude tar products into actual colouring matter by the tar distiller himself, due regard being had to the somewhat wide range running between tar distilling on the one hand and colour making on the other. That such an awakening should have occurred is, we reiterate, by no means astonishing, and the almost general desire evinced on the part of others to contest the point and follow in the wake of one of our largest and most influential public companies, who have set the example on a most magnificent scale, exhibits, we think, a fact that is not without significance and import as regards the future.

In reviewing the claims of the contending parties we hold that the movement now taking place and influencing our gas manufacture is a step in the right direction, and as calculated to introduce progress and reform in a process hitherto admittedly crude. We hold that the distillation of tar and its conversion into such products as ammonia, benzol, naphthas, carbolic acid, creosote, and anthracen up to pitch, as strictly within his province, and one to which he cannot long remain indifferent. Furthermore,

we hold that in absorbing the larger share of the industry, and which we believe he ultimately will do, he lifts it from an antiquated and empirical position, and places it on a footing where it will receive the many advantages derivable from the infusion of new blood, and the supervision and direction springing from the more advanced knowledge and requirements of the times in which we live. We think it is not too much to hazard this assertion, inasmuch as already do we see fruit of a venture that at the period of its contemplation was met with considerable rebuff and evil prognostication. It was urged, on the one hand, that "no gas company would either distil or succeed in distilling tar." It was said, on the other, that they "lacked experience and knowledge impossible to obtain: more, that even if actually "launched and in operation they would fail in showing financial success." Yet, in direct opposition to these statements, what do we really see? Within the brief space of a few short months has sprung up in our midst one of the finest and most perfect plants for tar distilling in the kingdom, and on the grounds and within the walls of an English Gas Company, second to none in dimension, efficiency, utility, and advantages derivable from the adoption of modern improvement, which, by-the-by, as regards some of them (curiously enough) are only now finding their way into other establishments whose origin have not been altogether of yesterday's date.

Here we have the nucleus of a concern that may, as time progresses, possibly rival that of our continental neighbours, the French, although whether we shall at any time award a like amount of scientific interest and enquiry to the pursuit of a commercial object is altogether another matter. Whether we shall ever conduct in our technical laboratories a class of work such as is carried out in those of the Paris Gas Company under the able directorship of its accomplished chemist and professor, M. Audouin, or appoint at their head men of similar talent and capacity, is also a matter of considerable surmise.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 17, 1880.

Prof. H. E. ROSCOE, President, in the Chair.

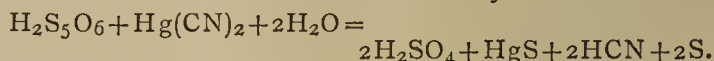
THE following certificate was read for the first time:—
A. C. Hawkes.

During the evening a ballot was taken, and the following gentlemen declared duly elected Fellows of the Society:—
P. S. Brown, H. Brown, K. dal Dey Rai Bahadour, G. H. Hughes, H. A. Lawrance, F. E. Matthews, E. Moritz, R. S. Marsden, E. A. Reilly, Tarapwsanna Roy, C. Rawson, B. Symons, T. Taylor, H. K. Tompkins, R. G. Watts, A. Wingham.

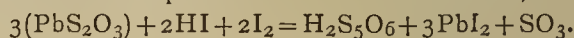
The following papers were read:—

"On Pentathionic Acid," by T. TAKAMATSU and WATSON SMITH. The authors, at the suggestion of Dr. Roscoe, have examined the proofs for and against the existence of this substance advanced by Wackenroder, Spring, Stingl, and Morawski, Kessler, &c. The acid was originally prepared by Wackenroder by passing hydrogen sulphide into a saturated solution of sulphurous acid. Spring obtained what he thought was the potassium salt of the acid in an alcohol-ether solution, and proved it to be merely a tetrathionate. Stingl and Morawski proved, by qualitative and quantitative results, that an acid containing more sulphur than tetrathionic acid exists in Wackenroder's solution. Spring also asserts that the alleged solution of pentathionic acid contains a mixture of tetrathionic and hyposulphurous acids. The authors have confirmed the results

obtained by Kessler in the investigation of the reaction of Wackenroder's solution with mercuric cyanide—



The ratios between the S in the sulphuric acid to that in the HgS to that existing in the free state were 2.01 : 1 : 2.08, whereas tetrathionic acid would give 2 : 1 : 1. They have also investigated the statement of Spring that the Wackenroder solution is not decomposed by alkaline hydrates, but yields a solution containing tetrathionate without separation of sulphur. The authors prove that a decomposition really occurs and that sulphur is separated, but its separation is masked, because as fast as it is precipitated it is re-dissolved by the alcohol-ether solution. They also prove that no hyposulphurous acid is present in the Wackenroder solution. They finally synthesised both tetrathionic and pentathionic acids by acting upon lead thiosulphate in the first instance with a moderately strong solution of iodine in hydriodic acid, when tetrathionic acid was produced, and secondly by acting on lead thiosulphate by as strong a solution of iodine in hydriodic acid as could be obtained when pentathionic acid was formed,—



Analyses of the products are given. The authors conclude that pentathionic acid exists, and recommend the reaction just given as a convenient and expeditious method of preparing it. They hope to be able to prepare some of the pentathionates which, under ordinary conditions, are less stable than the acid. In conclusion, they contrast the reactions of the thionic acids; pentathionic acid being distinguished by giving an immediate precipitate of sulphur with caustic potash, re-dissolving gradually on standing, and an almost immediate brown colouration, becoming black on warming with ammoniacal silver nitrate.

"Preliminary Note on some Orcinol Derivatives," by J. STENHOUSE and C. E. GROVES. Printed in full in another column.

"On the Determination of Carbon in Soils," by R. WARINGTON and W. A. PEAKE. The authors have examined the various methods which have been proposed:—Oxidation by chromic acid, suggested by Wolff (this gives but 79 per cent of the carbon present); oxidation by permanganate. 10 grms. of the soil, with 5 grms. KHO in 25 c.c. of water and solid permanganate, are digested in a salt-bath for an hour. The mass is then treated with H_2SO_4 , and the CO_2 evolved absorbed. This method gives 92 per cent of the total carbon. The best method is that of combustion in oxygen with oxide of copper, after treatment with sulphurous acid.

"Note on Camphydrene," by H. E. ARMSTRONG. The author sharply criticised a recent paper by Dr. Letts in the *Berlin Berichte*, in which Dr. Letts repeats an assertion made some months ago at this Society that the chief product of the action of sodium on the solid hydrochloride from turpentine oil is apparently homogeneous, as it has a constant boiling-point, and soon attains a constant melting-point on re-crystallisation from alcohol, and that its formula is $\text{C}_{10}\text{H}_{17}$, the mean only of ten analyses being given, which corresponds closely with $\text{C}_{10}\text{H}_{17}$. Dr. Armstrong asked whether these ten analyses were the same as those brought before this Society, and which were widely discordant. Dr. Armstrong has also repeated the experiments of Montgolfier, and completely confirms his conclusions that a mixture of camphene, $\text{C}_{10}\text{H}_{16}$, and of camphydrene, $\text{C}_{10}\text{H}_{18}$, is produced: from this mixture the latter substance can be isolated by agitation with 2 : 1 sulphuric acid and steam distillation. Thus a larger yield of the hydrocarbon can be obtained than by Montgolfier's process.

"On the Action of Nitric Acid upon Dipara-tolyl-guanidine," by A. G. PERKIN. This substance dissolves in strong nitric acid (sp. gr. 1.5) with rise of temperature. From the product, ammonia, after dilution and filtration, precipitates a new base having the formula $\text{C}_{15}\text{H}_{15}\text{N}_5\text{O}_4$, dinitro-dipara-tolyl-guanidine, melting at 205° , soluble in

cold alcohol, insoluble in water: it crystallises in red prisms. Treatment with tin and hydrochloric acid yields a new base, which is under investigation. By mixing an alcoholic solution of the nitrate of ditolyl-guanidin with its own volume of nitric acid (sp. gr. 1.4), and heating until chemical action ensued, another substance, crystallising in primrose needles, was obtained, which proved to be dinitro-dipara-tolyl-urea, melting with decomposition at about 233°. On boiling this substance with alcohol acidulated with hydrochloric acid and tin diamido-dipara-tolyl-urea was obtained in minute crystals. Ortho-ditolyl-guanidine, when treated with fuming nitric acid, also forms a nitro-base, which the author is at present investigating.

"On some Higher Oxides of Manganese and their Hydrates," by V. H. VELEY. In order to avoid any possible contamination with alkalis the author proceeded as follows:—A solution of manganese chloride was precipitated with ammonium sulphide. After washing, this was dissolved in acetic acid and diluted, and a current of chlorine passed for a limited time at 52° C. Black glistening scales were precipitated, which were air-dried. This substance is a hydrate, $\text{MnO}_5\cdot 5\text{MnO}_2$. After heating to 200° to 250° it was found that the oxide did not lose weight, even when heated for several hours, and that the loss of the weight of the oxide did not agree with the gain in weight of the drying-tube. The oxide dried for many hours at this temperature retained a small but constant quantity of water. This constant oxide was found to be a monohydrate of a high oxide, $\text{MnO}_{11}\cdot \text{MnO}_2$. The loss of water during the heating of the substance to 200° in dry air is simultaneous, but not necessarily dependent upon the absorption of oxygen. Experiments were made to determine the various hydrates of the oxide, and the lowest temperature at which oxidation begins in air and in oxygen. The oxide was heated in a slow current of air at intervals of 10° from 60° to 210°, and the loss and the water determined, each experiment lasting two hours. In air the principal points were 60° to 100° for $\text{MnO}_5\cdot 5\text{MnO}_2\cdot 2\text{H}_2\text{O}$; at 120° to 130°, $2(\text{Mn}_6\text{O}_{11})\cdot 3\text{H}_2\text{O}$; at 200°, $\text{Mn}_{12}\text{O}_{33}\cdot \text{H}_2\text{O}$. On repeating the experiment with oxygen instead of air further oxidation took place, and an oxide, $\text{MnO}_{23}\cdot \text{MnO}_2$, was obtained. In no case was the dioxide formed.

"On a New Method of Preparing Dinitro-ethylic Acid," by E. FRANKLAND and C. C. GRAHAM. This substance, N_2EtOHO , was first prepared by the action of zinc-ethyl upon nitric oxide. The process was, however, difficult and inconvenient (*Phil. Trans.*, 147, 59). They have adopted the following plan, which works well:—100 grms. of pure zinc-ethyl are introduced into a tubulated distilling flask, and enough sodium added to convert all the zinc-ethyl into the compound ZnNaEt_3 , taking care to keep the temperature down. When all action has ceased 100 c.c. of benzene are added, and the nitric oxide passed over the solution, the flask being shaken from time to time. When the absorption has ceased some commercial ether is added to the cooled contents. The zinc-ethyl is thus gradually decomposed, and finally the decomposition is completed by adding first some alcohol and then water. The zinc is precipitated by carbonic anhydride, and the filtrate evaporated to dryness. On treating the residue with absolute alcohol the sodic dinitro-ethylate alone dissolves out. It is purified by conversion into the copper salt: in some cases a yield of 50 per cent of pure copper salt was obtained. A further study of the reactions of this acid is promised.

"On the Action of Organo-zinc Compounds upon Nitriles and their Analogues (III. Action of Zinc-ethyl on Phenyl-acetonitrile)," by E. FRANKLAND and H. K. TOMPKINS. The phenyl-acetonitrile employed was prepared by digesting benzylic chloride with a solution of crude potassic cyanide in dilute alcohol. In one experiment, while preparing this substance on the large scale 2000 grms. of the chloride being used, a substance was observed to distil over above 300°. The distillation was stopped: the residue solidified on cooling, and was found to contain tribenzylamine. The authors were unable to

obtain phenyl-acetonitrile perfectly pure, and they therefore employed the portion of the distillate boiling at 200° to 230°. On treating it with zinc-ethyl a reaction immediately began, and had to be checked by cooling. Towards the end the temperature had to be raised to 130°. The product is decomposed by alcohol, and finally extracted with that liquid. The crystals thus obtained contained two substances, the least soluble fusing at 170° to 171°, crystallising in silky acicular tufts; the second fused at 150°, and crystallised in rhombohedra. The first substance has the same empirical formula as phenyl-acetonitrile, but differs from it in all its physical properties. From analogy its formula should probably be tripled: the authors propose to call it Cyanbenzine. It is sparingly soluble in hot alcohol; glacial acetic acid, benzene, and carbon disulphide dissolve it readily. Analyses of the second substance indicated the formula $\text{C}_{32}\text{H}_{27}\text{N}_3\text{O}$. The authors have named it Benzacin. It is a neutral body. The mother-liquor from these two bodies contained a high boiling-point oil, which the authors have not yet succeeded in satisfactorily isolating and purifying.

"On the Action of Benzoyl Chloride on Morphine," by C. R. A. WRIGHT and E. H. RENNIE. The authors prove, by various analyses and other quantitative measurements, that by heating benzoyle chloride and morphine in sealed tubes to 100° to 110° for several hours, the end result is invariably dibenzoyl morphine ($\text{C}_{17}\text{H}_{17}\text{NO})(\text{OC}_7\text{H}_5\text{O})_2$, morphine being for simplicity's sake regarded as $\text{C}_{17}\text{H}_{19}\text{NO}_3$, and that the product thus formed is identical with that formed by the action of benzoic anhydride on morphine. The conclusions recently arrived at by Polstorff (*CHEMICAL NEWS*, vol. xli., p. 220) that a tribenzoylated base is formed are shown to be utterly without foundation, Polstorff's own analytical figures rather indicating that the substance examined by him was the dibenzoylated base, whilst no determination at all of the molecular weight or of the quantity of benzoic acid regenerated by saponification was made as a check. Morphine contains two-thirds of its oxygen in the form of hydroxyl, and one-third functioning in some other way, as has been shown by C. R. A. Wright in conjunction with G. H. Beckett.

"On an Examination of Terpenes for Cymene by means of the Ultra-violet Spectrum," by W. N. HARTLEY. In a previous paper (*Proc. Roy. Soc.*) the author, with Prof. Huntington, has shown that cymene gives a well-marked absorption spectrum, which is not entirely obliterated by dilution with 5000 volumes of alcohol, if a layer of 15 m.m. in thickness be used. The author has in this way tested samples of orange oil, French turpentine, and Russian turpentine, received from Dr. Armstrong, for cymene. The first two substances contained no cymene; the Russian turpentine contained less than 4 per cent. The author concludes that the cymene found in these oils by Dr. Armstrong was formed by the chemical treatment to which they were subjected. The author examined the so-called terebene, which is also free from cymene.

"Notes on the Purple of the Ancients," by E. SCHUNCK. The author has examined a sample of cotton recently dyed with the juice of a species of shell-fish, *Purpura patula*, obtained through Herr von Stralendorff, from the west coast of Nicaragua, where this method of dyeing is still practised by the natives. The purple was dull and not so brilliant as was expected; it was dissolved out of the fabric by boiling aniline, and was found to have all the properties of the Punicin previously obtained by the author from the *Purpura lapillus* of the British coasts. It is characterised by its extreme stability, especially with regard to powerful oxidising agents. It dissolves in concentrated sulphuric acid, yielding a dull purple solution, having an absorption band between D and E. On heating this solution and allowing it to stand the colour changes to a bright green, and the band disappears. This green solution gives with water a purple precipitate.

The Society then adjourned over the summer recess.

PHYSICAL SOCIETY.

Ordinary Meeting, June 12, 1880.

Dr. HUGGINS, F.R.S., in the Chair.

NEW Members:—Mr. H. B. Luff, Mr. Adam Hilger, Mr. C. V. Boys.

Dr. SHETTLE, of Reading, read a paper "*On the Influence of Solar Radiation on the Earth's Rotation.*" The fact established by Dr. Shettle, that the magnetic energy of a bar-magnet acts along spiral lines, has led him to surmise that the energy emanating from the sun and impinging on the earth on the zone of the ecliptic traverses the earth in a spiral path, and finally emerges at the magnetic poles. The spiral of energy is "right-handed" at one pole and "left-handed" at the other, like the magnetic force in a magnet, and the electric discharge in Crookes's vacuum tubes. Like to precession and nutation these spiral paths are constantly changing and producing magnetic variations. He therefore infers that the magnetic poles will complete a cycle corresponding to the period of precession. Dr. Shettle thinks that bodies exhibit magnetic properties in proportion as they change the direction of the energy traversing them, and throw it into the spiral form. Terrestrial magnetism would be due to the solar radiance. On this hypothesis gravity also would be produced; so, likewise, would the earth's rotation (by a kind of "magnetic whirl"), electricity, tornadoes, cyclones, water-spouts, and whirlwinds. Moreover, this "spiral energy" would seem to operate throughout the whole universe.

Prof. WIEDEMANN, of Leipsic, made a communication "*On the Phenomenon of Interference in Rays of Long Path,*" and showed how the phase of vibration of the atom or molecule emitting the rays influenced the phenomenon. Molecular collisions could operate in preventing interference. From a study of this question he was able to deduce a method of determining the pressure on the surface of the sun and stars. He mentioned that he had found that the temperature of a glowing gas in Geissler's tubes may be under 100° C., and therefore the light of the aurora or of comets might be accompanied by a low temperature. He had determined that the quantity of heat produced in a gas by the electric discharge was always the same with the same amount of electricity, whether discharged at once or not, and that it increases nearly in proportion to the pressure of the gas. He had also determined that the heat which must be developed by a discharge in hydrogen in order to change the band spectrum of H into the line spectrum is about 100,000 calories for 1 gramme of hydrogen, and hence this might represent the amount of heat necessary to transform the hydrogen molecule into its atoms.

Dr. SCHUSTER suggested that Prof. Wiedemann should make a similar experiment with another gas, say nitrogen, as there was a disagreement about the H spectra, and Professor WIEDEMANN stated that he so intended.

Mr. RIDOUT exhibited a device for amplifying small motions. A small barrel is slung by two threads between the prongs of a metal fork in such a manner that if the fork is bodily carried to and fro the barrel will rotate round its axis. This is simply effected by making each thread, in its passage from one prong to the other, take a few turns round the barrel. To the barrel an index is attached, and the fork is then fixed on the body whose minute motion is to be indicated. The translation of the body shifts the fork and rotates the barrel, which in turn deflects the index round the face of a dial, and the magnifying power is expressed by the ratio of the diameter of the barrel to the length of the index. With this apparatus Mr. Ridout exhibited the lengthening of an iron core when magnetised by the passage of the current of two Grove's cells through an insulated wire coiled round it. By rivetting a slip of brass to the iron the unequal expansion of brass and iron under heat were also shown, the heat

being generated by keeping the current flowing in the coil.

Mr. D. WINSTANLEY exhibited his new radiograph for recording graphically the intensity of solar radiation throughout the day. It consists of a differential thermometer, with one black bulb and a circular stem. The lower part of the stem is filled with mercury, the upper branches with sulphuric acid and water. The tube is mounted on a brass wheel, so that when the black bulb is exposed to the sun's rays the differential motion of the mercury causes the wheel to turn. The wheel carries a light index or marker, which is free to traverse a vertical cylinder covered with paper coated with lamp-black, and leaves a white track where its point has scratched off the soot. The radiogram thus produced can be fixed and preserved.

Dr. GUTHRIE pointed out the curious "thermal twilight" these radiograms had betrayed to Mr. Winstanley. They show that before sunrise the temperature increases owing to solar radiation. Moreover, half an hour after sunset the index falls, and remains till within a few minutes of midnight, when it mysteriously rises and sinks again, although the sun is then directly over the opposite hemisphere.

Mr. BAILLIE then gave the results of a study he had made into the theory of the phoneidoscope. He finds that waves simultaneously start from each side of the soap film when the note is sounded, and, meeting in the middle, generate ventral points and nodes. The equations of several cases were given by him, and he suggested that photography should be employed to fix the appearance of the figures in order that they might be investigated theoretically.

THE METEOROLOGICAL SOCIETY.

THE last Ordinary Meeting of this Society for the present session was held on Wednesday, the 16th inst., at the Institution of Civil Engineers, Mr. G. J. SYMONS, F.R.S., President, in the Chair. T. W. Barry, M.D., A. W. Martin, and C. E. Peek were elected Fellows; and Senor A. Aguilar and Dr. H. H. Hildebrandsson were elected Honorary Members of the Society. The following papers were read:—

"*Ozone in Nature, its Relations, Sources, and Influences, &c.,*" from fifteen years' observations ashore and afloat under all conditions of climate, by J. MULVANY, M.D., R.N. The meteorological elements with which ozone is most intimately associated are such as occasion high vapour tension and a high degree of saturation: therefore it is promoted by wind passing over a large aqueous expanse and by heat producing rapid evaporation. Hence heat, if humid, is no bar to atmospheric ozonisation, but no definite relation exists in the atmosphere between heat *per se* and ozone; its relation to humidity is more definite and direct, but subject to many exceptions. In consequence of this relation it most abounds where its chemical qualities render it most useful. It appears to be formed in the upper strata and to be carried downwards by raindrops, whose office is vehicular. The spherules of water which constitute clouds and have their origin in radiation and condensation, have a similar office. Ozone does not appear to diffuse readily downwards, so that when the lower strata are robbed of ozone by jungle, &c., a considerable difference in the ozonic condition close to and at 170 feet above the surface may exist. The author is of opinion that no disease can be clearly traced to ozone as met with in the atmosphere.

"*The Average Weight of the Barometer in London,*" by HENRY STORKS EATON, M.A., F.M.S.

"*Note on a Waterspout Observed at Morant Cays, Jamaica, March 23, 1880,*" by Lieut. ALFRED CARPENTER, R.N., F.M.S.

"*Account of a Balloon Ascent from Lewes in a Whirlwind, on March 23, 1880,*" by Capt. JAMES TEMPLER and H. ELSDALE.

"Results of Meteorological Observations made at Stanley, Falkland Islands, 1875-77," by WILLIAM MARRIOTT, F.M.S.

"A New Thermograph," by WILLIAM DAVID BOWKETT.

"The Winter Climate of Davos," by C. T. WILLIAMS, M.D., F.M.S. Among the high altitude sanatoria of Europe, Davos at present enjoys the greatest reputation, partly on account of its easy accessibility, and partly on account of certain peculiarities of position and shelter. The Valley of Davos lies in the Canton of the Grisons, between the valleys of the Lower Rhine and the Upper Engadine. The valley runs from N.N.W. to S.S.E. for about 10 miles in length, with an average breadth of about a third of a mile, being for the most part of this extent a plain, gently sloping towards the north, and varying in elevation from 5400 to 4500 feet. Davos Platz is 5105 feet above the sea level. The author discusses the observations made during the four winters of 1876-7 to 1879-80. The peculiar effects of Davos winter climate seem to depend on (1) The rarefaction of the atmosphere; (2) Its dryness; (3) The absence of strong currents, owing partly to shelter, and partly to the uniform layer of snow spread around; and (4) The large percentage of the direct solar rays reaching the locality owing to rarefaction of the air, and also the considerable amount of heat reflected from the extensive snow plain in front of the villages of Davos Platz.

NOTICES OF BOOKS.

Nature's Hygiene; a Series of Essays on Popular Scientific Subjects, with Special Reference to the Chemistry of the Eucalyptus and the Pine. By C. T. KINGZETT, F.C.S. London: Baillière, Tindall, and Cox.

IN examining this interesting work we will first turn our attention to the fourth chapter, in which the author treats of infectants and contagious diseases. After enumerating some of the most signal epidemics recorded in history—among which he places the miasmatic fever of Cyprus in 1878-79—he quotes from Dr. Drysdale's work on the Germ Theory ("The Germ Theories of Infectious Diseases." By J. Drysdale, M.D. London: Baillière, Tindall, and Cox) the statement that in view of "the almost total impotence of medicine for direct cure, we may indeed consider this a question more for statesmen and governing bodies than for physicians." But what if statesmen and governing bodies, in virtue of the purely literary education which they receive, are grossly ignorant of the laws upon which health and disease attend, and refuse to be instructed? Sanitary reform is too useful and too practical to be a favourite with orators and sophists. After defining the three chief groups of so-called infectious diseases, the author subjects the "germ theory"—which has been somewhat too implicitly adopted by the public—to a rather severe criticism. The great fault of the theory is that it proves too much. Germs have been found, not merely in the fluids of men and other animals suffering from epidemics or from contagious diseases, not merely in the air of sewers and cesspools, but literally everywhere. If, therefore, these microbia are the cause of disease how does any mortal escape? It may, indeed, be contended that unlike true poisons, or rather poisonous chemical individuals, the poisons of disease, whether organised or not, do not act upon all human beings alike. The negro is proof against yellow fever, which sweeps away his white neighbours. The European inhabitants of the Fiji Islands were scarcely affected by that epidemic of measles which decimated the aborigines. Hence, then, it may be contended that admitting the very common occurrence of disease germs, they are capable of proving destructive only to persons of certain races, constitutions, or idiosyncracies.

A fact which strongly militates against the germ theory is brought forward in the fourteenth report of Dr. J. M.

Cunningham, the Imperial Sanitary Commissioner of India (1879). He points out that cholera "almost invariably travels, not down, but up the great drainage channels of the country." The same has been, as far as we are aware, the case in Europe. The pestilence first appeared at the mouths of the Wolga, the Neva, the Elbe, the Tyne, &c., and did not descend to these points from the interior of the country. Now we have always been told that the *materies morbi* of cholera is to be found in the excreta of the patients which find their way into wells and streams and poison those who partake of the water. That typhoid fever has thus been communicated by polluted wells and by streams flowing from an infected district seems to be established beyond the reach of doubt, but the progress of disease against rather than with the stream cannot be thus accounted for.

The theory of Dr. Drysdale, with which Mr. Kingzett substantially agrees, is that contagious diseases, in the stricter sense of the term, are occasioned by diseased secretions thrown off by animals suffering from the same disease, and differing greatly among themselves in volatility and persistence. The miasms or malarious poisons consist, on the contrary, in secretions thrown off by diseased vegetables, or perhaps it would be better to say by vegetable matter in certain phases of decomposition.

As a fact inconsistent with the germ theory mention is made of the geography of disease, as studied by Mr. Haviland. In a country so limited in extent and so uniform in climatic conditions as England we should expect to find little difference in the diseases met with in its various districts. Such, however, is not the case: Lancashire and Cheshire are a nidus of scarlet fever, whilst, on the other hand, the Thames "flows through a vast cancer field." If we remember rightly, rabies in dogs and its consequence, hydrophobia, in the human subject are also very local and at the same time constant in their occurrence.

Mr. Kingzett declares himself very sceptical as to the benefits to be derived from the use of carbolic acid as a disinfectant. Admitting its power of destroying germs he considers this property of little value so long as infectants are not known to be not merely organic matter but living organisms. He quotes from the *Transactions of the Social Science Congress* for 1874 the statement of Dr. Dougall that a minim of vaccine lymph retained its virtues unimpaired after having been kept for thirty-six hours in the midst of a cubic foot of concentrated carbolic vapours. He declares also, on the authority of the *British Medical Journal* of February 21, 1874, that carbolic acid had proved so unsuccessful in arresting the spread of yellow fever at New Orleans and Mobile that it was even suspected of proving positively injurious. As we have often heard accounts of the very opposite character we prefer here to suspend judgment.

Mr. Kingzett is a firm believer in the disinfecting powers of the *Eucalyptus* and also of various species of conifers. Whilst we fully admit that the sanitary character of a country is almost invariably lowered by denuding it of trees, we cannot forget that we have known villages surrounded with pine forests and yet suffering year after year from malignant dysentery. This, however, is not improbably a directly parasitic affection, since it is very extensively ward off by using no water except such as has either been boiled or treated with alum.

Mr. Kingzett has no faith in ozone as a natural disinfectant, but attaches much more importance to hydrogen peroxide.

The work is a very valuable contribution to sanitary literature, and is calculated to disabuse the public mind of some errors.

Handels-Bericht von Gehe and Co., of Dresden. April, 1880.

AN exceedingly thorough report on the imports, exports, stocks, and prices of drugs and chemicals, with an elabo-

rate essay on the causes of the prevailing commercial depression.

Practical Chemistry: the Principles of Qualitative Analysis. By W. A. TILDEN, D.Sc., F.C.S., &c. London: Longmans, Green, and Co.

THIS is a little book of 108 pages, intended, as the learned author says, for beginners. In the first part, "the student is required to make himself familiar with the appearance and properties of a few substances with which he will afterwards constantly be dealing." In the second part we have a brief exposition of the analytical methods. As might readily be expected we observe no errors in the work, but we should be somewhat at a loss to point out any well-marked superiority which it enjoys over some of the multitudinous elementary works on chemistry which have appeared within the last few years.

Report on the Revision of the United States Pharmacopœia. Prepared and Compiled by CHARLES RICE. New York: 1880.

AMONG the alterations and improvements proposed we note the recommendation that the text of the new Pharmacopœia is to be drawn up in the English language. This is, in our opinion, a decided change for the better. The use of the Latin language in scientific works and documents is now merely a "survival" for which no valid reason can be brought forward. It is proposed that all chemicals of a definite character shall be accompanied by their formulæ, both on the old and the new notation. It appears that a report on the German Pharmacopœia pronounces the addition of such formulæ of little value, since they may soon have to undergo a reconstruction. Empirical formulæ give the greatest facilities for calculating the proportionate quantity of any constituent in a compound. Temperatures it is proposed to express both in degrees of Fahrenheit and Centigrade, and weights both in decimal and apothecaries' weights. It would surely be better still if in all recipes, for any purpose whatever, the proportions were expressed simply in "parts," which would be equally intelligible in all countries.

Sixteenth Annual Report of the Alumni Association, with the Exercises of the Fifty-ninth Commencement of the Philadelphia College of Pharmacy.

THIS report contains an account of the proceedings at the annual meeting of the College, with a list of the students who have graduated in pharmacy and of those who have taken exceptional honours.

Proceedings of the American Pharmaceutical Association at the Twenty-seventh Annual Meeting, held in Indianapolis, September, 1879. Philadelphia: Sherman and Co.

WE have here a bulky and closely-printed volume well stored with important facts. Among the essays presented to the meeting we may mention that by P. C. Candidus on "The Increase of Volume on Dissolving Solids in Liquids." The author gives a table of the results obtained with fifty-two bodies, of which known weights were dissolved in known volumes of liquid—generally water—at 15.55°, the increase of bulk being measured in c.c. In ten cases no increase was observed.

Mr. G. W. Kennedy reported on the hygroscopic power of glycerin, which is capable of diluting itself very considerably on prolonged exposure to the air.

There is an extensive and well selected summary of chemical discoveries up to the summer of 1879, many of the paragraphs being taken from the CHEMICAL NEWS. A very large proportion of the matter in this section has no special connection with pharmacy.

The account of Mr. H. S. Wellcome's visit to the Cinchona Forests of South America is exceedingly interesting. The same remark applies to the paper on the flora of California. The so-called poison-oak—which, by the way, is not an oak but a *Rhus* (*Diversiloba*)—calls for chemical research. "To some persons the touch of the leaf is poisonous, causing an irritating eruption of the skin: its effect is sometimes felt even by passing to the leeward of the bush on a windy day, or going through the smoke of a fire in which it is burning. Poison-oak is the cause of a vast deal of suffering in California. It has been estimated that there are in this State near three thousand persons constantly affected with the cutaneous disease caused by this dreaded scourge."

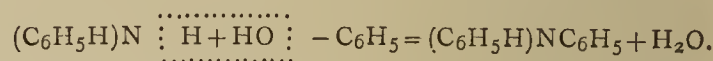
CORRESPONDENCE.

A NEW PHENOL REACTION.

To the Editor of the Chemical News.

SIR,—I beg to give you a short account of a new reaction which has just been discovered and worked out by my friends Profs. V. Merz and W. Weith, in conjunction with their demonstrator, Dr. Invalta. Since, on the other hand, this reaction is evidently of very great importance, both in a scientific and a practical way, and since, on the other hand, the discoverers will only publish it for themselves when they have worked it out in more detail, this preliminary account of it will interest many of your readers. It is unnecessary to say that I write with the full permission and knowledge of Profs. Merz and Weith.

These chemists had found previously (*Berl. Ber.*, xii., 1925), that phenol on heating with zinc chloride is transformed into phenyl ether, and they had reserved for themselves the right of instituting similar experiments for the purpose of obtaining other aromatic substances. There suggested itself first, an attempt to replace the hydrogen, not only of phenols but also of amines, or of ammonia itself by the "remainders" of phenols, thus:—

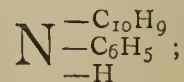


This supposition was entirely borne out by the following experiments.

A mixture of phenol and aniline-zinc chloride on heating produces abundant quantities of *diphenylamine*. This is probably the cheapest and most convenient way of preparing this compound as no pressure is required. Aniline-zinc chloride by itself furnished only traces of the secondary amine.

When phenol is treated with ammoniac-zinc chloride, aniline is produced, with a little diphenylamine, in very sensible quantity, but not so easily as in the first described case.

The smoothest and most abundant reaction takes place between β -naphthol and aniline-zinc chloride. The compound formed is *phenyl-beta-naphthylamine*,



it crystallises in fine white needles, fusing at 108° C. similarly as with diphenylamine, its basic character is not very decided, so that the chlorhydric salt,—



is already decomposed by cold water.

This reaction opens out a field for very many applications; it will probably lead to tertiary amines, and no

doubt also to many compounds interesting in a practical point of view.—I am, &c.,

GEORGE LUNGE.

Zürich, June 18, 1880.

CELLULOID.

To the Editor of the Chemical News.

SIR,—My attention has been drawn to an article on celluloid. Not having previously seen any notice of this chemical product, and being anxious to learn its nature, I shall feel obliged by your informing me regarding its composition and the process of manufacturing it.—I am, &c.

W. DONALD.

IRISH BOG BUTTER.

To the Editor of the Chemical News.

SIR,—In the CHEMICAL NEWS, vol. xli., p. 205, is a report of a paper by Mr. John Plant, F.R.S., on "Bog Butter," in which he throws a doubt over its occurrence in kegs. At the same time, he states, he was "informed that specimens of veritable kegs of bog butter are to be seen in the Museum of the Royal Irish Academy and in the Museums at Edinburgh,"—statements which he does not appear to have attempted to verify. Lately I came across some back numbers of the *Athenæum*, and in one of September 18, giving a report of the British Association Meeting of 1852, I found a paper on "Irish Bog Butter" was read by Mr. J. A. Brazier, who states that the substance is found in Ireland and also in Scotland, and is "usually in small kegs," which he describes as of the "most primitive form of construction and about one foot in height and ten inches in diameter." The details of Mr. Brazier's research are not given.—I am, &c.,

W. A. CARIUS.

Leominster, Herefordshire.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 21, May 24, 1880.

Refrigerating Mixtures formed by an Acid and a Hydrated Salt.—M. Berthelot.—Refrigerating mixtures formed of hydrated salts along with acids, bases, or other salts are governed by the following theory:—The abnormal phenomenon manifested by these mixtures results from the struggle of the chemical energies with other energies. The chemical energies act in conformity with the principle of maximum work to determine an initial endothermic reaction of which all the others are the consequence. The thermic energies then intervene in an inverse direction to determine an absorption of heat in the fourfold form of dissociation (hydrated sodium sulphate), of disaggregation by the solvent (equilibrium between sodium bisulphate and water), solution (which plays merely an intermediate part in the case of sodium sulphate and concentrated hydrochloric acid), and liquefaction (water of crystallisation).

Action of Acids upon the Alloys of Rhodium with Lead and Zinc.—H. Debray.—Lead cannot be alloyed either with iridium or ruthenium. At high temperatures the two latter dissolve in lead, but crystallise out on cooling, and if the mass is dissolved in dilute nitric acid they are obtained in the form of a brilliant crystalline powder, insoluble in all acids and even in *aqua regia*.

Platinum and rhodium combine with lead, giving out heat and light and forming crystalline alloys. When the lead is in large excess a very fusible alloy is obtained easily attacked by dilute nitric acid, which leaves a residue of platinum or rhodium combined with a certain quantity of lead. In case of platinum this residue is a true alloy, containing 11 per cent of lead and easily soluble in *aqua regia*. In case of rhodium after treatment with dilute nitric acid there remains P_6R_2 in small brilliant crystals, and a blackish matter lighter than the alloy and containing lead, rhodium, oxygen, and nitrogen. This compound is not a basic nitrite of rhodium and lead, but is more analogous to the nitriles of organic chemistry, though no known fact shows the possibility of hyponitrous acid being substituted for rhodium or lead.

Industrial Application of Solar Heat.—A. Mouchot.—The author has performed by means of solar heat a number of chemical operations on a practical scale. He enumerates the calcination of alum, the preparation of benzoic acid, the distillation of sulphuric acid, the production of wood charcoal in closed vessels, &c.

Combinations of Alcohols with Baryta and Lime: Products of the Decomposition of these Compounds by Heat.—A. Destrem.—Absolute alcohol combines readily with baryta and lime if brought in contact with them in closed vessels at 150° to 175° . The compound of ethylic alcohol and baryta gives about 300° ethylene, and hydrogen in about equal volumes. With amyl alcohol the process is similar, amylene being obtained in place of ethylene. The decomposition of the lime compounds is different. The carbide, C_nH_{2n} , is not liberated, and the products are hydrogen and a light liquid not yet examined. Anhydrous glycerin solidifies on being heated to 50° with baryta or quicklime. The lime compound on decomposition by heat yields water, hydrogen, carbonic acid, and a compound of carbon, hydrogen, and oxygen.

Reactions of Ammoniacal Salts with Calcium Carbonate.—M. Nivet.—In soils and in waters there takes place a double decomposition, the result of which is a loss of ammonia, the greater the lower is the absorptive power of the soil.

MISCELLANEOUS.

Tables of Spirit Gravities.—A complete series of tables of Spirit Gravities, by Dr. Thos. Stevenson, is preparing for publication by Mr. Van Voorst. This work will doubtless be appreciated by those engaged in analysis of alcoholic liquids.

Society of Arts.—The Council of this Society have awarded medals to the following gentlemen for papers read during the session which is just over:—Major-General H. Y. D. Scott, C.B., F.R.S., for his paper on "Suggestions for Dealing with the Sewage of London;" A. J. Ellis, F.R.S., for his paper on "The History of Musical Pitch;" John Spink, for his paper on "Recent Advances in the Production of Lambeth Art Pottery;" Henry B. Wheatley, F.S.A., for his paper on "The History and Art of Bookbinding;" W. Holman Hunt, for his paper on "The Present System of Obtaining Materials in use by Artist Painters, as compared with that of the Old Masters;" Thomas Fletcher, for his paper on "Recent Improvements in Gas Furnaces for Domestic and Laboratory Purposes;" John C. Morton, for his paper on "The Last Forty Years of Agricultural Experience;" Prof. Heaton, F.C.S., for his paper on "Balmains Luminous Paint;" Captain Abney, R.E., F.R.S., for his paper on "Recent Advances in the Science of Photography."

TO CORRESPONDENTS.

J. Schwartz.—Caustic soda is now made at most large alkali works.

THE JOURNAL OF SCIENCE

for June (Price 1s. 6d.), includes—

Insanity and its Difficulties.

The History of Antozone and Peroxide of Hydrogen. By Albert R. Leeds, Ph.D.

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INDEX.

- A**BSORPTION - SPECTRA, relation between the molecular structure of carbon compounds and their, 270
- Acetate, action of sodium on phenylic, 222
and sub-acetate of lead, determination of acetic acid in the, 110
- Acetic acid and potassic acetate, action of potassic bichromate upon, 36
determination of in the acetate and sub-acetate of lead, 110
- Acetyl, determination of by means of magnesia, 101
- Acetyl-urea and its derivatives, 197
- Acetylen-dicarbonic acids, 122
- Acid, alleged non-existence of pentathionic, 209
bioxy-fumaric, 172
boracic as a preservative, 152
carbolic, reddening of, 269
crystalline. oxalic, 219
cymen-carbonic, 36
formation-heat of persulphuric, 159
gases, injury to vegetation by, 111
lapacic, 36
new method of preparing phenyl-glycolic, 36
para-nitro-phenyl-acetic, 35
preparation and derivatives of xyllylic, 80
quinic, studies on, 208
rubin, 37
studies on persulphuric, 133
sulphuric, manufacture of, 16, 43, 47, 52, 67, 78, 263
vapours, noxious action of on vegetation, 14
- Acids, acetylen dicarbonic, 122
acidimetry of coloured, 110
action of hydro-ferro- and ferricyanic acids upon amines, 123
certain mono-chloro-crotonic, 173
free mineral in vinegar, detected, 110
maleic and malic, from bibrom-propionic acid, 252
- Ackroyd, W., the human eye as an automatic photometer, 179
- Actinism of the sun's rays, measurement of the, 211
- Ader, M., certain new experiments on magnetic attractions, 241
- Ador, E., and F. Meier, preparation and derivatives of xyllylic acid, 80
- Æthan and æthylen, halogen derivatives of, 122
- "Agenda du Chemiste" (review), 250
- "Agriculture, Chemistry of" (review), 250
- Air, action of upon peaty water, 245
analyses of, 97
influence of in the reduction of the ores of zinc, 220
- Albumen n urine, quantitative and qualitative determination of, 122
putrefaction-products of, 252
- Alcohol, certain properties of mixtures of methyl-cyanide with common, 218
cymen of cuminic 36
oxidation of by the ammoniacal binoxide of copper, 47
proportion of water in, 36
radicles, amido-acids with, 172
regulations concerning the sale of duty free for industrial purposes, 273
tables, 57, 70, 71, 100
- Alcoholic fermentation, rapid, 140
liquids, coagulation-point of, 241
- Alcohols, combination of with baryta and lime, 295
in unfermented fruit juices, 25
- Alexandrowicz, W., note on the actual state of the determination of zinc, 279
- Alizarin, iso-purpurin, and flavo-purpurin in mixtures, recognition of, 256
- Alkalies in æthusa cynapium, 277
- Alkalimetric determination of sulphates, 114, 158
- Alkaline carbonates, platinum attacked by melting, 25
dicarbonates examination of, 160
hypobromites and hypochlorites, determination of urea by means of, 161
tri-sulpho-carbonates, decomposition of in the animal system, 122
- Alkaloids, the, 255
artificial, 159, 220
notes on the, 63
of belladonna, datura, jusquiama, and duboisia, natural and mycristic, 241
- Allen, A. H., alcohol tables, 70, 100
artificial production of precious stones, 68
presence of nitrogen in iron, 231
- Aluminium and magnesium, gases retained by occlusion in, 273
atomic weight and valence of, 212
of, and occluded hydrogen, 285
chloride and bromide, reactions taking place in presence of, 251
wires for the telegraph, 134
- Amagat, E. H., change of form in glass tubes exposed to high pressures, 241
- "American Pharmaceutical Association" (review), 294
- American tantalates, analyses of some, 244
- Amines, action of hydro-ferro- and ferri-cyanic acids upon, 123
- Amido-acids with alcohol radicles, 172
- Ammonia, action of mercury on solution of sodium in, 133
- Ammonia and Leblanc soda, 102
behaviour of phenanthren-quinon with, 203
for atmospheric nitrogen, 208, 218, 229, 240, 259
from the nitrogen of the atmosphere and the hydrogen of water, 155, 195, 272
keeping salts of, 185
in plants, existence of, 241
preparation of water free from, 160
- Ammoniacal salts, reaction of with calcium carbonate, 295
- Ammonium carbonate, 251
citrate, reversion of phosphates soluble in, 173
nitrite, 252
- Ampère's currents in soft iron, application of the pre-existence of, 58
- Amphigen, reproduction of, 134
- Amplifying small motions, 292
- Anæsthetic, new, 154
- Analyses of air, 97
of some American tantalates, 244
of the ash of eucalyptus wood, 248
- Analysis, blowpipe, 133, 158
of children's food, 101
of milk, 230
of phosphates containing silica and fluorine, 101
of plants, proximate, 256
of water, 272
of wine and must, 159
of women's milk, 102
- "Analysis, Water, for Sanitary Purposes" (review), 206
- Analysts, Society of Public, 46
- Analytical chemistry, electric current in, 213
- Anethol derivatives, composition of the, 251
- Animal charcoal in the sugar manufacture, suppression of, 220
life, influence of manufacturing refuse on, 161
tar, carbons obtained from, 220
- Animals, influence of fattening upon the constitution of the fats formed in their tissues, 283
- Anschütz, R., fumaric and maleic ether, 172
- Ansdeil, G., physical constants of liquid hydrochloric acid, 75
- Anthraxens, use of the spectro-scope in discriminating, 52, 95, 117
- Anthraquinon, constitution of, 92
- Antimony, atomic weight of, 197, 201
- Apparatus for object lessons, lectures to schoolmistresses on, 130
- Aqueous dimethylamin, reactions produced by upon metallic solutions of, 173
- Arable soils, potash contained in the clay of, 70
- Armstrong, H. E., action of iodine upon oil of turpentine, 34
note on camphydrene, 290
- Aromatic aldehyds, synthesis of, 172
- Arsenic, chemical cause of the poisonous character of, 122
detection and estimation of, 177
in water colours, 110
new method of determining, 109
volatilising point of metallic, 189
- Arsenical and antimonial compounds solution, 110
vapours, contamination of atmospheric air with, 3
- Arseniferous bismuth with nitric acid, behaviour of, 23
- Artificial alkaloids, 220
- Arts, Society of, 59, 87, 134, 225
- Atkinson, R. W., action of the new diastase, eurotin, on starch, 169
- Atmosphere, ammonia from the nitrogen of, 272
- Atmospheric air with arsenical vapours, contamination of, 3
nitrogen, ammonia from, 208, 218, 229, 240, 259
oxidation of phosphorus, formation of ozone by the, 76
- Atomic weight and valence of aluminium, 212
of antimony, 197, 201
weights, reciprocal relations between, of the elements and their properties, 283
- Atomistic periodicity, history of, 203
- Auerbach, Dr., influence of manufacturing refuse on animal life, 161
G., azo and naphthol colouring-matters, 283
- Auric chloride, reduction of, 13
- Automatic mercurial pump, 279
- Ayrton and Perry, Profs., theory of terrestrial magnetism, 91
- Azo and naphthol colouring-matters, 283
- Azo-benzene, action of zinc-ethyl on, 270
- Azo-colours, some new, 117
- Azo-compounds, certain, 219
- B**ACTERIA of putrefaction, chemical composition of, 23
- Baeyer, A., and O. R. Jackson, synthesis of the homologues of hydrocarbostryl and chinolin, 251
of methyl-ketol, 252
- Balling, A. M., separation of gold by quartation with zinc, 173
C., determination of silver in galena, 42
- Balmann's luminous paint, 81, 135
- Balsohn, M., synthesis of ethylbenzin, by means of ether and benzin, 160
and C. Friedel, transformation of bromised styrolen into methyl benzoyl, 160

- Barbieri, J., and E. Schulze, decomposition of albumen in the sprouts of pumpkins, 23
- Bardy, M., methylated products and the preparation of pure methyl alcohol, 220
- Barium binocide, active oxygen in, 215
- Barker, G. F., and H. A. Rowland, efficiency of Edison's electric light, 200
- Barley, W., phoneidoscope, 292
- Barnes, J., error in vapour-density determinations, 260
- Barometer vacuum, on a simple method of determining vapour-densities in, 271
- Barth, L., and M. Kretschy, researches on picrotoxin, 229
- Base, a new, 219
- Baswitz, M., diastase, 35
- Battery, application of the Tommasi, 58
- Baudrimont, E., action of potassium permanganate upon potassium cyanide, 48
- Baudrowski, E., acetylen-dicarbonic acids, 122
- Bayle, O., and R. Poutigga, new process for bleaching vegetable fibres, 263
- Bechi, G., certain constituents of tar, 80
- Bees, disease among, 132
- Beet-root, propagation between the sugar and the mineral, and nitrogenous substances, 219
- Bell, C. A., on a simple method of determining vapour-densities in the barometer vacuum, 271
- J. C., unfermented wine case, 70
- Belladonna, datura, jusquiama, and duboisia, natural and mydriatic alkaloids of, 241
- Benzene, artificial colouring-matters derived from, 225
- Benzines, coal, and petroleum, 263
- Benzo-nitrite, action of zinc-ethyl on, 270
- Benzoyl-chloride and benzo-trichloride, action on phenols, 284
- Berchellmann, G. A., erroneous test for kirshwasser, 230
- Berger, F., guanidins of orthotoluidine, 35
- Berkley, P. A., notes on magnesian limestone bricks, 5
- Bernhardt, W., alkalies in *Æthusa cynapium*, 277
- Bert, P., and M. d'Arsonville, on a microphonic apparatus receiving words at a distance, 193
- Berthelot, M., action of organic solvents upon sulphur and metallic sulphides, 23
- of oxygenated water upon silver oxide, 183
- certain compounds of the halogens, 240
- chemical stability of matter in sonorous vibrations, 171
- copper hydride, 47
- decomposition of oxygenated water, 159
- of potassium permanganate by oxygenated water, 195
- formation-heat of gaseous chloral hydrate, 171
- of persulphuric acid, 159
- heat of formation of chloral hydrate, 79
- of gaseous chloral hydrate, 47
- protochloride of copper, 11
- reciprocal displacements of the halogens, 250
- refrigerating mixtures formed by an acid and a hydrated salt, 295
- relation between the heat of solution and the heat of dilution in complex solvents, 10
- researches on so-called hydride of copper, 22
- saccharosis, 10
- silver tetroxide, 195
- Berthelot, M., stability of oxygenated water, 250
- studies on persulphuric acid, 133
- and M. Vieille, explosive properties of mercury fulminate, 261
- Bertrand, M. A., active oxygen in barium binocide, 215
- Betorcinol and some of its derivatives, 163, 175
- Binnendijk, Dr., poisonous properties of phenol, 24
- Binon, J., influence of air in the reduction of the ores of zinc, 220
- Binocide of copper, oxidation of alcohol by the ammoniacal, 47
- Binz, C., and H. Schulz, chemical cause of the poisonous character of arsenic, 122
- Bioxy-fumaric acid, 172
- Birmingham, endowment of research in, 131
- Philosophical Society, 131
- Bismuth, behaviour of arseniferous with nitric acid, 23
- cadmium, and copper, qualitative determination of, 263
- method for estimating volumetrically, 153
- Blanchard, M. Emile, report on the scientific researches of the members of the learned societies in the departments published in the year 1879, 284
- Bleunard, A., on legumin, 273
- products of the splitting-up of proteic bodies, 184
- Blowpipe analysis, 133, 158, 250
- combination, for glass-working, 253
- "Blowpipe Analysis, Alphabetical Manual" (review), 249
- Blue flame in coal-fires, 22, 47
- Böckmann, F., "On Explosives" (review), 194
- Bodies absolute expansion of liquid and solid, 35
- new and old views on the nascent state, 1
- Bøke, T. D., detection and estimation of arsenic, 177
- Bog butter from Ireland, 205
- Irish, 295
- Boiler protector, corrosion of metallic zinc when used as, 102
- Bolton, H. C., ancient methods of filtration, 54
- Bone-black, absorption of sugar by, 287
- Boric acid as a preservative, 152
- Borneol and camphor, camphen of, 252
- Boron and silicon fluorides, action of water on, 134
- Böttiger, E., decomposition of mesoxalic acid by hydrogen sulphide, 48
- Bouchardat, G., action of the hydracids upon isopren, 48
- Bourgeois, L., and A. Verneuil, artificial production of a crystalline iron arsenate, 173
- Bourgoin, E., electrolysis of malonic acid, 183
- Bourrières, M., application of the Tommasi battery, 58
- Boutigny, P. H., laws which govern matter in the spheroidal state, 273
- Braga, J. F., analyses of some hair-dyes, 278
- Braithwaite, W. and J., "Retrospect of Medicine" (review), 109
- Brasseur, A., the mean composition of the saliferous waters and of the various products of the salt works of St. Leopold, near Volterra, and the theory of the extraction of salt there practised, 283
- Brauner, B., action of silver cyanate upon isobutylic iodide, 36
- Bredt, J., and R. Fittig, nature of pyroterebic acid, 209
- Brenken, O., examination of mineral lubricating oils, 25
- Bricks, magnesian limestones, 5
- Brieger, L., skatol, 80
- Burg, Dr., antisepticity of copper, 284
- Brom-dimethyl-aniline, 35
- Bromine and iodine, detection and determination of chlorine in presence of, 280
- chlorine, and iodine, agreement of the oxygen-steric, 54, with the steric of, 263
- relative replaceability of in the mono-brom-benzyl bromides, 146
- vapour, density of at a yellow heat, 151
- Bromised styrolen, transformation of into methyl-benzoyl, 160
- Bronze, tungsten, 123
- Brown, H. T., and J. Heron, history of starch and its transformations, 22
- Brühl, J. W., chemical constitution of organic bodies, 208
- Bruylants, G., method of preparing hydrobromic and hydriodic acids, 80
- Buisine, A., and E. Duvalier, formation of tetra-methyl-ammonium nitrate, 241
- Burrs in wool, destruction of, 24
- Butter, detection of olco-margarin, 77
- two ancient samples of, 46
- Butyl-chloral hydrate, dissociation of, 273
- CADMIUM in zinc, separation and estimation of, 28
- Cahours, A., and A. Etard, new derivative of nicotin, 133
- Cailletet, L., compression of gaseous mixtures, 101
- O., detection of copper in olive oil, 160
- Calcium carbonate, reaction of ammoniacal salts with, 295
- fluoride, crystalline, 24
- oxalate in plants, signification of, 160
- Camphen of borneol and of camphor, 252
- Camphidrene, note on, 290
- Cane-sugar, detection of starch-sugar mixed with, 221
- Cantoni, G., constitution of liquids, 284
- Carey, E., proposed Society of Industrial Chemistry, 260
- Carbohydrates, 48
- Carbolic acid, reddening of, 269
- Carbon acids of pyridin, 220
- compounds, determination of nitrogen in, 223
- relation between their molecular structure of and their absorption-spectra, 270
- crystallisation of, 13
- crystallised, in abstract, 111
- estimation of in steel, 17, 152, 229, 250
- in soils, determination of, 290
- Carbons obtained from animal tar, 220
- Carbonate, ammonium, 251
- Carbonates, thermo-chemical researches on the, 80
- Carbonic acid, proportion of in the air, 282
- cymen, 36
- reduction of by phosphorus at common temperatures, 92
- oxide as an agent for the preservation of meat, 122
- in the products of the combustion of coal-gas, 146
- Carisbrooke Casle well-water, 10, 57
- Carius, W. A., Irish bog butter, 295
- Carnelly, T., and W. C. Williams, melting- and boiling-points of certain inorganic substances, 45
- Mendelejeff's periodic law, 48
- vapour-density of stannous chloride 35
- Casamajor, P., action of bone-black on solutions of pure sugar, 66
- detection of starch-sugar mixed with cane-sugar, 221
- Catalytic force and the nascent state, 208
- Cats, electricity of, 59
- Celluloid, 295
- Cements and lubricants, quantitative determination of fats and resins in, 36
- Cerium, volumetric determination of, 231
- Certes, A., glycogenesis in infusoria, 70
- "Ceylon Coffee, Soils, and Manures" (review), 217
- Chambrier, M., new electromagnet, 159
- Characin and palmellin, 22
- Charcoal, influence of the nature of the upon the electric light, 70
- Chemical affinity in terms of electromotive force, 108
- "Chemical Analysis, a Course of Qualitative, by the late W. G. Valentin" (review), 282
- Chemical calculations, use of slide rule for, 33
- cause of the poisonous character of arsenic, 122
- committee of the Industrial Society of Mulhouse, 11
- composition of the bones in the arthropathy of ataxic cases, 22
- constitution of organic bodies, 208
- curves, illustration of, 191
- element, periodic law of the, 2, 27, 39, 48, 49, 61, 71, 83, 93, 106, 113, 125
- equilibrium, 168
- Institute of the University of Strasburg, 208
- manufactures of the South of France, notes on, 273
- photometer, new, 122
- repulsion, 40
- Society, 44, 76, 98, 100, 120, 143, 167, 191, 222, 245, 269, 290
- of Newcastle, 5, 32, 132, 179
- Russian, 160, 173
- specimens, 274
- stability of matter in sonorous vibrations, 171
- "Chemical Medicine, Annals of" (review), 10, 20
- "Chemiste, Agenda du" (review), 217
- Chemistry, electric current in analytical, 213
- Institute of, 147, 156
- "Chemistry of Agriculture" (review), 250
- "Chemistry, Qualitative" (review), 19
- "Chemistry, Questions on" (review), 19
- "Chemists' and Druggists' Diary for 1880" (review), 21
- Chemists, proposed Society of, 243, 260
- Chevreul, E., physical functions of leaves, 48
- Childrens' food, analysis of, 101
- Chloral hydrate, combination-heat of, 159, 171
- dissociation-tension of, 70
- heat of formation of gaseous, 47, 79
- and tichromate of potash, oxidising value of, 21
- Chloride, auric, reduction of, 13
- platinopotassic, determination of potassium as, 11
- of zinc as a reagent for alkaloids, 110
- vapour-density of stannous, 35
- Chlorine, contribution to the knowledge of, 135
- density of at high temperatures, 92
- detection and determination of in presence of bromine and iodine, 280
- gas, volatility of platinum in,

- Chlorophyll, crystalline, 11
Cholic acid, oxidation of, 172
Chrome, sulphides and selenides of, 219
Chromium and tungsten in steel and in iron alloys, 31
determination of, 222
Ciamician, G. L., and H. Weidel, carbons obtained from animal tar, 220
Cinchona barks, 252
Coal and petroleum benzines, 263
condition in which sulphur exists in, 201
fires, blue flame in, 22, 47
Coal-gas, carbonic oxide in the products of the combustion of, 146
heating power of, 41
products of combustion of, 169
production of ozone during the combustion of, 98
Coal-tar industry, progress of for the last ten years, 283
Cobalt and manganese, determination of, 184
nickel, determination of, 15
researches on, 74
electrolytic determination of, 25
"Coffee, Soils, and Manures, Ceylon" (review), 217
Cohné, S., alcohol tables, 57, 78
Cole, G., new metallic compound, 87
Colours, non-poisonous, 282
Colson, M. A., reversion of phosphates soluble in ammonium citrate, 173
Comstock, W. J., analyses of some American tantalates, 244
Conche, E., photography of the solar spectrum, 195
Condensers, use of tempered glass for, 159
Conechy, E. G. M., volatilising-point of metallic arsenic, 89
Cooke, J. P., atomic weight of antimony, 201
Copper, antiseptic of, 284
bismuth, and cadmium, qualitative separation of, 263
detect on of in olive oil, 160
electrolytic, determination of, 25
hydride, 47, 58, 70
in silk, 284
protochloride of, 11
reducing agents in refining and obtaining copper alloys, 272
researches on the so-called hydride of, 22
"Cotton Goods, Sizing of" (review), 9
Cotton goods sizing, 34
Couttolenc, M. G., automatic mercurial pump, 279
Crafts, J. M., density of certain gases at high temperature, 134
density of chlorine at high temperatures, 92
and F. Meier, density of iodine at high temperatures, 195
Crookes, W., a fourth state of matter, 275
Crystalline chlorophyll, 11
oxalic acid, 219
Cucumis anguria, colouring matter of, 36
Cumol, synthesis of, 219
Cupric test-pellets for sugar, 63, 78, 91
Cuprous chloride, formation-heat of, 251
Curares, plants which serve as the basis of the different, 79
Cyanallyl, 80
Cyanogen compounds, formation of graphite by the decomposition of, 283
formation-heat of, 251
Cymen-carbonic acid, 36
Cymen of cumic alcohol, 36
Cymene, examination of terpenes for, 291
DALLICAN, M., quantitative determination of stearin and olein in tallow, 283
Danesi, L., action of potassic bichromate upon acetic acid and potassic acetate, 36
Danos, winter climate of, 293
D'Arsonval, M., new voltaic condenser, 84
D'Arsonville, M., and P. Bert, microphonic apparatus receiving words at a distance, 183
Davis, G. E., carbon in steel, 229
proposed Technical Society, 261
"Sizing and Mildew in Cotton Goods" (review), 9
testing of vitriol chamber exits, 69, 146, 188
De Backer, H., electric lighting, 220
Debray, H., action of acids on alloys of rhodium with lead and zinc, 295
De Fonvielle, W., dependence of two electro-magnetic gyroscopes submitted to the same induction circuit, 262
Defresne, T., researches on pytaline and diastase, 22
Degener, P., volumetric determination of phenol, 222
De Koninck, L., platinum attacked by melting alkaline carbonates, 25
Delachanal and Vincent, MM., certain properties of mixtures of methyl-cyanide with common alcohol and methylic alcohol, 218
Delf's H., behaviour of sulphuretted hydrogen with the salts of the heavy metals, 279
Delplace, G., comparison of the apparatus for the concentration of sulphuric acid according to the systems of Delplace and Prentice, 274
Delafontaine, MM., new metals from gadolinite and Samarskite, 72
De Lépinay, MM., accidental double refraction, 146
Demole, E., partial synthesis of milk-sugar, 48
De Negri, A. and E., colouring matter of cucumis anguria, 36
Denzel, J., halogen derivatives of æthen and æthylen, 122
Deprez, M., measurer of energy, 219
Des Cloizeaux, M., crystalline form of magnesium, 282
Destrem, A., combinations of alcohols with baryta and lime, 295
Deville, H. Ste.-Claire, motion produced by the diffusion of gases and liquids, 58
Diamond, artificial production of the, 4, 97, 106
in an ophtic rock from South Africa, 46
Dibrom-antbraquinons, 45
Diastase, 35
action of upon starch-paste, 92
and maltin, active matter in, 25
and pytaline, action of, 160
researches on, 22
Dimethyl-meta-toluydin, certain derivatives of, 35
Dimethyl-naphthyl-amin and naphtho-quinon, 209
Dimethyl-para-phenylen-diamin, 35
Dinitro-ethyllic acid, new method of preparing, 291
Dioxethylen-amines, methylated, 77
Dipara-tolyl-guanidine, action of nitric acid on, 290
Diphenyl-di-imido-naphthol, 251
Disease, propagation of, 110
Ditte, M. A., action of metallic nitrites upon mono-hydrated nitric acid, 23
refrigerating mixtures formed by an acid and a hydrated salt, 283
Dobbin, L., reactions of tertiary isobutylic iodide, 77
Dœbner, O., action of benzoyl chloride and benzo-trichloride upon phenols, 284
Donald, W., celluloid, 295
Donath, E., detecting and determining iodine in presence of bromine and chlorine, 110
determination of cobalt and nickel, 15
solution of arsenical and antimonial compounds, 110
Dott, D. B., notes on solubility, 165
Dor, H. H., phenomena of vision, 146
Doubrava, S., movement of plates between the electrodes of the Holtz machine, 185
and E. Mach, electric perforation of glass, 184
Dreschel, E., urea-palladium chloride, 23
Drewsen, J., new method of determining the value of zinc-powder, 161
Dublin waters, examination of some, 62
Dubrunfaut, M., active matter in maltin and diastase, 25
mechanical equivalent of fat in cattle being fattened, 134
Ducretet, M., tempered glass for condensers, 159
Dufour and Rouaix, MM., pallet for the assay of oils, 24
Dumas, M., gases retained by occlusion in aluminium and magnesium, 273
Dunnington, F. P., new form of instrument for determination of specific gravity, 154
Dupré, Dr., detection of foreign colouring matters in wine, 269
Duvillier, E., and Buisine, formation of tetra-methyl-ammonium nitrate, 241
Du Moncel, T., influence of the nature of the charcoal upon the electric light, 70
Dvorak, V., method for recognising the optical difference of transparent bodies, 230
Dwars, M., separation of quinine from strychnine, 160
EARTH, edible, from Japan, 187
Eder, J. M., new chemical photometer, 122, 252
reducing properties of potassic ferro-oxalate, 230
Edison's electric light, 182
horse-shoe lamps, electrical measurements of, 199
"Egasse" disinfectant, the, 25
Eichler, E., octyl derivatives, 36
Eisenberg, L. J., action of hydro-, ferro-, and ferri-cyanic acids upon amines,
Ekin, C., "Potable Water" (review), 280
water analysis, 272
Electric burner, new, 48
current in analytical chemistry, 213
discharges in rarefied gases, analysis of the luminous phenomena produced by, 195
experiment unexplained, 81
lamp, smoke of an, 32
light, Edison's, 182
efficiency of, 200
influence of the nature of the charcoal upon the, 70
lighting, 220
perforation of glass, 184
regulation of time in Paris, 195
sparks produced by discharges of condensers, thermic laws of, 194
thermic laws of, 70
Electrical Company, Universal, 252
measurements of Edison's horse-shoe lamps, 199
Electricity of steel, 81
of cats, 59
Electricity, source of the force exciting, 230
"Electricity Tommasi," 58
Electrodes of the Holtz machine, movement of the plates between the, 185
Electrolysis, action of upon terebenthen, 172
of malonic acid, 183
Electrolytic determination of cobalt, nickel, and copper, 25
of metals, 280
Electro-deposition of lead, 209
Electro-magnetic gyroscopes, dependence of two submitted to the same induction-circuit, 262
Electro-magnet, new, 159
"Electro-plating" (review), 19
Electro-motive force, chemical affinity in terms of, 108
Elements, chemical, periodic law of the, 2, 27, 39, 48, 49, 61, 71, 83, 93, 106, 113, 125
Elsässer, E., galvanic conductivity of metallic alloys, 184
Emmerich, R., infection with town sewage and water from manufactories, 184
Endemann, H., boracic acid as a preservative, 152
Energy, measurer of, 219
Engel, M., dissociation of butyl-chloral hydrate, 273
and M. Moitessier, dissociation-tension of chloral hydrate, 70
Engler, C., apparatus for testing petroleum, 283
solubility of the metals in petroleum, 284
Etard, A., and A. Cahours, new derivatives of nicotine, 133
Ether and benzin, synthesis of ethyl-benzin by means of, 160
Ethereal salts of phenyl-acetic acid, action of sodium on, 222
Ethyl-benzin, limited oxidation of, 160
synthesis of by means of ether and benzin, 160
Ericin, 146
Erzbach, W. M., luminous nature of phosphorus, 92
Eucalyptus wood, analyses of the ash of, 248
Euphorbiaceæ, gum of the, 25
Eupittonic acid and pittacal, 122
Evans, J. C., action of zinc-ethyl upon benzo-nitrile, 270
Exner, F., theory of inconstant galvanic elements, 227
"Explosives" (review), 194
Explosives, decomposition of certain in closed vessels, 273, 282
FAT, mechanical equivalent of in cattle being fattened, 134
Fats and resins in cements and lubricants, quantitative determination of, 36
Fauconnier, A., determination of urea by means of alkaline hypochlorites and hypobromites, 161
"Fermentation, Studies on" (review), 20
Fernet, E., analysis of the luminous phenomena produced by electric discharges in rarefied gases, 195
Ferric chloride, method of detecting free hydrochloric acid in a solution of, 196
Fileti, M., and A. Piccini, decomposition of hydrochlorate of ethylamin by heat, 36
Filtration, ancient methods of, 54
Fish, danger to from the liquid refuse of manufactures, 122
Fisher, E., hydrazin compounds, 23
Fittig, R., polymerised non-saturated acids, 34
and J. Bredt, nature of pyroterebic acid, 209
"Flashing," phenomenon of in assays of gold, 126, 131

- Fletcher, J., examination of some Dublin waters, 62
 Fleischer, M., valuation of reverted phosphates, 283
 Fluids, some effects of vibratory motion in, 108
 Formic acid, 185
 synthetic formation of, 219
 Foster, W., contamination of atmospheric air with arsenical vapours, 3
 Fourth state of matter, 275
 Fonvielle, W., electro-magnetic gyroscope, 251
 Francken, J., suppression of animal charcoal in the sugar manufacture, 220
 Frankland, E., action of organo-zinc compounds upon nitriles and their analogues, 270
 zinc-ethyl upon benzo-nitrile, 270
 and C. C. Graham, new method of preparing dinitro-ethyl acid, 291
 and H. K. Tompkins, action of zinc-ethyl on phenyl-acetonitril, 291
 and Miss L. Halcrow, action of air upon peaty water, 245
 "Water Analysis for Sanitary Purposes" (review), 206
 Franchimont, A. P. N., carbohydrates, 48
 French patent law, 262
 Fresenius, R., determination of sulphur in pyrites, 173
 Fresnel's interference phenomena, true theory of, 184
 Friedel, C., and M. Balsohn, transformation of bromised styrolen into methyl-benzoyl, 160
 Fringes, interference, new, 134
 Friiswell, R. J., artificial colouring matters derived from benzene, 225
 Fromme, C., changes in the specific gravity of steel, 70
 Frost of December, 1879, 99
 Fruit-juices, unfermented, alcohols in, 25
 Fumaric and maleic ether, 172
- GADOLINITE** and samarskite, new metals from, 72
 Gaiffe, A., Thomson's galvanometer, 63
 Galena, determination of silver in, 42
 Galvanic conductivity of metallic alloys, 184
 elements, theory of inconstant, 227
 Galvanometer, Thomson's, 63
 Gardenin, 252
 Gas companies v. tar distillers, 289
 purification of, 155
 tar, distillation of, 289
 Gaseous mixtures, compression of, 101
 Gases and liquids, motion produced by the diffusion of, 58
 at high temperatures, density of certain, 134
 retained by occlusion in aluminium and magnesium, 273
 solubility of solids in, 103, 133
 Gauier, A., crystalline chlorophyll, 11
 isomers of phloro-glucose, 262
 Gelose, 251, 273
 Gerber, N., and P. Radenhauen, analysis of children's food, 101
 analysis of milk, 230
 analysis of women's milk, 102
 Giacoso, P., new method of preparing phenol glycolic acid, 36
 Gibertini, Dr. D., reciprocal relations between the atomic weights of the elements and their properties, 283
 Gigli, T., santonin contaminated with strychnin, 283
- Gilbert, H., analysis of phosphates containing silica and fluorine, 101
 Gimmingham, C. H., combination blowpipe for glass working, 253
 Girdwood, G. P., transparency of gold, 133
 Glasgow Philosophical Society, 236
 "Glasgow Sewage" (review), 207
 Glassford, W. O., cupric test-pellets, 91
 Glass, soluble, reactions of, 146
 tubes exposed to high pressure, change of form in, 241
 use of tempered for condensers, 159
 weights, new, 110
 Glass-working, combination blowpipe for, 253
 Glycerin, determination of in wines, 273
 production of, 283
 Glycogenesis in infusoria, 70
 Goës, B., diphenyl-di-imidonapththol, 251
 Gold, cupelled, influence of superfusion on the molecular arrangement of, 266
 phenomena of "flashing" in assays of, 126, 131
 reduction of by hydrogen in presence of platinum, 116
 separation of by quartation with zinc, 173
 transparency of, 133
 Gouy, M., new interference fringes, 134
 theory of circular double refraction, 262
 Gorceix, M., martite of Brazil, 134
 Graebe, C., occurrence of para-leucanilin in the magenta melt, 146
 and W. Knecht, synthesis of phenyl-napththyl-carbazol, 146
 Graham, C. C., and E. Frankland, new method of preparing dinitro-ethyl acid, 291
 Grandhomme, Dr., "The Tar-Colour Works of Messrs. Meister, Lucius, and Brüning" (review), 216
 Grant, Mr., induction in telephonic circuits, 56
 Graphite, formation of by the decomposition of cyanogen compounds, 283
 Greene, W. H., new synthesis of saligenin, 58
 preparation of the iodine and bromine derivatives of benzoin, 58
 Gréhaut, M., carbonic oxide in the products of the combustion of coal-gas, 146
 Griess, P., amido-acids with alcohol radicles, 172
 Grimshaw, H., water of the Carisbrooke Castle well, 57
 Grossmann, J., alkalimetric determination of sulphates, 114, 183
 Groves, C. E., determination of nitrogen in carbon compounds, 223
 and J. Stenhouse, betorcinol and some of its derivatives, 168, 175
 gardenin, 252
 preliminary note on some of its derivatives, 286, 290
 Guanidins of orth-toluidine, 35
 Guehhard, A., coloured rings produced on the surface of mercury, 10
 new phoneidoscopic process by means of coloured rings, 48
 Guetta, G., reduction of nitrates during metallic substitutions, 196
 Gum, new reaction with, 273
 of the euphorbiaceæ, 25
 Gustavson, G., reactions taking place in presence of aluminium chloride and bromide, 251
- Gutzeit, M., presence of alcohols in unfermented fruit juices, 25
 Gyroscope, electro-magnetic, 251
 Gyroscopes submitted to the same induction-circuit, dependence of two magnetic, 262
- HAGENBACH**, E., the law of Stokes, 184
 Hager, H., reddening of carbolic acid, 269
 Hair-dyes, analyses of, 278
 Halcrow, Miss L., and E. Frankland, action of air upon peaty water, 245
 Halogen derivatives of æthan and æthylen, 122
 Halogens, certain compounds of the, 240
 reciprocal displacements of the, 250
 Hammerl, H., action of water upon silicon and boron fluorides, 134
 "Handels-Berichte von Gehe and Co. of Dresden" (review), 293
 Hannay, J. B., and J. Hogarth, solubility of solids in gases, 103
 artificial formation of the diamond, 106
 Hardtung, E., and J. Post, products of the sulphurisation of isomeric nitramido- and diamido-benzols, 219
 Harmann and Reimer, MM., vanillin, 24
 Hartley, Prof., on the relation between the molecular structure of carbon compounds and their absorption-spectra, 270
 W. N., on the examination of terpenes for cymene, 291
 Hautefeuille, P., new property of the vanadates, 218
 reproduction of amphigen, 134
 simultaneous reproduction of orthose and quartz, 219
 two new sodium silico-titanates, 241
 Heat, decomposition of the hydrochlorate of ethyl-amin by, 36
 of solution and the heat of dilution, relation between in complex solvents, 10
 Heaton, C. W., Balmains luminous paint, 135
 Hehner, O., "new" methods, 158
 Heighman, W., "Photographic Printers' Assistant" (review), 20
 Henry, L., dry distillation of sodium trichloro-acetate, 35
 Herman, W. D., volatilisation of solids in vacuo, 191
 Heron, J., and H. T. Brown, history of starch, 22
 Herzfeld, A., action of diastase upon starch paste, 92
 Herzig, J., and H. Weidel, compounds from animal tar, 122
 Hesse, O., cinchona barks, 252
 studies on quinic acid, quinine, and on connected bodies, 208
 Heumann, H., on ultramarine, 284
 K., ultramarine compounds, 23
 Hippuric acid in the urine of the herbivora, source of, 23
 Hodgkinson, W., action of sodium on ethereal salts of phenyl-acetic acid, 222
 "A Course of Qualitative Chemical Analysis," by the late W. G. Valentin (review), 282
 and W. H. Perkin, action of sodium of phenylic acetate, 222
 Hoffmann, R., ammonia and Leblanc soda, 102
 Hofmann, A. W., further particulars on pittacal and eupittonic acid, 122
 Hogarth, J., and J. B. Hannay, solubility of solids in gases, 103
- Hollway, J., dephosphorisation of iron, 228, 237
 Hoogewerff, S., and W. A. van Dorp, carbon acids of pyridin, 220
 Houzé, A., new process for the manufacture of sulphuric acid, 263
 Houzeau, A., proportion of iron in the mineral waters of Rouen, 262
 Hughes, J., "Ceylon Coffee Soils and Manures" (review), 217
 Hungary, wine manufacture in, 110
 Hutchinson, C. C., separation and estimation of cadmium in zinc, 28
 Hydracids, action of upon isopren, 48
 Hydrate, dissociation-tension of chloral, 70
 heat of formation of chloral, 79
 Hydrazin compounds, 23
 Hydride, copper, 47, 58, 70
 of copper, researches on the so-called, 22
 Hydriodic and hydrobromic acids, preparation of, 80
 Hydrocarbons, distillation of, 209
 Hydrocarbo-styryl and chinolin, synthesis of the homologues of, 251
 Hydrochlorate of ethyl-amin, decomposition of the by heat, 36
 Hydrochloric acid, detection of, 176
 in a solution of ferric chloride, method of detecting free, 196
 physical constants of liquid, 75
 Hydro-ferro- and ferri-cyanic acids, action of upon amines, 123
 Hydrogen, allotropic conditions of, 80
 and ozone, peroxide of, 163, 182
 apparatus, sublimetted, 243
 lines, new, 116
 nascent, 176, 208, 284
 occluded, and atomic weight of aluminium, 285
 of water, ammonia from the nitrogen of the atmosphere and the, 155, 195
 reduction of metallic oxides by, 92
 sulphide, decomposition of mesoxalic acid by, 48
 sulphuretted, behaviour with the salts of the heavy metals, 279
 Hydrometers, 271
- I GASURINE**, 77
- Iles, M., qualitative separation of copper, bismuth, and cadmium, 263
 Ilimow, S. P., qualitative and quantitative determination of albumen in urine, 122
 Induction balance, theory of the, 57
 coil, effects produced by an, 94
 Inorganic substances, melting and boiling-points of certain, 45
 Institute of Chemistry, 147, 156
 Institution, Royal, 41, 121, 123, 235
 Interference phenomena in rays of long path, 292
 Iodide, reactions of tertiary isobutylic, 77
 Iodine and bromine derivatives of benzil, preparation of the, 58
 detection and determination of chlorine in presence of, 280
 at elevated temperatures, behaviour of, 151
 at high temperatures, density of, 195
 in presence of bromine and chlorine, detection and determination of, 110
 upon oil of turpentine, action of, 34

- Ireland, bog butter from, 205
 Irish bog butter, 295
 Iron alloys, chromium and tungsten in steel and in, 31
 and steel, nitrogen in, 231
 application of the pre-existence of Ampère's currents in soft, 58
 arseniate, artificial production of a crystalline, 173
 dephosphorisation of, 228, 237
 ores, &c., separation of silica in limestones, 234
 passivity of, 262
 proportion of in the mineral waters of Rouen, 262
 Isobutyl-aldehyd, action of potassium carbonate upon, 34
 Isobutyl iodide, reactions of, 77
 Isobutyl-iodide, action of silver cyanate upon, 36
 Isomeric dibrom-toluols, six possible, 192
 nitramido-and diamido-benzols, products of the sulphurisation of, 219
 Isomers of phloro-glucine, 262
 Isono, T., "On Shoyu" (review), 282
 Isopren, action of hydracids upon, 48
 Isopurpurin, flavo-purpurin, and alizarin in mixtures, recognition of, 256
- JACKSON, C. L.**, relative replaceability of bromine in the mono-brom-benzyl bromides, 146
O. R., and **A. Baeyer**, synthesis of the homologues of hydrocarbo-styryl and chinolin, 251
 of methyl-ketol, 252
Jacobsen, E., "Acid Rubin," 37
Japp, F. R., action of organozinc compounds on quinones, 170
Jay, M., influence of sugars on the determination of urea in urine, 161
Jean, F., sophistication of commercial silicate of soda, 251
Jones, C. H., polysulphides of sodium, 170
F., "Questions on Chemistry" (review), 19
H. C., on a black substance produced from sulphur, 244
H. E., purification of gas, 155
Jorissen, A., chloride of zinc as a reagent for alkaloids, 110
Joulin, L., researches on diffusion, 218
Jüptner, H. v., detection of mercury, 273
Jutsum, S. C., estimation of carbon in iron and steel, 17
- KACHLER, J.**, and **F. V. Spitzer**, camphen of borneol and of camphor, 252
Kahlbaum, G. W. A., certain mono-chloro-crotonic acids, 173
Kayser, H., influence of temperature upon tuning-forks, 184
Kekulé, A., and **H. Schröter**, propyl-bromide into iso-propyl-bromide, 172
Kern, S., some remarks on Siemens Martin steel, 278
 steel plates, 173
Kessler, F., alleged non-existence of pentathionic acid, 209
 atomic weight of antimony, 157
Kestner, S., digestive ferment produced during panification, 159
Kingzett, C. T., formation of ozone by the atmospheric oxidation of phosphorus, 76
 "Nature's Hygiene: a Series of Essays on Popular Scientific Subjects, with Special Reference to the Chemistry of the Eucalyptus and the Pine" (review), 293
- Kingzett, C. T.**, organic matter in water, 254
 peroxide of hydrogen and ozone, 182
Kirshwasser, erroneous test for, 230
Kjeldahl, M., action of diastase and pytaline, 160
Knecht, W., and **C. Graebe**, synthesis of phenyl-naphthyl-carbazol, 146
Koch, A., sulphuretted colouring-matter from para-phenylen-diamin, 81
König, J., quantitative determination of oxygen dissolved in water, 215
Kosmann, Dr., corrosion of metallic zinc when used as boiler protector, 102
Kretschy, M., and **L. Barth**, researches on picrotoxin, 229
Kuhara, M., method for estimating volumetrically, 153
Kutscheroff, M., oxidation of cholic acid, 172
- LADENBURG, A.**, artificial alkaloids, 159, 220
 natural and mydriatic alkaloids of belladonna, datura, jusquiama, 241
 on the tropeines, 251
Ladureau, M., utilisation of sewage, 81
Lalieu, M., examination of alkaline dicarbonates, 160
Landolph, F., composition of the anethol derivatives, 251
Langbeck, H. W., cupric test-pellets for sugar, 78
Lapacic acid, 36
Lead, action of concentrated sulphuric acid on, 236
 bath, convenient form of, 224
 Leaves, physical functions of, 48
Leblanc, R., destruction of burrs in wool, 24
Leeds, A. R., ammonium nitrite, 252
 lines of discovery in the history of ozone, 138
 nitrous acid in drinking-water, &c., 42
 peroxide of hydrogen and ozone, 163
 reduction of carbonic acid by phosphorus at common temperatures, 92
 solubility of ozone in water, 35
Lefevre, M., megascope and magic lantern, 284
Lefort, J., earthy tritungstates, 24
 use of Smithson's battery for the detection of mercury, 79
Legumin, 273
Letellier, A., oxidation of alcohol by the ammoniacal binoxide of copper, 47
 "Letts and Co.'s Diaries for 1880" (review), 21
Leuckart, R., acetyl-urea and certain derivatives, 197
Levitzki, M., reducing agents in refining copper and obtaining copper alloys, 272
Levulosate of lime, 92
Lewin, L., decomposition of alkaline tri-sulpho-carbonates in the animal system, 122
 Library bindings, deterioration of, 64
Liebmann, A., synthesis of cumol, 219
Light, action of and the function of chlorophyll in plants, 114, 122
Lime, an extremely sensitive reagent for, 102
 levulosate of, 92
Lippmann, E. O. v., presence of vanillin in crude sugars, 284
 Liquids and gases, motion produced by the diffusion of, 58
 constitution of, 284
 new method for taking the specific gravity of, 203
Locht, "pantelephone" of, 220
- Lockyer, J. N.**, new method of spectrum observation, 84
Lodge, O. J., intermittent currents and the theory of the induction balance, 57
Loew, O., source of hippuric acid in the urine of the herbivora, 23
Louis, D. A., on the action of organo-zinc compounds upon nitriles and their analogues, 270
H., sulphur in pyrites, 250
Lovett, W. J., testing vitriol chamber exits, 70, 181
Love, E. G., edible earth from Japan, 187
Lubricants and cements, quantitative determination of fats and resins in, 36
Luminous tubes, new, 58
Lunge, G., action of acid vapours on vegetation, 14
 a new phenol reaction, 294
 composition of Weldon mud, 78, 129, 141, 179, 181
 determination of sulphur in pyrites, 184
 nitrous compounds in sulphuric acid, 78
Luckow, C., electric current in analytical chemistry, 213
- MACAGNO, H.**, analyses of air, 97
 tannic acid of Sumach leaves, 63
Mach, E., and **S. Doubrava**, electric perforation of glass, 184
MacIvor, R. W., "Chemistry of Agriculture" (review), 250
Maclaur, J., action of concentrated sulphuric acid on lead, 236
 manufacture of sulphuric acid, 16, 43, 52, 66
Magenta melt, occurrence of para-leucanilin in the, 146
Magic lantern and megascope, 284
Magnesia, determination of acetyl by means of, 101
Magnesian limestone bricks, 5
Magnesium and aluminium, gases retained by occlusion in, 273
 crystalline form of, 282
Magnetic attractions, new experiments on, 241
 needles, mutual action of plunged into liquids, 282
Maladies, virulent, 121
Maleic and malic acids from bi-brom-propionic acid, 252
 and fumaric ether, 172
Mallet, J. W., atomic weight and valence of aluminium, 212
 note on the bearing on the atomic weight of aluminium of the fact that the metal occludes hydrogen, 285
Malonic acid, electrolysis of, 183
Maltin and diastase, active matter in, 25
Manchester Literary and Philosophical Society, 204, 248
Manganese and cobalt, determination of, 184
 some higher oxides of, and their hydrates, 291
 volumetric estimation of, 207
Marchetti, C., certain derivatives of the naphthols, 101
Marignac, C., earths of samarskite, 250
Marriott, W., the frost of December, 1879, 99
Martite of Brazil, 134
Maskelyne, N. S., artificial production of the diamond, 97
 supposed artificial production of the diamond, 4
Matter, chemical, stability of in sonorous vibrations, 171
 fourth state of, 275
 laws which govern, in the spheroidal state, 273
Maumené, E. J., ammonium carbonate, 351
 composition of slate, 160
- Maxwell, T.**, para-nitro-phenyl-acetic acid, 35
Mayer, A. M., **B. F. Thomas**, and **H. Morton**, electrical measurements of Edison's horse-shoe lamps, 199
Mazzara, G., meta-amido-cinnamic acid, 36
 phenol tolylate, 36
Meat, carbonic oxide as an agent for the preservation of, 122
Mechanical equivalent of fat in cattle being fattened, 134
 "Medicine, Annals of Chemical" (review), 10, 20
 "Medicine, retrospect of" (review), 109
Megascopie and magic lantern, 284
Meir, F., and **E. Ador**, preparation and derivatives of xylylic acid, 80
 and **J. M. Crafts**, density of iodine at high temperatures, 195
Melting-points of minerals and metals, 279
Mendeleeff, D., periodic law of the chemical elements, 2, 27, 39, 49, 61, 71, 83, 93, 106, 113, 125
Menzies, J. W., oxidising value of chlorate and bichromate of potash, 21
Mercurial pump, automatic, 279
Mercury, action of on solution of sodium in ammonia, 133
 coloured rings produced on the surface of, 10
 detection of, 79, 273
 fulminate, explosive properties of, 261
Merz, V., and **J. Tibirica**, synthetic formation of formic acid, 219
 and **G. Zetter**, some derivatives of resorcin and orcin, 80
Mesoxalic acid, decomposition of by hydrogen sulphide, 48
Meta-amido-cinnamic acid, 36
Meta-chlor-salicylic acid, products obtained by nitrising, 219
Metallic alloys, galvanic conductivity of, 184
 arsenic, volatilising-point of, 189
 compound, new, 87, 158
 nitrates, action of upon monohydrated nitric acid, 23
 oxides, reduction of by hydrogen, 92
 zinc, corrosion of when used as boiler protector, 102
Metallurgical Chair at the Royal School of Mines, 235
Metals in contact, measurement of the difference of the potential of two, 262
 noble, action of ozone on certain, 54
 solubility of in petroleum, 284
 the electrolytic determination of, 280
Meteorite which fell at Estherville, 262
Meteorological optics, 284
Society, 99, 292
Methylated dioxethylen-amines, 77
 products and the preparation of pure methylic alcohol, 220
Methyl-chloride, properties, preparation, and uses of, 281
Methyl-cyanide with common alcohol, certain properties of mixtures of, 218
Methyl-ketol, synthesis of, 252
Methyl-morphine iodide, action of potassium ferricyanide upon, 220
Methylic alcohol, preparation of pure, 220
Metropolitan Water, composition and quality of, 42, 111
Meyer, L., history of atomistic periodicity, 203
V., behaviour of iodine at elevated temperatures, 151

- Meyer, V., and H. Züblin, contribution to the knowledge of chlorine, 135
density of bromine vapour at a yellow heat, 151
Michler, W., and F. Salathé, behaviour of sulpho-chlorides with amines, 35
Microphonic apparatus receiving words at a distance, 183
"Mildew and Sizing in Cotton Goods" (review), 9
Milk, analysis of, 230
women's, 102
Milk-sugar, partial synthesis of, 48
Milot, A., reversion of the phosphates in the superphosphates used in agriculture, 160
synthesis of ulmic bodies, 184
Mills, E. J., chemical repulsion, 340
lecture illustration of chemical curves, 191
river water, 260
Mineral acids, detection of in vinegar, 110
lubricating oils, examination of, 25
waters of Rouen, proportion of iron in, 262
Mineralogical Society, 271
Mines, Royal School of, 220, 229, 235
Moissan, H., sulphides and selenides of chrome, 219
Moitessier, M., dissociation of butyl chloral hydrate, 273
and R. Engell, dissociation-tension of chloral hydrate, 70
Molecular weights and specific gravities of substances, 120
Mondésir, P. de, the tensions of saturated vapours have different methods of variation according as they are given off above or below the point of fusion, 212
Monnet, P., F. Reverdin, and E. Noelting, dimethyl-naphthylamin and naphtho-quinon, 209
Mono- and diethylen-diphenyl-diamin, action of nitrous acid upon, 35
Mono-brom benzyl-bromides, relative replaceability of bromine in the, 146
Mono-chloro-crotonic acids, certain, 173
Mono-hydrated nitric acid, action of metallic nitrates upon, 23
Montigny, M., meteorological optics, 284
Morin, H., gelose, 251
Morley, H. F., action of nitrous acid upon mono- and diethylen-diphenyl-diamin, 35
methylated dioxethylen-amines, 77
Morton, H., A. M. Mayer, and B. F. Thomas, electrical measurements of Edison's horse-shoe lamps, 199
Edison's electric light, 182
Mott, H. A., absorption of sugar by bone-black, 287
Mouchot, A., industrial application of solar heat, 295
Mud, composition of Weldon, 78, 129, 141, 179, 181, 219
Muir, M. M. P., chemical equilibrium, 168
essential oil of sage, 223
Mulhouse Industrial Society, 11, 209
Mulvay, J., ozone in nature, its relations, sources, and influences, 292
Muntz and Schläesing, M. M., researches on nitrification, 22
Must and wine, analysis of, 159
- NAPHTHALINS**, and their Derivatives, Constitution of" (review), 193
Naphthols and azo colouring matters, 283
Naphthols, certain derivatives of the, 101
Naphtho-quinon and dimethylnaphthylamin, 209
Nascent hydrogen, 284
state, catalytic force and the, 268
"Nature's Hygiene" (review), 193
Mencki, M., and F. Schaffer, chemical composition of the bacteria of putrefaction, 23
Nevile, R. H. C., and A. Winther, six possible isomeric dibromtoluols, 192
Newcastle Chemical Society, 5, 32, 132, 179
"New" methods, 158, 183
Nichols, W. R., deterioration of library bindings, 64
Nickel and cobalt, determination of, 15
researches on, 74
electrolytic determination of, 25
Nickels, B., use of the spectro-scope in discriminating anthracenes, 52, 95, 117
Nicotine, new derivatives of, 133
Nietner, H., and K. Zimmermann, carbonic oxide as an agent for preserving meat, 122
Nietzki, Dr. R., retrospect of the progress of the coal-tar industry for the last ten years, 283
toluylen diamin, 123
Nitrates, reduction of during metallic substitutions, 196
Nitric acid, action of on diparatolyl-guanidin, 290
Nitrification, 22
Nitrite, ammonium, 252
Nitrogen, determination of by the Will and Varrentrapp process, 108
determination of in carbon compounds, 223
in iron and steel, 231
of the atmosphere, ammonia from the, and the hydrogen of water, 155, 195
researches on the oxides and acids of, 80
sulphur, &c., detection of in organic bodies, 101
Nitrogenous substances in beet-root, proportion between the sugar and the mineral and, 219
Nitrous acid, action of upon mono- and diethylen-diphenyl-diamin, 35
in drinking-water, &c., 42
Nivet, M., reactions of ammoniacal salts with calcium carbonate, 295
Noelting, E., and F. Reverdin, "Constitutions of the Naphthalins and their Derivatives" (review), 193
P. Monnet, and F. Reverdin, dimethyl-naphthylamin and naphtho-quinon, 209
Non-poisonous colours, 282
Norwegium and vesbium, 116
- OBALSKI**, M., mutual action of magnetic needles plunged into liquids, 282
Octyl derivatives, 36
Ogier, M. J., new hydrate of silicium, 1
Ohl, W., electrolytic determination of cobalt, nickel, and copper, 25
Oil of sage, essential, 223
palm, 146
Turkey-red, 263
Oils, pallet for the assay of, 24
Olein and stearin, quantitative determination of in tallow, 283
Oleo-margarin, detection of in butter, 77
Oligiste from lava of Vesuvius, artificial production of, 36
Olive oil, detection of copper in, 160
Opal and alabaster glass, manufacture of, 160
- Optical difference of transparent bodies, method for recognising the, 230
Optics meteorological, 284
Organic bodies, chemical constitution of, 208
containing nitrogen, analysis of, 191
matter in rivers, oxidation of, 246
in water, detection of, 2, 30, 254
solvents, action of upon sulphur and metallic sulphides, 23
Organo-zinc compounds, action of on quinons, 170
upon nitriles and their analogues, 270
Orcin, derivatives of resorcin and, 80
Orcinol derivatives, note on, 286, 290
Ortho-para-toluidin, derivatives of, 251
Ortho-toluidin guanidins, 35
Orthose and quartz, simultaneous reproduction of, 219
Oxalate in plants, signification of calcium, 160
Oxalic acid, crystalline, 219
Oxides and acids of nitrogen, researches on, 80
Oxidising value of chlorate and bichromate of potash, 21
Oxygen dissolved in water, quantitative determination of, 215
Oxygen in barium binoxide, active, 215
Oxygen-stere, 54, with the stere of bromine, chlorine, and agreement of, 263
Oxygenated water, action of upon silver oxide, 183
decomposition of, 159
stability of, 250
Ozone, action of on certain noble metals, 54
formation of by the atmospheric oxidation of phosphorus, 76
in nature, 292
in water, solubility of, 35
lines of discovery in the history of, 138
peroxide of hydrogen and, 163, 182
production of during the combustion of coal-gas, 98
- PAINT**, luminous, 81, 135
Pallas, M., electricity of cats, 59
Palmelline and characine, 22
preservation of solutions of, 216
Palm oil, 146
Panification, digestive ferment produced during, 159
"Pantephone" of Loch, 220
Papasogli, G., researches on cobalt and nickel, 74
Para- and ortho-toluidin, derivatives of, 251
Para-leucanilin, occurrence of in the magenta melt, 146
Para-nitro-phenyl-acetic acid, 35
Para-phenylen-diamin, sulphuretted colouring-matter from, 81
Para-toluidin, on some products of the oxidation of, 269
"Paris, sewage of" (review), 171, 193
Parsons, H. B., proximate analysis of plants, 256, 267
Pasteur, L., "Studies on Fermentation" (review), 20
virulent maladies, 121
Patent Bill, a new, 285
Paterno, E., and P. Spica, cymen of cumic alcohol, 36
lapacic acid, 36
Patrouillard, C., determination of the, 262
Pawel, O., Roussin's salt, 48
Payne, A., new metallic compound, 158
Peaty water, action of air upon, 245
Pechmann, H., constitution of anthraquinon, 12
- Peirce, G. K., and E. F. Smith, products obtained by nitrising meta-chlor-salicylic acid, 219
Peligot, E., levulose of lime, 92
on saccharine, 282
Pellet, H., measurement of the difference of the potential of two metals in contact, 262
existence of ammonia in plants, 241
fixity of the composition of plants: analysis of Soya hispida, 283
proportion between the sugar and the mineral and nitrogenous substances in beet-root, 219
Pellizzari, G., analytical observations on tin, 196
Penke, W. A., and R. Warington, on the determination of carbon in soils, 290
Pentathionic acid, 290
alleged non-existence of, 209
Perkin, A. G., on the action of nitric acid on dipara-toluylguanidine, 290
W. H., analysis of organic bodies containing nitrogen, 191
dibrom-anthraquinons, 45
on some products of the oxidation of para-toluidin, 269
and W. Hodgkinson, action of sodium on phenylic acetate, 222
Peroxide of hydrogen and ozone, 163, 182
Perrey, A., potash contained in the clay of arable soils, 70
Perruche, M., new electric burner 48
Perry and Ayrton, Profs., theory of terrestrial magnetism, 91
Persulphuric acid, formation-heat of, 159
studies on, 133
Petroleum Act, 1879, the, 34
and coal benzines, 263
apparatus for testing, 283
report on, 134
solubility of metals in, 284
"Pharmacopœia, United States Revision of the" (review), 294
Pharmacy, South London School of, 10, 197
"Pharmacy, Year-book of" (review), 46
Phenanthren-quinon, behaviour of with ammonia, 203
Phenol glycolic acid, new method of preparing, 36
poisonous properties of, 24
reaction, new, 294
tolylate, 36
volumetric determination of, 222
Phenols, action of benzoyl-chloride and benzo-trichloride on, 284
Phenyl-aceto-nitrile, action of zinc-ethyl on, 291
Phenyl-betain, preparation of, 122
Phenylic acetate, action of sodium on, 222
Phenyl-naphthalin, synthesis of, 80
Phenyl-naphthyl-carbazol, synthesis of, 146
"Philadelphia College of Pharmacy, Sixteenth Annual Report" (review), 294
Philipp, J., and P. Schwebel, tungsten bronze, 123
Philosophical and Literary Society, Manchester, 204, 243
Society of Birmingham, 131
of Glasgow, 236
Phipson, T. L., catalytic force and the nascent state, 208
palmellin and characin, 22
preservation of solutions of palmellin, 216
reduction of auric chloride, 13
Phloro-glucine, isomers of, 262
Phoneioscopic process by means of coloured rings, new, 48
Phosphates, action of sulphuric acid upon 220

- analysis of, containing silica and fluorine, 101
redonda, 102
conversion of, in the superphosphates used in agriculture, 160
of soluble in ammonium citrate, 173
valuation of reverted, 283
Phosphorescence, phenomena of, 252
Phosphoric acid, blowpipe test for, 187
reverted, valuation of in manurial phosphates, 283
poisoning, variations of urea in cases of, 283
Phosphorus, luminous nature of, 92
products of the slow oxidation of, 73
question, 173
"Photographic Printers' Assistant" (review), 20
Photometer, new chemical, 252
the human eye as an automatic, 179
"Photometer, the Scale" (review), 193
Physical constants of liquid hydrochloric acid, 75
Physical Society, 56, 77, 91, 108, 131, 179, 204, 224, 258, 292
Piccini, A., and M. Fileti, decomposition of the hydrochlorate of ethylamin by heat, 36
Picrotoxin, researches on, 229
Pictet, R., considerations on the experimental possibility of the dissociation of certain non-metallic elements, 284
general equation giving the relation for all liquids between their temperature and the maximum tension of their vapours at this temperature, 273
Pilocarpine, 284
Pillitteri, A., cyanallyl, 80
Platin, L., compound platinate and a new platino-potassium salt, 118
Pittacal and eupittonic acid, 122
Planchud, M. E., formation of sulphuretted mineral waters, 236
Planchon, G., plants which serve as the basis of the different curares, 79
Plant, G., bog butter from Ireland, 205
Plants, fixity of the composition of: analysis of *Soya hispida*, 283
proximate analysis of, 256, 267
which serve as the basis of the different curares, 79
Platinates, compound, and a new platino-potassium salt, 118
Platinum attacked by melting alkaline carbonates, 25
reduction of gold by hydrogen in presence of, 116
volatility in chlorine gas, 81
Pöhl, A., pilocarpine, 24
Polstorff, K., action of potassium ferricyanide upon methylmorphine iodide, 220
Porumtaru, M., gelose, 273
Portland cement, 189
Post, Dr. J., composition and analysis of the Weldon mud, 276
J., action of sulphuric acid upon phosphates, 220
composition of Weldon mud, 219
and E. Hardtung, products of the sulphurisation of isomeric nitramide and diamido benzoils, 219
"Potable Water" (review), 283
Potash contained in the clay of arable soils, 70
Potassic bichromate, action of upon acetic acid and potassic acetate, 36
ferro oxalate, reducing properties of, 230
Potassium, determination of as platino-potassic chloride, 11
carbonate, action of upon isobutyl-aldehyde, 34
cyanide, action of potassium permanganate upon, 48
ferricyanide, action of upon methylmorphine iodide, 220
permanganate, action of upon potassium cyanide, 48
decomposition of by oxygenated water, 195
Polymerised non-saturated bodies, 34
Polysulphides of sodium, 170
Pontigga, R., and O. Bayle, new process for bleaching vegetable fibres, 263
"Practical Chemistry: the Principles of Qualitative Analysis" (review), 294
Precht, H., determination of potassium as platino-potassic chloride, 11
Prescott, A. B., "Qualitative Chemistry" (review), 19
Preusse, C., and F. Tiemann, detection of organic matter in water, 30
Prevost, J. H., amount of matter removed from the soil by rain, 44
"Price List of Dr. Theodor Schuchardt's Chemical Works, Goerlitz" (review), 282
Pringsheim, M., action of light and the function of chlorophyll in plants, 115, 122
"Printers' Photographic Assistant" (review), 20
Procter, H. R., use of the slide rule for chemical calculations, 33
Propyl-bromide, conversion of into isopropyl-bromide, 172
Proteic bodies, products of the splitting-up of, 184
Protchloride of copper, 11
Prud'homme, M., new violet colouring matter, 24
Pump, automatic mercurial, 279
Pumpkins, decomposition of albumen in the sprouts of, 23
Purple of the ancients, 291
Pyrites, determination of sulphur in, 173, 184
sulphur in, 250
Pyroterebic acid, nature of, 209
Pyralin and diastase, action of, 160
researches on, 22
"QUALITATIVE Chemical Analysis" (review), 282
"Qualitative Chemistry" (review), 19
Quartz and orthose, simultaneous reproduction of, 219
Quinic acid, 208
Quinine, 23
salts, 79
separation of from strychnine, 160
Quinon, quinic acid, and connected bodies, studies on, 208
Quinones, action of organo-zinc compounds on, 170
RADENHAUSEN, P., and N. Gerber, analysis of children's food, 101
analysis of milk, 230
women's milk, 102
Radiograph, the, 204, 292
Rain, amount of matter removed from the soil by, 44
Rammelsberg, C., vesbium and norwegium, 116
Raoult, F. M., coagulation-point of alcoholic liquids, 241
Raynaud, M., determination of glycerin in wines, 273
Reaume, N., method of detecting free hydrochloric acid in a solution of ferric chloride, 196
Recht, H., volumetric determination of sulphuric acid in sulphates, 24
"Redonda" phosphates, 58, 102
Redwood, B., the Petroleum Act, 1879, 34
Refraction, double, accidental, 146
theory of circular double, 262
Refrigerating mixtures, 295
formed by an acid and a hydrated salt, 283
Regnard, P., chemical composition of the bones in the arthropathy of ataxic cases, 22
Reichel, C., new reaction with gum, 273
Reimer and Harmann, MM., vanillin, 24
Reiset, J., researches on the proportion of carbonic acid in the air, 282
Rénard, A., action of electrolysis upon terebenthen, 172
Rennie, E. H., and C. R. A. Wright, action of benzoyl chloride on morphine, 291
"Report on the Revision of the United States Pharmacopœia" (review), 294
Resorcin and orcin, derivatives of, 80
Reverdin, F., E. Noeltling, and P. Monnet, dimethyl-naphthylamin and naphtho-quinon, 209
"Constitutions of the Naphthalins and of their Derivatives" (review), 193
Rhodium alloyed with lead and zinc, action of acids on, 295
Rice, C., "Report on the Revision of the United States Pharmacopœia" (review), 294
Ridout, Mr., amplifying small motions, 292
production of ozone during the combustion of coal-gas, 93
products of the slow oxidation of phosphorus, 73
some effects of vibratory motion in fluids, 103
Riedel, C., and C. Wurster, certain derivatives of dimethyl-meta-toluidin, 35
Righi, A., a case of permanent polarity in steel opposed to that of the magnetising coil by which it is produced, 192
Rivers, oxidation of organic matter in, 246
River water, 143, 260
Rocholl, R., separation of silica in limestones, iron ores, 234
Rœmer, H., and E. Schunck, recognition of alizarin, isopurpurin, and flavo-purpurin in mixtures, 256
Ross, W. A., "Alphabetical Manual of Blowpipe Analysis" (review), 249
blowpipe analysis, 158
test for phosphoric acid, 187
blue flame in coal fires, 22
Rossetti, M., temperature of the sun, 146
Rössler, C., determination of manganese and cobalt, 184
Rouaix and Dufour, MM., pallet for the assay of oils, 24
Roussin's salt, 48
Rowland, H. A., and G. F. Barker, efficiency of Edison's electric light, 200
Royal Institution, 41, 121, 123, 235
School of Mines, 220, 229, 235
Russell, W. J., Chemical Society, 100
Russian Chemical Society, 160, 173
SACCHARINE, 282
Saccharoses, remarks on, 10
Sage, essential oil of, 223
Sathé, F., and W. Michler, behaviour of sulpho-chlorides with amines, 35
Silicious waters, mean composition of, 283
Saligenin, new synthesis of, 58
Salkowski, E. and H., putrefaction products of albumen, 252
Salomon, F., acidimetry of coloured acids, 110
Salt, Boussin's, 48
Samarските, and gadolinite, new metals from, 72
earths of, 250
Santonin contaminated with strychnine, 283
Sarran, M., experimental researches on the decomposition of certain explosives: composition of the gases formed, 273, 282
"Scale Photometer, the" (review), 193
Schaffer, F., and M. Mencki, chemical composition of the bacteria of putrefaction, 23
Schaffer's glass reaction wheel, 59
Scheibe, A., and C. Wurster, brom-dimethyl-anilin, 35
Schertel, A., melting-points of minerals and metals, 279
Schicht, L., electrolytic determination of metals, 280
Schiff's memoir, reply to, 284
Schlœssing and Muntz, MM., researches on nitrification, 22
Schneider, R., behaviour of arseniferous bismuth with nitric acid, 23
Schöder, H., agreement of the oxygen-stere, 54, with the stere of chlorine, bromine, and iodine, 263
Schrötter, H., and A. Kekulé, conversion of propyl-bromide into iso-propyl-bromide, 172
Schuchardt, Dr. T., chemical specimens, 274
"Schuchardt's, Dr., Chemical Works, Price List of" (review), 282
Schulz, H., and C. Binz, chemical cause of the poisonous character of arsenic, 122
Schulze, E., and J. Barbieri, decomposition of albumen in the sprouts of pumpkins, 23
Schunck, E., notes on the purple of the ancients, 291
and H. Rœmer, recognition of alizarin, iso-purpurin, and flavo-purpurin in mixtures, 256
Schwebel, P., and J. Philipp, tungsten bronze, 123
Science, Yorkshire College of, 197
Sciences, the Academy of, 92
Scientific researches, report on of members of learned societies, 284
Seelheim, F., volatility of platinum in chlorine gas, 81
Selenides and sulphides of chrome, 219
Sendtner, R., and C. Wurster, dimethyl-para-phenylen-diamin, 35
"Sewage of Paris" (review), 171
193
Sewage purification, 284
town, infection with, 184
treatment of, 50, 73
utilisation of, 81
Shenstone, W. A., igasurine, 77
Shettle, Dr., influence of solar radiation on the earth's rotation, 292
"Shoyu" (review), 282
Siemens-Martin steel, remarks on, 278
Silica and fluorine, analyses of phosphates containing, 101
in limestone, iron ores, &c., separation of silica in, 234
Silicate of soda, sophistication of, 251
Silicium, new hydrate of, 1
Silicon and boron fluorides, action of water on, 134
Silico-titanates, sodium, two new, 241
Silver cyanate, action of upon isobutyl iodide, 36
determination of in galena, 42

- Silver tetroxide, 195
Sizing of cotton goods, 34
"Sizing of Cotton Goods" (review), 9
Skatol, 80
Skraup, Z. H., quinin, 23
Slate, composition of, 160
Smith A., treatment of sewage, 50
A. P., blue flame in coal-fires, 47
sulphuretted hydrogen apparatus, 243
E. F. A., new base, 219
and G. R. Peirce, products obtained by nitrising meta-chlor-salicylic acid, 219
J. L., meteorite which fell May 10, 1879, at Estherville, 262
report on petroleum, 134
R. A., measurement of the actinism of the sun's rays, 211
W., analyses of the ash of Eucalyptus wood, 248
convenient form of lead-bath, 224
synthesis of phenyl-naphthalin, 80
and T. Takamatsu, pentathionic acid, 290
Society of Arts, 59, 87, 134, 225, 295
of Industrial Chemists, proposed, 243, 260
Society of Public Analysts, 46
Soda ash, valuation of, 161, 173, 185
Leblanc, and ammonia, 102
sophistication of silicate of, 251
Sodium, action of on phenylic acetate, 222
silico-titanites, two new, 241
trichlor-acetate, dry distillation of, 35
"Soils and Manures, Coffee, Ceylon" (review), 217
Soils, determination of carbon in, 290
Solar heat, industrial application of, 295
radiation, influence on the earth's rotation, 292
spectrum, photography of the, 195
Solids in gases, solubility of, 103, 133
vacuo, volatilisation of, 191
Solubility, notes on, 165
Sommaruga, E., behaviour of phenanthren-quinon with ammonia, 203
Sommerkorn, H., new method for taking the specific gravity of liquids, 203
South London School of Pharmacy, 197
Soya hispida, analysis of, 283
Specific gravities and molecular weights of substances, 120
gravity, new instrument for determination of, 154
of liquids, new method for taking, 203
tables of, 295
Spectral analysis, contributions to quantitative, 230
Spectroscope, slit for, 79
use of the in discriminating anthracenes, 52, 95, 117
Spectroscopic notes, 172
Spectrum observation, new method of, 84
Spence's metal, 272
Spica, P., detection of nitrogen, sulphur, &c., in organic bodies, 101
and E. Paterno, cyment of cumic alcohol, 36
Spitzer, F. V., and J. Kachler, camphen of borneol and of camphor, 252
Spottiswoode, W. E., effects produced by an induction-coil, 94
Staats, G., derivatives of ortho- and para-toluidin, 251
Stannous chloride, vapour-density of, 35
Starch, history of, 22
Stearin and olein, quantitative determination of in tallow, 283
Stebbins, J. H., certain azo-compounds, 219
some new azo-colours, 117
Steel and electricity, 81
and iron, nitrogen in, 231
changes in the specific gravity of, 70
estimation of carbon in, 17, 152, 229, 250
opposed to that of the magnetising coil by which it is produced, a case of permanent polarity in, 192
plates, note on, 178
Siemens-Martin, remarks on, 278
Stenhouse, J., and C. E. Groves, betorcinol and some of its derivatives, 168, 175
on gardenin, 252
preliminary note on some orcinol derivatives, 286, 290
Stokes, the law of, 184
Stolba, F., crystalline calcium fluoride, 24
volumetric determination of cerium, 31
Stones, artificial production of precious, 68
Strasburg, Chemical Institute of the University of, 208
Strychnin, santonin contaminated with, 283
separation of quinin from, 160
Stürtz, B., phenomena of phosphorescence, 252
Sugar, absorption of by bone-black, 287
action of bone solutions of pure, 66
and the mineral and nitrogenous substances in beet-root, proportion between the, 219
cupric test-pellets for, 63, 78, 91
starch, mixed with cane-sugar, detection of, 221
Sugars, crude, presence of vanillin in, 284
Sulphates, alkalimetric, determination of, 114, 158
Sulphides and selenides of chrome, 219
Sulpho-chlorides, behaviour of with amines, 35
Sulphur and metallic sulphides, action of organic solvents upon, 23
an experiment on, 110
condition in which it exists in coal, 201
in pyrites, 250
determination of, 173, 184
nitrogen, &c., detection of in organic bodies, 101
on a black substance produced from, 244
Sulphuretted colouring-matter from para-phenylen-diamin, 81
hydrogen apparatus, 243
behaviour of with the salts of the heavy metals, 279
mineral waters, formation of, 236
Sulphuric acid, action of upon phosphates, 220
comparison of the apparatus for the concentration of according to the systems of Delplace and Prentice, 274
concentrated, action of on lead, 236
in sulphates, volumetric determination of, 24
manufacture of, 16, 43, 47, 52, 67, 78, 263
Sumach leaves, tannic acid of, 63
Sun's rays, measurement of the actinism of the, 211
Sun, temperature of the, 146
Superfusion, on the influence of on the molecular arrangement of cupelled gold, 266
Superphosphates used in agriculture, reversion of the phosphates in the, 160
Swan, Mr., smoke of an electric lamp, 32
Synthesis of aromatic aldehyds, 172
TAKAMATSU, T., and W. Smith, on pentathionic acid, 290
Tanatar, S., bioxy-fumaric acid, 172
maleic and malic acids from bi-brom-propionic acid, 252
Tannic acid of sumach leaves, 63
Tar, certain constituents of, 80
distillers v. gas companies, 289
"Tar-colour Works of Messrs. Meister, Lucius, and Brünig" (review), 216
Tattersall, T., notes on the alkaloïds, 63
Technical society, proposed, 243, 261
Technological pictures from western England, 109
Technology of the Mint, 234
Teed, F. L., on a simple method of determining vapour-densities in the barometer vacuum, 271
Telegraph, aluminium wires for the, 134
Telephonic circuits, induction in, 56
Temperatures, determination of high, 218
Terebenthin, action of electrolysis upon, 172
Teroxide, silver, 195
Terpenes, examination of for cymene, 291
Terrestrial magnetism, theory of, 91
Tetra-methyl-ammonium nitrate, formation of, 241
Thalheim, W., formation of graphite by the decomposition of cyanogen compounds, 283
Thein, determination of, 262
Thermic laws of electric sparks, 70, 195
Thermo-chemical researches, 80, 197
Thermograph, a new, 293
Thibaut M., variations of urea in cases of phosphoric poisoning, 283
Thomas, B. F., H. Morton, and A. M. Mayer, electrical measurements of Edison's horse-shoe lamp, 199
Thompson, J. B., ammonia from the nitrogen of the atmosphere, 240, 272
Thomsen, J., allotropic conditions of hydrogen, 80
formation-heat of cuprous chloride, 251
of cyanogen, 251
researches on the oxides and acids of nitrogen, 80
thermo-chemical researches on the carbonates, 80, 197
Thomson's galvanometer, 63
Thomson, W., "Sizing of Cotton Goods" (review), 9
Thorpe, Prof., molecular weights and specific gravities of substances, 120
Thudichum, J. L. W., "Annals of Chemical Medicine" (review), 10, 20
Tibirica, J., and V. Merz, synthetic formation of formic acid, 219
Tidy, C. M., composition and quality of the Metropolitan water, 42, 111
river water, 143
water analysis, 272
Tidy's paper on river water, observations on, 265
Tiemann, F., and C. Preusse, detection of organic matter in water, 30
Tilden, W. A., "Practical Chemistry: the Principles of Qualitative Analysis" (review), 294
Time in Paris, electric regulation of, 195
Tin, analytical observations on, 196
and lead in dilute acetic acid, solubility of alloys of, 109
Tôkiô, contributions from the laboratory of, 76
Toluylen-diamins, 123
Tolylate, phenol, 36
Tommasi battery, application of the, 58
Dr., nascent hydrogen, 176, 284
new and old views on the nascent state of bodies, 1
reduction of gold by hydrogen in the presence of platinum, 116
reply to Prof. Ugo Schiff's memoir on certain observations made in a former paper of the author's, entitled "Researches on the Constitutional Formulae of the ferric compounds, 284
Universal Electrical Company, 252
Tompkins, H. K., and E. Frankland, action of zinc-ethyl on phenyl-aceto-nitrile, 291
Toxicology, review of, 209
Tresca, M., electric regulation of time in Paris, 195
Trève, M., application of the pre-existence of Ampère's currents in soft iron, 58
new luminous tubes, 58
Trichinosis, 37
Trichlor-acetate, dry distillation of sodium, 35
Trisulpho-carbonates in the animal system, decomposition of alkaline, 122
Tritungstates, earthy, 24
Troost, L., and H. Ste.-Claire Deville, determination of high temperatures, 218
Tropéines, on the, 251
Tucker, A. E., slit for spectro-scope, 79
Tungsten and chromium in steel and in iron alloys, 31
bronze, 123
Tuning forks, influence of temperature upon, 184
Turkey-red oil, 263
Turpentine, action of iodine upon oil of, 34
ULBRICHT, R., analysis of wine and must, 159
Ulmic bodies, synthesis of, 184
Ultramarine, 284
compounds, 23
Urea, determination of by means of alkaline hypochlorites and hypobromites, 161
palladium chloride, 23
variations of in cases of phosphoric poisoning, 283
Urech, F., action of potassium carbonate upon isobutyl-aldehyd, 34
Urine, influence of sugars on the determination of urea in, 161
qualitative and quantitative determination of albumen in, 122
Urquhart, J. W., "Electro-plating" (review), 19
VALENTIN, the late W. G., "A Course of Qualitative Chemical Analysis" (review), 282
Vanadates, new property of the, 218
Van der Ploeg, B. J., significance of calcium oxalate in plants, 160
Van Dorp, W. A., and S. Horgeverff, carbon acids of pyridin, 220
Van Riemsdijk, Dr. A. D., on the influence of superfusion on the molecular arrangement of cupelled gold, 266
phenomenon of flashing in assays of gold, 126

- Vanillin, 24
presence of in crude sugars, 284
Vapour-densities, simple method
of determining in the baro-
meter vacuum, 271
Vapour-density determinations,
error in, 260
Vapours, saturated, the tensions
of, have different methods of
variation, according as they
are given off above or below
the point of fusion, 282
temperature of the decomposi-
tion of, 22
Varenne, L., passivity of iron, 262
Vasey, T. E., testing of vitriol-
chamber exits, 47, 91, 145
Vegetable fibres mixed with
wool, destruction of, 37
new process for bleaching, 263
Veley, V. H., on some higher
oxides of manganese and
their hydrates, 291
Verneuil, A., and L. Bourgeois,
artificial production of a crys-
talline iron arseniate, 173
Vernon, A., review of toxicology,
209
Vesbium and Norwegium, 116
Vesuvius, artificial production of
oligiste from the lava of, 36
Vieille, M., experimental re-
searches on the decomposi-
tion of certain explosives: com-
position of the gases
formed, 273, 282
and M. Berthelot, explosive
properties of mercury ful-
minate, 261
Villari, E., thermic laws of elec-
tric sparks, 70, 195
Villiers, A., crystalline oxalic
acid, 219
Vincent, C.M., reactions produced
by aqueous dimethylamin
upon metallic solutions, 173
and M. Delachanal, certain
properties of mixtures of
methyl-cyanide with common
alcohol and methylic alcohol,
218
Violet colouring matter, new, 24
Vision, phenomena of, 146
Vitriol chamber exits, testing of,
47, 69, 70, 91, 145, 146, 181, 188
Vogel, A., proportion of water in
alcohol, 36
H. W., new hydrogen lines, 116
spectroscopic notes, 172
Volta, A., action of ozone on
certain noble metals, 54
Voltaic condenser, 84
Von Morden, C., contributions to
quantitative spectral analy-
sis, 230
Vortmann, G., detection and
determination of chlorine in
presence of bromine and
iodine, 280
WAGNER, G., arsenic in water
colours, 110
Wallace, W., condition in which
sulphur exists in coal, 201
"Glasgow Sewage" (review),
207
heating power of coal-gas, 41
Warrington, R., observations upon
Dr. Tidy's paper on river
water, 265
and W. A. Peake, determina-
tion of carbon in soils, 290
Wartha, V., explosion during the
heating of wine, 262
Water, action of on silicon and
boron, 134
analysis, 272
"Water Analysis for Sanitary
Purposes" (review), 206
Water, detection of organic
matter in, 30
Metropolitan, composition and
quality of, 42, 111
of Carisbrooke Castle well, 10,
57
organic matter in, 254
oxygenated, stability of, 250
peaty, action of air upon, 245
preparation of free from ammo-
nia, 160
proportion of in alcohol, 36
quantitative determination of
oxygen dissolved in, 215
Water, river, 143, 260
observation on Dr. Tidy's
paper on, 265
solubility of ozone in, 35
Waters, Dublin, examination of
some, 62
mineral, formation of sulphu-
retted, 236
Watson, Mr., Portland cement,
189
Weber, F. H., theory of Fresnel's
interference phenomena, 184
Werdermann, Prof., phenomena
of interference in rays of long
path, 292
Weidel, H., and G. L. Ciamician,
carbons obtained from animal
tar, 220
and J. Herzig, compounds from
animal tar, 122
Weldon mud, composition of, 78,
129, 141, 179, 181, 219
and analysis of, 276
W., volumetric estimation of
manganese, 207
Westmoreland, J. W., estimation
of carbon in steel, 152, 250
Whewell, G., sizing of cotton
goods, 34
Whitlam, W. G., Spence's metal,
272
Wiebe, H. F., absolute expansion
of liquid and solid bodies, 35
Wigner, G. W., water of Caris-
brooke Castle well, 10
Will and Varrentrapp's process,
determination by the, 108
Williams, C. T., the winter cli-
mate of Danos, 292
W. C., and T. Carnelly,
melting- and boiling-points
of certain inorganic sub-
stances, 45
Wiley, H. W., detection of hydro-
chloric acid, 176
Wilm, T., determination of chro-
mium, 222
Wine and must, analysis of, 159
detection of foreign colouring
matters in, 269
explosion during the heating
of, 262
manufacture in Hungary, 110
unfermented, sale of, 56, 70
Wines, determination of glycerin
in, 273
Winstanley, D., the radiograph,
204, 292
Winther, A., and R. H. C. Nevile,
six possible isomeric dibrom-
toluols, 192
Wolff, C. H., new colorimeter, 109
Wood, W. H., action of mercury
on solution of sodium in am-
monia, 133
Wool, destruction of burrs in, 24
destruction of vegetable fibres
mixed with, 37
Wright, C. R. A., and E. H.
Rennie, action of benzoyl
chloride on morphine, 291
wright, C. W., chemical affinity
in terms of electromotive
force, 108
L. T., products of combustion of
coal-gas, 169
Wurster, C., and C. Riedel, cer-
tain derivatives of dimethyl-
meta-toluidin, 35
and R. Sendtner, dimethyl-para-
phenylen-diamin, 35
and A. Scheibe, brom-dimethyl-
anilin, 35
Wurtz, A., chloral hydrate, 79
combination-heat of chloral
hydrate, 159
hydride of copper, 58
temperature of the decomposi-
tion of vapours, 22
XYLYLIC acid, preparation
and derivatives of, 80
"YEAR-BOOK of Pharmacy
(review), 46
Yellow heat, density of bromine
vapour at a, 151
Yorkshire College of Science, 197
ZETTER, G., and V. Merz,
some derivatives of resorcin
and orcin, 80
Zimmermann, J., preparation of
phenyl-betain, 122
K., and H. Nietner, carbonic
oxide as an agent for the pre-
servation of meat, 122
Zinc-ethyl, action of on azo-
benzene, 270
action of upon benzo-nitrile, 270
upon phenyl-aceto-nitrile, 291
Zinc, note on the actual state of
the determination of, 279
ores of, influence of air in the
reduction of the, 220
Zinc-powder, new method of
determining the value of, 161
Zinc, separation and estimation
of cadmium in, 28
separation of gold by quartation
with, 173
Züblin, H., and V. Meyer, con-
tribution to the knowledge of
chlorine, 135
density of bromine vapour at
a yellow heat, 151



END OF VOLUME XLI.



